Does More Money Make You Fat? The Effects of Quasi-Experimental Income Transfers on Adolescent and Young Adult Obesity

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Abstract

Average body weights in the US vary significantly with socio-economic status. The direction of causality between income and body mass is not well understood: are rising incomes affecting body mass or do slimmer people make more money? We investigate the effect of exogenous income transfers during adolescence on contemporaneous body mass (BMI) measures and young adult obesity rates. The effects of extra income on children's health differ depending on the households' initial socio-economic status; individuals from initially wealthier households are less likely to experience an increase in BMI. Part of this effect is due to differential increases in height, as well as weight. Utilizing the unique panel structure of our data, we investigate whether the age at intervention or duration of treatment has the biggest effect on the adolescent's BMI, weight and height. We find that age at intervention is most important.

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I. Introduction

The global obesity epidemic is widely recognized as one of the most significant non-communicable disease threats to global public health in the next several decades (The Lancet, 2011). Leading public health experts around the world have called for coordinated government action to help turn the tide of growing body mass and the twin threats of cardiovascular disease and type two diabetes (Wang et al, 2011; The Lancet, 2011). There is significant concern that the rise in obesity world-wide will slow or even reverse the significant mortality reductions experienced by high income countries in the past several decades (Swinburn et al, 2011) and that obesity has become a bigger threat to public health than smoking.

Current trends are particularly alarming among children and adolescents. Globally in 2004, there were 170 million overweight (inclusive of obese) children (Lobstein et al, 2004). The US has experienced a drastic increase in the prevalence of childhood and adolescent obesity since the 1980s. According to the most recent National Health and Nutrition Examination Survey (2008) 11.9 percent of children aged 2-19 were at or above the 97th percentile of the BMI-for-age growth charts and 17 percent were at or above the 95th percentile (Ogden et al, 2010).

There are significant differences in the prevalence of obesity among socio-economic groups. The poor have been hit particularly hard by the obesity epidemic, both across and within countries. Cross-national studies have found that as GDP rises, the burden of obesity shifts from higher to lower socio-economic groups within counties, and from higher- to lower income countries globally (Monteiro et al, 2004; Mendez et al, 2005). But the direction of causality between income and body mass is unclear. Do higher earnings contribute to slimmer bodies or are slender people likely to earn more?[1] A clear understanding of the link between poverty and obesity is particularly pressing today, when the poverty rate in the US stands at its highest since 1993[2]. The identification problem is less severe if one considers children's outcomes, since children in the US do not work full-time and are dependent on their parents. But children's outcomes may be strongly correlated with those of other household members. For example, a number of correlational studies have shown that overweight mothers are more likely to have overweight children.[3]

The main contribution of this paper is to overcome the income-body mass endogeneity problem. We use quasi-experimental evidence from a government transfer program that exogenously increased incomes for one group of children while leaving the comparison group unaffected. We are also able to compare the outcomes of children across three age cohorts who were affected for different lengths of time. The variation in age at first intervention and duration of treatment allows us to disentangle the confounded effect of treatment duration from age at intervention. Finally, we show that the effects of government interventions on children's health are not unambiguous.

Theory predicts an inverted U-shape relationship between unearned income and weight (Lakdawalla and Philipson, 2009). The inverted U-shape results from a restriction in calories due to an income constraint at the very lowest levels of household socio-economic status (SES). As income increases, households and individuals increase their consumption of food and consequently we expect to see an increase in weight. Beyond a certain threshold, the wealthiest households are either able to purchase higher quality foods that are more nutritious or pursue

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[1] There is research suggesting that this may be the case. Hamermesh and Biddle (1994) find that attractive people tend to earn more than their plain counterparts. Schultz (2002) and Persico et al (2004) show that taller people gain higher wages.

[2] The Census Bureau, announcement from September 13, 2011

[3] E.g. Malrid et al, 2004, Danielzik et al, 2004, Nguyen et al, 1996

health-related activities, so the income-weight curve starts sloping downwards. To our knowledge, there is no experimental evidence testing this prediction. This study confirms the non-linearity of the relationship using exogenous changes in unearned income. We find evidence that extra unearned income increases BMI among youths from poorer households. At the same time, we find a significant reduction in obesity rates among children in wealthier households. The children who are treated to the government transfer program earliest experience the strongest effects.

Our findings suggest that the transfer increased BMI among adolescents from families with average incomes below $30,000, but not among their better off peers. Further investigation reveals that this is due to differential changes in weight and height among youths from different economic backgrounds. Adolescents from initially poorer households are more likely to increase their weight and less likely to experience increases in height as compared to adolescents from initially wealthier households. Consequently, children from the initially poorer households tend to increase their BMI over time. These results imply that growing up in a poor household has long-lasting effects on future health.

The government transfer is a per capita disbursement to adult members of an American Indian tribe; non-members do not receive these disbursements. The exogenous income transfers depend only tribal membership.[5] Our goal is to examine the effect of the transfer payment (derived from casino profits on the American Indian reservation) on BMI, weight and height at different points in the adolescent's development. Uniquely, this program provided casino

[5] Memership in the Eastern Cherokee tribe is determined by genealogical ties to existing tribal membership rolls from 1924. Additionally, the minimum blood quantum required is 1/16 for tribal membership; therefore, ethnically individual tribal members may be mixed race but they may still be politically tribally enrolled members. The enrollment requirements are for tribal citizenship, not ethnicity or race. Only tribally enrolled citizens are eligible for the casino transfer payments. We use Native American, American Indian, and tribal member interchangeably through the rest of the text.

transfer payments to the entire distribution of tribal member household types; both wealthy and poor households received the transfers.

The non-linearity in the effect of an exogenous change in household income on BMI, weight and height of household adolescents agrees with existing theory. Our results suggest that policies intended to improve overall health outcomes must account for differences in household characteristics. Additionally, our analysis indicates that the age at which an intervention occurs has a stronger effect on BMI, weight and height than the duration of treatment. These results accord with studies in empidemiology and public health that indicate that there are stages in child development during which health interventions can be particularly effective in improving future health outcomes (e.g. Schmeer, 2010).

A potential identification challenge could come from differential growth paths for adolescents born in tribal member households vs non-members. It is important to note that in our preferred specifications we control for such differences. Specifically, in the panel regressions to follow we include age-by-ethnicity fixed effects and linear (and non-linear) ethnicity-specific time trends. In further robustness checks, we explore the role of changes to parental labor force participation as a result of the opening of the casino and the potential exposure to casino-related restaurants. We do not find evidence that the observed results are being driven by these channels.[6]

The next section puts the present study in the context of the current literature on obesity. Section 3 describes the data and the empirical strategy. Next, we discuss the results and some of the potential mechanisms. In Section 5 we offer some robustness checks and comment on alternative hypotheses. Section 6 discusses the potential linkages between changes in household

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[6] We do not find an increase in employment in the entertainment, recreation, accommodations or food industries for either parent after the opening of the casino.

income and increasing obesity rates in the US. Finally, Section 7 offers some concluding remarks.

II. Background

When studying the determinants of childhood obesity in developed countries, economists have concentrated primarily on the effects of the supply and quality of food consumed by children. This research is highly relevant for public policy aimed at reducing adolescent obesity rates. For example, it has been shown that fast food restaurants close to school grounds increase the prevalence of obesity among 9th graders (Currie et al, 2009) and higher prices for fruit and vegetables in the neighborhood are associated with higher BMI, especially among economically disadvantaged children (Powell and Chaloupka, 2009). Increased supply of fast food or "bad" food potentially available to children contributes to higher incidence of childhood obesity.

However, studies investigating the effects of changing access to different types of food assume that the demand-side effects are negligible. In this study we ask the opposite question: holding the access and availability of foods constant, would higher household incomes cause changes in obesity rates among youths? Due to the panel nature of our data, we can control for unobserved area characteristics, such as the kinds of restaurants and supermarkets in a particular area that affect all children residing there in the same way.[7]

One way to assess the contribution of increased incomes on adolescents' BMI is to consider exogenous changes in the affordability of different types of food. Affordability can increase in two ways: by providing extra funds that can be spent on food only (such as food stamps and other coupons) and by changes in expendable income. Previous studies have found

[7] We include global positions system (GPS) coordinates for each American Indian and non-Indian household as well as an interaction variable with the casino-transfer payment. These results are reported in later appendix tables and the proximity to the casino or casino-related restaurants does not have an effect on our reported results.

mixed results on the effect of food stamps on adult obesity rates (Townsend et al, 2001, Chen et al, 2005; Kaushal, 2007). Two recent studies examine the causal effects of extra expendable income on BMI. Schmeiser (2008) considers low income women while Cawley et al (forthcoming) study Social Security recipients. Both utilize instrumental variable (IV) strategies to estimate changes in BMI and obesity rates attributable to changes in income. Our study differs from previous studies in that we focus on children and utilize a quasi-experimental framework. We are not aware of any previous economics research on the effects of exogenously increased household income on adolescents' BMI in the United States.[8]

Empirically, the relationship between income and obesity is hard to identify. Among studies using data on adult populations, the main problem is identifying the direction of causation – higher incomes make food more accessible, but obesity and the associated health problems make it harder to earn high incomes. People with higher incomes can afford better food, and they are also less likely to be obese.[9] There is a separate literature estimating the effect of BMI on earnings (Kline and Tobias, 2008; Cawley, 2004; Mocan and Tekin, 2009) and at least one study shows that overweight and obese adults are likely to suffer from low self-esteem which may be underlying their lower earnings (Mocan and Tekin, 2009). To plausibly capture the empirical relationship between income and weight, one has to exogenously increase the amount of dispensable income available to the household without affecting the extent of physical activity or physical attractiveness needed to earn that income.

Assessing the effect of exogenous income transfers on the BMI of children and adolescents is attractive for two reasons. First, the transfers we consider come from an exogenous

[8] In a study examining obesity rates for adults over thirty years, Chang et al (2005) find that there has been an increase at all levels. Their study differs from ours in that they are looking at an association between income and obesity (they do not have an exogenous change to income) and they are looking at adults only.

[9] Behrman and Deolalikar (1987) have shown that changes in income in a developing country are not necessarily associated with changes in food consumption – they find that it depends on the income elasticity of food.

source and their size is not affected by the initial financial situation of the household. Second, the exogenous income transfer affects children while they are teenagers - a time when most children earn little on their own.[10] The children in our study are subjected to the income effect, but unlikely to be affected by a substitution effect away from labor. In developing countries, the case would be quite different in that the additional household income would allow children to work less and enter school which may have separate effects on the child's BMI.[11]

This analysis shows that extra unearned income has a non-linear effect on children's BMI depending on the household's initial income level. Probing further into the components of BMI – weight and height - we find that extra income decreases weight and might additionally increase height in children coming from initially better-off households relative to their poorer peers[12]. For children coming from tribal member households with average household incomes we find no effects of extra income on height.

This paper confirms that the relationship between household income and adolescents' body mass is non-linear in initial income. Moreover, we find that children coming from households earning $40,000 or more experience no change in their BMI following the income transfer. Our results show that the turning point between increasing and decreasing BMI as a function of extra unearned income is somewhere around the $30,000 initial household income level.

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[10] Child labor laws and mandatory schooling requirements in the U.S. prevent children from working full time until age 18.

[11] See, for instance, the literature on child labor in developing countries. Edmonds (2008) provides a useful overview of the findings.

[12] There are several growth spurts in children's physical development, during which they gain significantly in height. For example boys in the US gain up to 10 cm/year at age 13, and up to 5 cm/year at ages 14-16 (see, e.g. Figure 1 in Case and Paxson, 2008). In our study, the youngest treated cohort were aged thirteen at the time that the income transfers were first received by the parents. On average, these children would have gained around 25 cm (girls) and 28 cm (boys) in height between their 13th and 20th year.

III. Data and empirical strategy

The Great Smoky Mountains Study of Youth (GSMS) is a longitudinal survey of 1420 children aged 9, 11 and 13 years at the survey intake that were recruited from 11 counties in western North Carolina. The children were selected from a population of approximately 20,000 school-aged children using an accelerated cohort design.[13] Children from the Eastern Band of Cherokee Indians were over sampled for this data collection effort. Survey weights are used in the child outcome regressions that follow. The federal reservation is situated in two of the 11 counties within the study. The initial survey contained 350 Indian children and 1070 non-Indian children. Proportional weights were assigned according to the probability of selection into the study; therefore, the data is representative of the school-aged population of children in this region. Attrition and non-response rates were found to be equal across ethnic and income groups.

The survey began in 1993 and has followed these three cohorts of children annually up to the age of 16 and then re-interviewed them at ages 19 and 21.[14] Both parents and children were interviewed separately up until the child was 16 years old; interviews after that were only conducted with the child alone.

After the fourth wave of the study, a casino was opened on the Eastern Cherokee reservation; the survey children were approximately 13, 15 and 17 years of age at that time. The casino is owned and operated by the tribal government. A portion of the profits are distributed on a per capita basis to all adult tribal members.[15] Disbursements are made every six months and

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[13] See Costello E. Jane, Adrian Angold, and Barbara Burns, and Dalene Stangl, and Dan L. Tweed, and Alaatin Erkanli, and Carol M. Worthman (1996) for a thorough description of the original survey methodology.

[14] Individuals are interviewed regardless of where they are living (whether on their own, in college, or still living with their parents). No child is dropped from the survey because they moved out of their parent's home. We find no statistically significant difference in attrition between the treatment and control groups or selective attrition on health outcomes. American Indians comprise 24% of the sample in the very first survey wave and comprise approximately 27% of the sample at age 21.

[15] All adult tribal members received these per capita disbursements. If there were any non-compliers (parents that either did not receive or refused the additional income) then any estimates found here would be an under estimate of

have occurred since 1996. The average annual amount per person has been approximately $4000. This income is subject to the federal income tax requirements. However, as the transfers are not part of earned income, they do not directly affect EITC for eligible individuals.

The outcome variables of interest are Body Mass Index (BMI), height, weight and obesity. The first three measures are recorded at each survey wave. Interviewers measured survey respondents using rulers and scales. Medically recommended levels of BMI are between 20 and 25 for adults. Individuals with BMI levels of 25-30 are considered overweight in adults; those with BMI greater than 30 are considered obese[16]. We have constructed a simple obesity index variable for our survey subjects (ages 19 and 21) which takes on the value of 1 when BMI is greater than 30 and is 0 otherwise. We utilize the Centers for Disease Control BMI-for-age chart for boys and girls. These measures account for differential growth rates between the genders at different ages. Adolescents are classified by age, gender, weight and height and assigned a percentile. Individuals that exceed the 95th percentile for their age and gender group are considered obese while individuals who are above the 85th percentile are classified as overweight (inclusive of the obese). We employ these designations in the tables that follow.

Table 1 provides descriptive statistics. The sample is balanced on conditions at intake such as age, sex, and maternal labor force participation between American Indians and the rest. American Indian mothers are significantly less likely to have been to college, and more likely to have completed high school. The incidence of obesity and being overweight is substantially

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the true effects of additional income. Children listed as tribal members were eligible for the casino disbursements themselves at age 18 if they completed high school; even if they did not complete high school they would receive the casino transfers at age 21. While they initially did not know exactly how much the transfers will amount to, tribal members had every reason to believe that this was a permanent positive change in their incomes. Casino operations are authorized under the National Indian Gaming Act of 1987 which authorized the development of economic activities related to gaming on US Federally recognized American Indian reservations. By the time the Eastern Cherokee tribal casino had begun operation, other tribal casinos had been operating in places such as Florida and the mid-west for almost a decade.

[16] In the analysis we drop several extreme outliers (which we attribute to either recording error or measurement error) for recorded BMI levels that exceed 100 or are below 10.

higher among American Indian youth. A large proportion of these adolescents are obese (36%) as compared to 19% of the rest of the sample. The difference comes from an eight kilogram difference in weight, while average height is very similar between the two groups. Tribal members come from poorer families – on average, their households received ten thousand dollars less in annual income in the three survey waves before the casino opened. Household income is provided in categories and a value of 6 corresponds to approximately $30,000 (the average for non-Indians) while a value of 4 corresponds to an annual income of approximately $20,000 (the average for American Indian households); these amounts correspond closely to data for the region from the 1990 US Census. The casino disbursements (approximately $4,000) represent more than a twenty percent increase in the average household income of parent couples of mixed heritage, and more than 40 percent increase in households of two American Indian parents. The casino transfers alone would be enough to close the income gap between an average family with two non-members parents and families composed to two tribal members.

In Figure 1 we show simple correlation coefficients between household income in the last survey wave before the initiation of the income transfers and children's body mass. The graph indicates that children who come from households with an initial income of between $10-20,000 will have on average a BMI that is 2 points higher than a child from a household with incomes of $60,000 or more (the omitted income category). A clear negative correlation between household income and children's BMI is apparent, at least up to annual income levels of around $40,000. Similarly to studies based on national data, we find that poverty and children's obesity are strongly related in this sample.

In Figures 2 and 3 we show a basic illustration of the changes in BMI attributable to the exogenous income transfers. In these figures we hold the age of all three cohorts constant at ages

13 and age 21, respectively.[17] Figure 2 indicates that American Indians tend to have higher BMI than non-Indians even at a relatively young age. By age 21, this effect becomes more pronounced with a proportionately higher increase in BMI for American Indians. The raw data here indicate that there is a differential increase in BMI for American Indians; however, these figures can not tell us whether that is due to the increase in household incomes or due to other unobserved differences across the groups. In the next section, we explore different regression models that allow us to control for these unobserved group characteristics.

Empirical Estimation

Difference-in-Difference Regressions

In our investigation of the effect of changes in household incomes on child obesity we first utilize a difference-in-difference regression strategy. We compare young adult outcomes for adolescents that resided for a total of six (four years for the middle age cohort) years as minors in households with increased incomes to adolescents who resided for two years as minors in households with exogenously increased incomes. This specification allows us to compare the effect of four additional years of higher household incomes on adolescent health outcomes. The two youngest age cohort variables (Age 9 and Age 11 at survey intake; ages 13 and 15 at first treatment) function as the "after-treatment" cases and the oldest age cohort (Age 13 at survey intake; age 17 at first treatment) functions as the "before-treatment" case. We focus explicitly on the effect of the per capita transfer on BMI and the prevalence of obesity at ages 19 and 21. Non-tribal members serve as the control group in this case. In the analysis to follow, we will

[17] For the youngest age cohort, we restrict analysis to age 12 as there were no observations at age 13 (survey wave 5) for this cohort. It is important to note that none of the three cohorts were treated with the increase in household income at these ages.

emphasize the individual panel aspect of our dataset. However, the difference-in-difference methodology is useful as a first step in our analysis of the effect of a change in household income on childhood obesity.

An examination of the effect of the treatment on household income indicates that almost all of the additional cash transfer shows up as additional household income in each survey wave.[19] The size of the exogenous increase in household incomes can take on two different values depending upon the number of American Indian parents in each household. It is possible for there to be 0, 1 or 2 American Indian parents in each household. Clearly households with two American Indian parents will have double the amount of exogenous income than households with only a single American Indian parent. We treat the number of parents as a continuous variable and we therefore have two interaction variables of interest. The equation below details the specification:

$$\begin{aligned} Y_i = \alpha + \beta_1 \times Age9_i + \beta_2 \times Age11_i + \delta_1 \times NumParents + \gamma_1 \times Age9 \times NumParents_i + \\ \gamma_2 \times Age11_i \times NumParents_i + X_i'\theta + \varepsilon_i \end{aligned} \quad (1)$$

In the equation above, Y is BMI or obese status for the survey children measured at ages 19 or 21. In the equation above, the Age9 and Age11 variables indicate whether or not the child is drawn from the initially age 9 or age 11 cohorts respectively -- the age 13 cohort is the omitted category in this regression. The variable NumParents indicates the number of parents that are tribal members that child's household. The two coefficients of interest are γ_1 and γ_2, which

[19] We find that the effect of the treatment (household eligibility for the casino per capita transfer) results in approximately $3900 additional household income at each survey wave. The average amount distributed per person has been about $4000 per year. This suggests that households do not alter their labor participation in response to this additional household income.

measure the effect of receiving the casino disbursements and being in either the age 9 or age 11 cohorts relative to the 13 year old cohort. The vector X controls household conditions prior to the opening of the casino and includes average household income over the four years, the sex of the child, the race of the child, mother's labor force participation and education level of the mother.

Identification of equation 1 relies on the fact that the different age cohorts of children were randomly sampled within American Indian and non-Indian groupings.[20] Additionally, in order for the difference-in-difference method to be valid it is important that the pre-intervention trends between the treatment and control groups move in a similar direction; these graphs are provided in Appendix Figures 1-3 for pre-treatment BMI, weight and height.

It is also important to note that there were no new health or educational programs that were created immediately after the advent of casino disbursements by the tribal government. This is important in establishing the fact that time variant characteristics that were related only to American Indians (such as tribally-funded health and nutrition programs) are not the causal factor here. In later years new programs have been developed, but for the crucial period in which these children were minors in their parents' households, there is little evidence of new programs. Anecdotal evidence suggests that the revenues from the casino operations were, at least in the short run, spent only on per capita disbursements to the tribally-enrolled membership. Spending on large scale construction was not initiated until 2001, when the youngest cohort was around 17 years old. Therefore, the children in this study were not minors when these new programs and facilities were operational and were not affected by them.

There is little evidence that the casino itself generated differential employment for tribal members relative to the non-Indian parents. Overall employment does not appear to change after

[20] See Akee et al (2010) for evidence of the comparability of respondents across age cohorts.

the casino opens for either group of parents. This is not surprising as the reservation and its residents are integrated into the regional labor market; similarly, many non-Indians work, and even reside, on the Eastern Cherokee reservation. Previous researchers have found that casino job growth on American Indian reservations was due primarily to non-Indian employment (Evans and Topoleski, 2002). Using data from the universe of tribes that opened a casino, Wolfe et al (2011) find that casino openings did not increase labor force participation among the affected tribes. The Eastern Cherokee reservation is relatively small; it is a little over 100 square miles and is less than an hour from Asheville, NC and less than two hours from Knoxville, TN.

Fixed-Effects Panel Regression

Given the panel nature of the data, we are able to examine the effect of a casino transfer payment on health outcomes at each survey wave for the adolescents in the survey. Because the panel data contains information on the same individuals at multiple points in time, we are able to include age-by-race fixed effects and Native American-specific time trends. We examine changes in the body mass index (BMI), as well as the weight and height. Therefore, we employ an individual fixed-effects regression for these three health outcome variables. We use all available data, covering each individual of the three cohorts from ages 9 (11 and 13 respectively) onwards, interviewed every year until age 16 and then again at ages 19 and 21. The regression is:

$$Y_{it} = \alpha_i + X_{it}'\beta + \varepsilon_{it} \quad (2)$$

In this regression, α_i is the individual fixed effect and X is the vector of control variables, including whether the individual child, i, belongs to a household that is eligible for casino payments. This indicator variable is always zero for households that have no members of the tribe; for households with adult members of the tribe the variable is zero for the first four survey waves and then takes the value of one thereafter. Identification of the casino effect is driven by differences between Native American treated and untreated children of the same age; this is possible because the Native American children in our panel data are treated to casino payments at different ages.

Because the extra income was received by tribal member parents only, a potential concern is that the estimates from (2) could be driven by differences in the growth path of height, weight, and BMI between children of American Indian heritage and the control group. We emphasize that X also includes a set of age by race fixed effects, to control for potentially different growth paths for Native American and white children. We also include a Native American-specific time trend. Taken together these two different types of race-cohort controls (time invariant and time variant) should account for any potential differences across the two groups. [24]

Duration of treatment vs age at intervention

Due to the panel nature of our data, we can disentangle the two different effects of the exogenous change to household income – the duration of treatment and age at intervention. Our fixed-effects analysis for the data is given below:

$$Y_{it} = \alpha_i + X_{it}'\beta + \delta D_{it} + \gamma_1 \times Cohort1_i \times Age_{it} + \gamma_2 \times Cohort2_i \times Age_{it} + \varepsilon_{it} \quad (3)$$

[24] In results not reported here, we include a squared Native American-specific time trend and find no differences in our results.

In the model above we include a variable, D, which measures the length of time (duration) that an adolescent is exposed to the casino transfer payment; non-Indians always are coded zero for this variable. This variable is non-negative as we restrict our analysis to survey waves after the first year of the casino operation. We also measure the effect of being treated at ages 13 and 15 (coefficients γ_1 and γ_2 respectively) on the health outcomes relative to the oldest cohort of children (age 17 at the time of intervention). These coefficients estimate the effect of holding current age constant (and duration of treatment, D, constant as well) and allowing for the age at intervention to differ (between ages 13 and 17 for the youngest and oldest age cohorts represented by γ_1 and between ages 15 and 17 for the middle and oldest age cohorts represented by γ_2). We then compare the magnitude and statistical significance of the coefficients δ, γ_1 and γ_2 to gauge the relative significance of spending one more year under treatment versus being treated at a different age.

IV. Results

We first present results from the difference-in-differences models (equation 1) comparing outcomes between adolescents of different cohorts and tribal membership at ages 19 and 21. These results provided very suggestive evidence for the effect of changes in household income on child obesity. We then explore in greater detail this effect by using the panel data (equation 2) which provides information on the effects of additional household income at each survey wave. Finally, we examine the differences in the age at intervention versus the duration of treatment effects (equation 3) for the Native American subsample alone.

Body mass and obesity rates

Table 2 shows the results from difference-in-differences specification comparing adolescent BMI and obesity status at ages 19 and 21. The omitted category of children in these regressions is the oldest age cohort (age 13 at survey intake; age 17 at beginning of treatment).

Panel A in Table 2 reports coefficients obtained from an OLS regression of BMI on a number of controls specified in equation (1) above. Panel 2 reports marginal effects after probit regressions for youths at ages 19 and 21.[25] Columns 1 and 3 show the difference-in-difference regressions based on the model in equation 1. The coefficients of interest, while not statistically significant in these two regressions, indicate that adolescents who reside in households with at least one tribal member parent and in the youngest age cohort have lower BMI and are less likely to be obese by ages 19 and 21.

The other covariates in these regressions are also informative. We find that American Indian adolescents are 4-6 body mass index points heavier and between 33 and 42 percentage points more likely to be obese than non-Indians. We also find that the average of childhood household income (in the three years prior to the government transfer program) has a negative relationship with BMI and obesity levels at age 19, but the effect loses significance by age 21.

Based on the pure correlations plotted in Figure 1, we expect that the effects of exogenous income transfers will vary depending on initial household income. In columns 2 and 4, we test this hypothesis. In these regressions we interact initial household income (prior to the casino payments) with the original difference in difference term from columns 1 and 3. We find

[25] We report marginal effects for ease of exposition. Linear probability regressions yield the same results. The tables are available from the authors. In a series of papers Norton and co-authors (2003, 2004) have shown that interaction terms in binary regressions are not properly calculated by standard statistical analysis software output (e.g. STATA). We have used their suggested estimator (inteff) and report interaction coefficients evaluated at the mean.

significant coefficients on the triple difference term. Our results confirm that differences in initial household income affect both BMI and obesity for the household children later in life. There are differential effects of extra income between youths in different parts of the income distribution.[26] The results suggest that exogenous income transfers reduce BMI by 0.6 index points and the probability of obesity by 3% at age 19 with each $5,000 increase in *initial* household income. The effects exist only for the youngest cohort of adolescents – youths who were first treated at the age of 13. We find similar effects at age 21. The effects of exogenous income transfers on children's health last beyond children's teenage years into young adulthood.

Panel level regression analysis

In the previous analysis we have seen that there is a differential effect on BMI and obesity rates at ages 19 and 21 depending upon the initial household income levels. In this section, we investigate whether the data support similar non-linearity in the effect once we account for fixed individual characteristics and exploit only variation coming from different survey waves. As a first attempt, we compare the difference in BMI between American Indians and Non-Indians by initial household poverty status in Figures 4 and 5. These figures are similar to Figures 2 and 3 except that the two groups are now sub-divided by whether the child's household was ever in poverty prior to the casino operations. As we can see graphically, most of the increases in BMI for American Indians are attributable to the children coming from households that were previously in poverty. While all four sub-groups drift further to the right between ages 13 and 21, initially poverty-stricken American Indian children move proportionately more into the higher BMI categories.

[26] Behrman and Hoddinott (2005) find for Mexican children enrolled in the PROGRESSA program that the effects on growth are more pronounced for individuals from poorer households.

We find similar results in our regression estimates. The panel estimations, based on the model in equation (2), are reported in Tables 3-5. In addition to individual specific fixed effects, all reported models include age-by-race dummies and we cluster standard errors at the individual level. We also include an indicator for the presence of children in the household that are less than 6 years old; these results are robust to controlling for the total number of siblings in the family. Consistent with previous results in the development literature, the effect of other siblings in the household are negative and significant.

In column 1 of Table 3 we include a binary variable for casino payments that is equal to one in years when households are eligible to receive transfers and zero otherwise. The coefficient is small and not statistically different from zero. In column 2 we add an interaction term with initial household income, testing the hypothesis that the effects of casino transfers differ across income groups. Adolescents residing in households eligible for casino transfer payments have on average two thirds of a unit increase in BMI which is equal to 10% of the standard deviation of the mean BMI for adolescent tribal members (however, the coefficient is not statistically significant at conventional levels). The interaction effect, however, is negative and statistically significant; an adolescent from a household with $5,000 more in initial household income will have a BMI that is 0.18 BMI units lower than a comparable individual from the poorer household. These results indicate that the non-linearities observed in the cross-section data presented in Table 2 are also present once we account for individual-specific invariant characteristics such as racial heritage. Changes in household income have a non-linear effect on BMI for adolescents depending upon the level of the household's initial income.

Weight and Height

BMI has two components – weight and height; these components could be affected differently by extra household income. We investigate whether the differences in BMI between adolescents residing in households from different parts of the income distribution could be caused by the differential impact of extra income on these two components. Table 4 reports the effect on the government transfer on adolescent weight. We find in column 1 that there is a negative effect of receiving casino payments on gaining weight. However, this coefficient is not statistically significant and when we include an interaction variable with the initial level of the household income, the main effect becomes positive in sign (but not statistically significant).[27] The interaction term is statistically significant implying that there is a non-linearity in the effect of additional household income on weight. In this case, it appears that a child coming from a household with an additional $5,000 in initial income would experience a 0.4kg reduction in weight as a result of the extra income compared to a child coming from a household that was $5,000 poorer before the intervention.

We repeat this analysis for the adolescents' height at each survey wave. These results are presented in Table 5. In column 1 the casino disbursement dummy is positive, but not significant. We find that the coefficient on the interaction term with income in column 2 is positive and marginally statistically significant, implying that an adolescent from a household with $5,000 more in initial income will experience a 0.23 cm increase in height if they also receive the casino payments. We conclude that extra income transfers are more likely to result in height increases for children coming from better off families. This result should however be interpreted with caution as it is only marginally statistically significant.

[27] In unreported analysis we tested whether the government transfers were significantly correlated with the probability that the respondent was on a diet or had any nutritional problems such as bulimia and anorexia. We found no evidence that the casino transfers resulted in differential eating behavior or eating disorders across treatment groups.

Taken together these results show evidence for non-linearity in the height and weight measures at each survey wave. Adolescents from initially wealthier households that receive the casino transfers are more likely to experience an increase in height but less likely to experience an increase in weight. Overall, these diverging effects on the two BMI components generate the results from the BMI analysis –exogenous income transfers will have heterogenous on adolescent body mass depending on the location of the household in the initial income distribution.

Duration of Treatment and Age at Intervention Effects

Our research design is fairly unique in that we have panel data for three different cohorts of children over a relatively long period of time and we study an intervention that occurred and continued over the years of their early adolescence. Due to the unique data available, we are able to disentangle two different effects of the casino payments: the duration of the treatment versus the age at intervention. We present the results from regression equation 3 in Table 6.

The first three columns provide regression results for BMI, weight and height for survey waves 7, 9 and 11. The duration of treatment is either 2, 4 or 6 years for American Indians and set to zero for Non-Indians. We have two observations per child from each one of the three cohorts in this analysis. The coefficient for the duration variable in all three models is not statistically significant at conventional levels. However, the age at intervention variables are statistically significant for the youngest age cohort variables and achieves marginal significance for all three of the second age cohort variables. These results indicate that the age at which the casino payments first start amending household income has the largest effect on BMI, weight and height. These results are consistent with a basic model where income affects health at pivotal times in an individual's lifecycle.

V. Robustness Tests and Alternative Explanations

We undertake several robustness tests for the difference-in-difference regressions in this section. The results overall indicate that our results are not being driven by changes in labor force participation of the parents, or who receives the transfer payments, initial health conditions or education levels or geographic distance from the casino itself.

Government Transfers, Parents and the Effect on Obesity

Our initial results in Table 2 indicate that there is a long-run effect of the additional household income on the young adult obesity rates of recipients. In this section, we report whether the income effects differ according to who receives the income. Previous research in both developed and developing countries has shown that exogenous changes to household income controlled by an adult female can have beneficial effects on spending for children and household consumption goods (Duflo, 2003; Duflo and Udry, 2003; Duncan 1990 and 1994; Lundberg et al, 1997). These findings indicate that household bargaining power may dictate how additional income is spent.[29] Appendix Table 1 presents results implying that differences in the gender of the transfer recipient were not significant for this intervention. It is important to note, however, that the extra income recipients were not randomly assigned across parents' genders and there may be systematic differences between families where the mother or the father is Native American (and the other parent is not).

[29] See Lundberg and Pollak (1996) for a discussion of this literature or Behrman (1997). Additionally, in a previous paper (Akee et al, 2010) we report that household structure appears to be unaffected by the casino income payments; we find no evidence for increased divorce or marriage rates over time.

Additional Controls

In the Appendix Table 2, we present additional difference-in-difference regression results. Extra income transfers might directly affect the child's characteristics which in turn could affect their BMI and obesity levels. For example, using the same survey data Akee et al (2010) find that the income transfers improved high school attendance and completion rates particularly among children from economically disadvantaged households. Increases in own education may account for some of the observed effects on obesity. In Appendix Table 2 column 1 we report a specification controlling for own high school completion at 21. The results are very similar to the baseline specification reported in Table 2 except that the main effect of the extra income at age 21 becomes marginally statistically significant.

In column 2 of Appendix Table 2 we report a specification controlling for birth weight. This is the best proxy for initial health status, or the child's pre-treatment health capital, that we have. We split birth weight into three categories – low birth weight (below 2500 grams), normal weight (>2500 and <4500 grams) and high birth weight (>4500 grams). The omitted category in the estimation is normal birth weight. At age 21, there still is a significant negative effect of low birth weight on the probability of being obese.

The differences we find between youths coming from different income backgrounds could be due to nutrition supply, rather than demand effects. For example, if low-income households reside in areas where high quality food is sparse, children would receive worse nutrition even if parents have the financial means to provide better quality food. To test for such effects we include county-level fixed effects in our main regression. The results are reported in column 3 of Appendix Table 2. There are no significant changes in the main coefficients, suggesting that the effects we find are due to household demand choices rather than the availability of high quality food or amenities on the neighborhood level. Finally, in column 4 of Appendix Table 2 we include a measure of the individual's own income at age 21. Even though the coefficient is negative, it is not statistically significant. The main coefficient on the triple interaction term is not significantly changed.

We were concerned that the extra income would alter maternal labor force participation among tribal members who would then, in turn, affect the child's obesity levels into adolescence and young adulthood.[30] Our data contains information on parental labor force participation for all survey years. We do not find any evidence, in Appendix Table 3, for changes in parental labor force participation affecting our outcomes in our data. One possible explanation is that the extra $4000-$8000 a year was not enough to compensate for the loss of either the mother's or father's earnings.

Finally, we use global positioning system data (GPS) to compute a distance measure which serves as proxy for other non-cash transfer related effects of the casino operations on households in Appendix Table 4. The average household is 32 miles (median is 36 miles) away from the casino, with a minimum distance of 5 miles and a maximum distance of 75 miles. We find that inclusion of this measure (which is available for all survey households) and an interaction variable with treatment households does not diminish the main effects already reported.[31]

[30] Cawley (2010) offers a nice summary of the current state of the economics literature on children's obesity and in particular the role of maternal labor force participation. Skoufias and di Maro (2006) find no evidence for changes in parental labor force participation for households receiving payments from the PROGRESSA program in Mexico.

[31] We include a measure of distance from each household to the casino (using Global Position System data) in level and interacted with household eligibility for casino payments in the Appendix Table 4. One can think of this distance measure as a proxy for the other non-cash transfer effects of the casino on households. The results for this regression indicate that the proximity to the casino does not statistically affect obesity

Placebo tests

Native American children may differ from their white counterparts differently in different cohorts for reasons unrelated to the extra income transfers. To test for systematic differences we use the available information on health from the pre-casino period. Appendix Table 5 shows the results from models estimating the effects of extra income on birth weight, children's weight, height, and BMI at age 13. There are no significant differences between the three cohorts and across income categories when we examine the effects of casino payments on pre-intervention health outcomes.

VI. Discussion of Household Income – Obesity Connection

Rates of obesity among 12 to 19 year-olds in the US increased from 6.1% in 1970 to 15.5% in 2000 (American Obesity Association). Average BMI in 12-17 year-olds increased from 20.7 (in the early 1970s) to 22 (around 2000) in whites and from 20.3 to 23.7 in blacks. If we make the assumptions that increases in households' earned and unearned income affect children's similarly, and that Native Americans make similar consumption choices to the rest of the US population, we can explain between 1.8% (using BMI changes in black adolescents as base) and 3.3% (using BMI changes in white adolescents as base) of the increase in adolescent BMI with rising incomes at the lower half of the income distribution (initial household incomes less $50,000).[32] We report the break-down of the extra income effect by initial income category in

[32] Based on simulations conducted by the authors using estimates of income mobility between 1970 and 2000 (as reported by the US Census 1970 and 2000) by $5,000 income categories. In short, we estimate the proportion of households changing income categories across the income distribution between 1970 and 2000. We assign the corresponding increase in BMI (based on the results from our study, table 3) for each of these upwardly mobile households. We make the conservative assumption that a decrease in the percent of households in a low income bin is the result of these households moving to the next lowest income bin. So for example, a decrease in the percent of households in the <5000 USD category from 4.3% in 1970 to 1.8% in 2000 was the result of 2.5% of the population moving from the <5000 bin to the 5000-10000USD bin. Notice the change in annual income here is close to giving these households the average annual transfer received from the casino receipts, so we can use the estimates from

Table 7 and use these coefficients in the simulations. The important caveat here is that the effects of earned and unearned income may be different, even though we expect the difference to be less important for minors, who do not work in the US. By comparison, Schmeiser (2009) finds that increases in income explain between 23 and 29% of the increase in obesity prevalence among 25-45 year-old low income women between 1990 and 2002. Cawley et al (forthcoming) report no significant changes in the BMI of elderly Social Security recipients that can be attributed to changes in Social Security income. Currie at al (forthcoming) explain 0.5% of the increase in obesity among ninth-graders since the 1970s by the increased availability of fast food. The empirical evidence to date suggests that effects of income increases on adolescent obesity are significantly larger than the contribution of increased fast food supply.

In the next couple of paragraphs we discuss some of the relevant literature on household consumption choices, assuming that the results we have found are mainly due to household-level decisions on nutrition. There are at least two ways in which extra income could affect children's nutrition intake. First, there may be differences in the proportions of extra income devoted to food consumption. Even though we are not able to trace families' consumption choices before and after the casino payments, there is enough evidence in the literature to suspect that consumption, and food consumption in particular, was affected in different ways by the casino transfers depending on the family's level of initial income. For example, Souleless (1999) shows that liquidity-constrained households are more likely to spend extra income on food and non-durables. He finds that total consumption increases by significantly less among poorer households following an exogenous income shock.

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table 7 to estimate what would be the change in BMI resulting from moving between income categories. As low income households are likely to have more children, and we are not taking into account the distribution of children across different household bins, we are underestimating the income effect.

Second, the amount spent on food could be spent on different baskets of goods. The types of food that are bought with the extra income are also likely to differ depending on the family's finances. Reed et al (2005) estimate an array of own price and income elasticities for different types of foods. According to their results, demand for fruits and vegetables has the highest own price elasticity, while the demand for meats has the lowest. Home-cooked food is a gross complement with fruits and vegetables, dairy products, and cereals, but serves as a substitute for meats. On the contrary, food away from home is a gross substitute for all other types of foods except for meats. Dairy is the most income-elastic food class, followed by meats. Therefore, we expect that as households' incomes increase, families that react by increasing food spending are more likely to consume those foods.[33] This may be one of the main factors behind our finding that casino payments increased the height of children from the highest income groups.

The findings in the study are relevant for the debate about the causes of the great increase in obesity rates in the US since the 1980s. We interpret this increase as partly due to changes in households' real income. It is implausible that food prices decreased for American Indians, but not for the rest of the sample population at the same time as the transfers began. The differential opening of fast food chains in territories more densely populated by American Indians is also an unlikely explanation, and we offer some evidence against it by controlling for distance to the casino and county fixed effects in sensitivity analyses. In earlier research, Akee et al (2010) find that the casino transfers did not induce changes in labor force participation of parents, even though we cannot rule out the hypothesis that patterns of food preparation may

[33] Richards et al (2006) find, specifically for Native Americans, that the reduction in prices of carbohydrates over time may have resulted in an increase in spending (and consumption) on these relatively high calorie foods. The substitution away from protein rich food towards calorie-rich carbohydrates are driven primarily by differences in prices and may be an explanation for increased incidence of obesity in this community.

have changed as a result of the extra income (as suggested by Cutler at al, 2003). However, there must have been differential movement away from home-cooked and into pre-processed food across the initial income distribution for this explanation to be valid given our findings.

This study is informative to potential future policies intended to address the increasing obesity epidemic in the US. In developing countries, cash transfer programs are typically targeted at improving nutrition and child health. In Mexico, for instance, Hoddinott and Skoufias (2004) report that the Progressa program affected the quality of foods that people consumed; although this may also be due to changes in nutritional education programs. Providing poor US households with extra income would probably increase the BMI of household children; however, there may be detrimental long-run effects that affect the children's adolescent and adult obesity levels. There may be a need for educational and nutritional programs to assist in improving household consumption decisions that mitigate adolescent obesity.

VII. Concluding remarks

Due to the quasi-experimental nature of our data, we are able to identify the effect of a permanent increase in household income on weight gain and eventual obesity in adolescents and young adults. We trace out differential effects of extra income depending on the initial financial conditions in the household.

We find that individuals who come from the poorest households tend to gain more weight after the introduction of the transfer payments than their richer neighbors. Additionally, we find some evidence that the effects on height are uneven across the initial income distribution, and the initially better off children may also experience gains in height. Overall this leads to differential increases in BMI and risks of becoming obese depending on initial conditions. We show that these effects are not due to initial health conditions as proxied by birth weight or due to increases in own educational attainment. Finally, our data allow us to test whether interventions at different periods of children's development have different impacts. We show that, conditional on having the same duration of treatment, children who started the treatment at an earlier age experience more substantive changes in outcomes.

Taken as a whole, our findings support the notion that unearned extra household income affects adolescent's body mass index in very different ways depending upon where the household stands in the income distribution. This has significant implications for the design of policies intended to address the continuing adolescent and young adult obesity epidemic in the US.

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Table 1: Means and Standard Deviations of Main Outcomes and Control Variables at First Survey Wave

	Non-Indians			American Indian		
	Obs.	Mean	St. Dev.	Obs.	Mean	St. Dev.
Obese	909	0.190	0.393	304	0.362	0.481
Body Mass Index (BMI)	909	20.048	4.703	304	22.990	6.069
Weight in kgs	909	43.951	14.790	304	51.876	18.141
Height in cm	909	146.531	12.390	304	148.735	11.951
Age	909	10.870	1.625	304	10.895	1.605
Sex (1=male)	909	0.567	0.496	304	0.536	0.500
Number of American Indian Parents	909	0.017	0.127	304	1.217	0.634
Mother with high school education	909	0.290	0.454	304	0.342	0.475
Mother with college education	909	0.498	0.500	304	0.352	0.478
Mother participates in the labor force	791	0.861	0.346	255	0.863	0.345
Average pre-casino household income	899	29,104.56	17111.64	299	18,754.18	14231.39
Birthweight	822	7.485	1.333	281	7.516	1.274

Note: Average pre-casino household income categories taken at midpoints

Table 2: Marginal Effect of Casino Transfers on Obesity and Overweight Status at Ages 19 and 21; Probit Regressions

VARIABLES	(1) BMI at Age 19	(2) BMI at Age 19	(3) BMI at Age 21	(4) BMI at Age 21	(5) Obese at Age 19?	(6) Obese at Age 19?	(7) Obese at Age 21?	(8) Obese at Age 21?
Age Cohort 1 x Number of AI Parents x Average HH Income		-0.637**		-0.635*		-0.0284**		-0.0444***
		(0.269)		(0.327)		(0.0125)		(0.0167)
Age Cohort 2 x Number of AI Parents x Average HH Income		-0.0254		0.0446		0.00513		0.00295
		(0.233)		(0.277)		(0.0117)		(0.0169)
Age Cohort 1 x Number of American Indian Parents	-0.624	3.033*	-1.105	2.813	-0.0244	0.119	-0.0525	0.175*
	(1.000)	(1.777)	(1.196)	(2.092)	(0.0411)	(0.0755)	(0.0513)	(0.102)
Age Cohort 2 x Number of American Indian Parents	0.168	0.248	1.042	0.888	0.0100	-0.0225	0.0157	-0.0126
	(0.981)	(1.797)	(1.144)	(2.078)	(0.0426)	(0.0753)	(0.0543)	(0.104)
Age Cohort 1 x Average HH Income		0.219		0.520***		0.0125		0.0258*
		(0.172)		(0.184)		(0.0103)		(0.0138)
Age Cohort 2 x Average HH Income		-0.0401		0.178		-0.0121		-0.00725
		(0.153)		(0.178)		(0.0102)		(0.0142)
AI Parents and Average HH Income		-0.0694		-0.132		0.00450		0.00147
		(0.181)		(0.232)		(0.00865)		(0.0123)
Average HH Income	-0.157**	-0.179	-0.0543	-0.276**	-0.0120***	-0.0118	-0.00818	-0.0166
	(0.0682)	(0.113)	(0.0786)	(0.128)	(0.00437)	(0.00775)	(0.00564)	(0.0111)
Age Cohort 1 (13 yo)	1.461**	0.0300	1.239*	-2.287	0.0380	-0.0422	0.0596	-0.102
	(0.705)	(1.488)	(0.744)	(1.571)	(0.0470)	(0.0708)	(0.0540)	(0.0926)
Age Cohort 2 (15 yo)	0.278	0.569	0.687	-0.583	-0.0102	0.0657	0.0189	0.0743
	(0.649)	(1.524)	(0.728)	(1.708)	(0.0428)	(0.0890)	(0.0570)	(0.120)
Number of AI Parents	-0.748	0.547	-1.228	0.562	-0.0379	-0.0488	-0.0952*	-0.0673
	(0.825)	(1.415)	(1.106)	(1.666)	(0.0379)	(0.0614)	(0.0498)	(0.0824)
American Indian race	5.729***	3.906***	6.050***	3.754***	0.331***	0.283***	0.424***	0.311***
	(0.983)	(0.863)	(1.190)	(1.035)	(0.0803)	(0.0795)	(0.0842)	(0.0797)
Sex	0.630	0.566	0.571	0.493	0.0145	0.0130	0.0508	0.0492
	(0.554)	(0.560)	(0.610)	(0.614)	(0.0329)	(0.0317)	(0.0399)	(0.0395)
Mother has a High School Diploma	0.374	0.261	0.505	0.297	0.00266	-0.00236	-0.00105	-0.00869
	(1.453)	(1.442)	(1.550)	(1.552)	(0.0574)	(0.0553)	(0.0692)	(0.0676)
Mother has Some College or More	-0.661	-0.848	-1.785	-2.019	-0.0346	-0.0451	-0.135*	-0.140*
	(1.420)	(1.414)	(1.519)	(1.534)	(0.0596)	(0.0590)	(0.0770)	(0.0777)
Average Labor Force Participation of Mother	-0.358	-0.338	0.655	0.566	-0.0285	-0.0315	0.0693	0.0643
	(0.882)	(0.879)	(0.934)	(0.885)	(0.0544)	(0.0521)	(0.0697)	(0.0660)
Constant	25.65***	25.99***	25.48***	27.42***				
	(1.602)	(1.829)	(1.685)	(2.001)				
Observations	921	921	913	913	920	920	912	912

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Effect of Casino Transfers on BMI; Individual Fixed-Effects Panel Regression

	(1)	(2)
VARIABLES	BMI	BMI
Household Eligible for Casino Disbursement	-0.211	0.673
	(0.379)	(0.467)
Interaction of Casino x Average Household In		-0.184***
		(0.0513)
Number of Children in Household Less than	-0.231	-0.221
6 Years Old	(0.145)	(0.145)
Constant	22.44***	22.45***
	(1.120)	(1.118)
Observations	4,585	4,585
R-squared	0.319	0.321
Number of gsms	1268	1268

Clustered standard errors at the individual level in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Included in all specifications but not reported are: age-by-race fixed effects and a Native American-specific time trend.

Table 4: Effect of Casino Transfers on Weight in Kilograms; Individual Fixed-Effects Panel Regression

VARIABLES	(1) Weight in Kilograms	(2) Weight in Kilograms
Household Eligible for Casino Disbursement	-0.667	1.384
	(1.083)	(1.413)
Interaction of Casino x Average Household Income		-0.428**
		(0.187)
Number of Children in Household Less than 6 Years Old	-0.840*	-0.817*
	(0.458)	(0.458)
Constant	66.41***	66.43***
	(3.024)	(3.021)
Observations	4,585	4,585
R-squared	0.549	0.550
Number of gsms	1268	1268

Clustered standard errors at the individual level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Included in all specifications but not reported are: age-by-race fixed effects and a Native American-specific time trend.

Table 5: Effect of Casino Transfers on Height in Centimeters; Individual Fixed-Effects Panel Regression

VARIABLES	(1) Height in Centimeters	(2) Height in Centimeters
Household Eligible for Casino Disbursement	0.191	-0.910
	(0.404)	(0.688)
Interaction of Casino x Average Household Income		0.230*
		(0.129)
Number of Children in Household Less than 6 Years Old	-0.123	-0.135
	(0.231)	(0.231)
Constant	170.6***	170.6***
	(1.413)	(1.409)
Observations	4,585	4,585
R-squared	0.568	0.568
Number of gsms	1268	1268

Clustered standard errors at the individual level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Included in all specifications but not reported are: age-by-race fixed effects and a Native American-specific time trend.

Table 6: Fixed-Effects Regression with Age at Intervention and Duration of Treatment Control Variables

VARIABLES	(1) BMI	(2) Weight in Kilograms	(3) Height in Centimeters
Number of Years Receiving Casino Payment	0.264	0.533	-0.114
	(0.183)	(0.483)	(0.184)
Cohort 1 x Age	0.510**	2.252***	0.711***
	(0.219)	(0.605)	(0.236)
Cohort 2 x Age	0.622*	2.250***	0.375*
	(0.321)	(0.849)	(0.228)
Number of Children in Household Less than 6 Years Old	-0.489	-2.404*	-1.247***
	(0.376)	(1.337)	(0.469)
Constant	24.607***	69.089***	167.790***
	(0.647)	(1.736)	(0.543)
Observations	2,047	2,047	2,047
R-squared	0.0957	0.135	0.0593
Number of gsms	1,179	1,179	1,179

Clustered standard errors at the individual level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Included in all specifications but not reported are age-fixed effects.

Table 7: Effect of Casino Transfers on BMI; Individual Fixed-Effects Panel Regression

VARIABLES	(1) BMI
Household Eligible for Casino Disbursement	-1.401**
	(0.571)
Casino x Lowest 2 Average Household Income Categories in First 3 Survey Waves	1.631**
	(0.765)
Casino x 3rd and 4th Average Household Income Categories in First 3 Survey Waves	1.492**
	(0.590)
Casino x 5th and 6th Average Household Income Categories in First 3 Survey Waves	1.208*
	(0.723)
Casino x 7th and 8th Average Household Income Categories in First 3 Survey Waves	1.598**
	(0.678)
Casino x 9th and 10th Average Household Income Categories in First 3 Survey Waves	-0.798
	(0.731)
Casino x 11th and Higher Average Household Income Categories in First 3 Survey Waves	0.370
	(0.785)
Number of Children in Household Less than 6 Years Old	-0.234
	(0.145)
Constant	22.36***
	(1.130)
Observations	4,585
R-squared	0.321
Number of gsms	1268

Clustered standard errors at the individual level in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Included in all specifications but not reported are: age-by-race fixed effects and a Native American-specific time trend.

Figure 1: Ordinary Least Squares Regression Coefficients of BMI on Initial Household Income

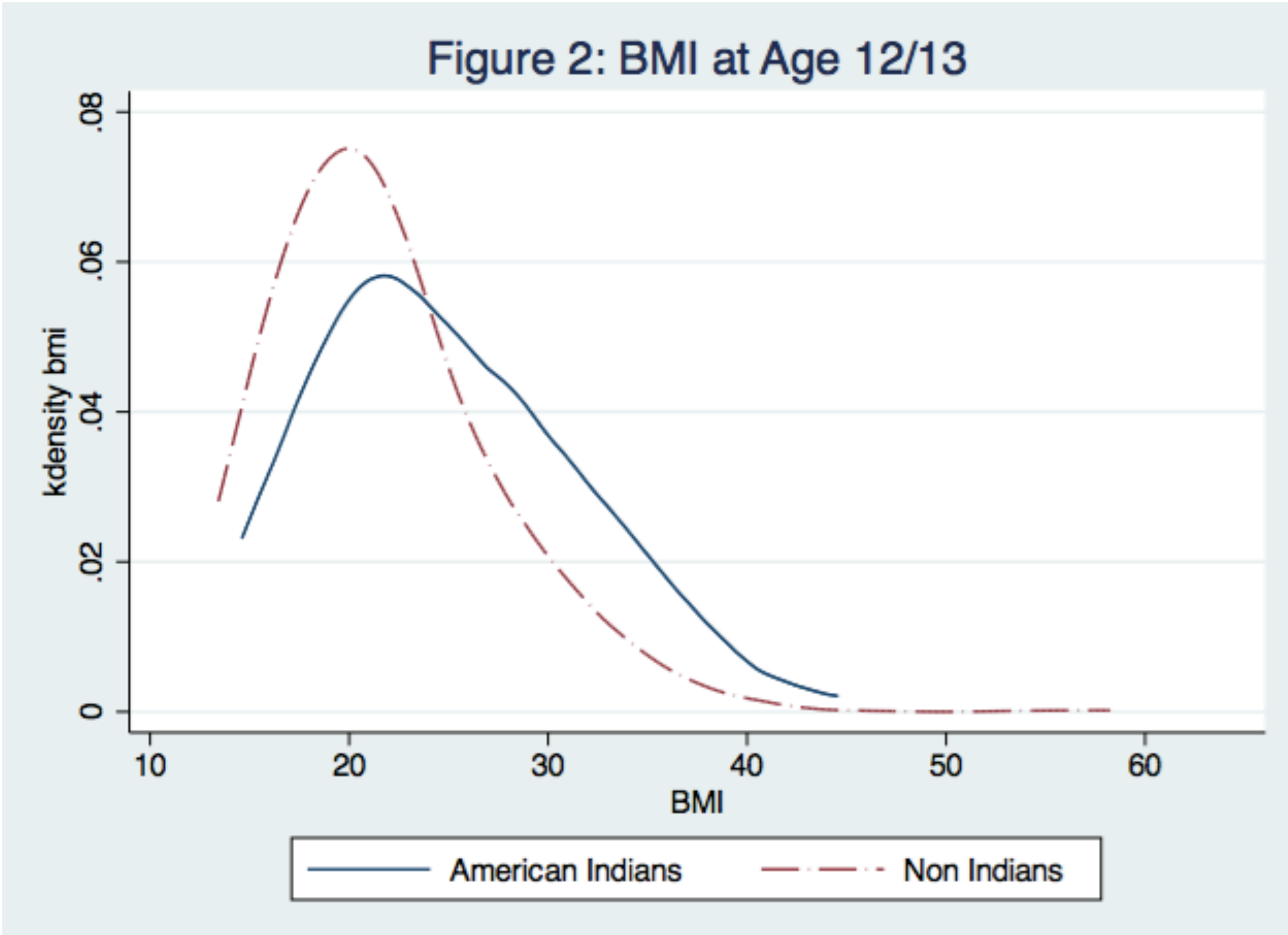
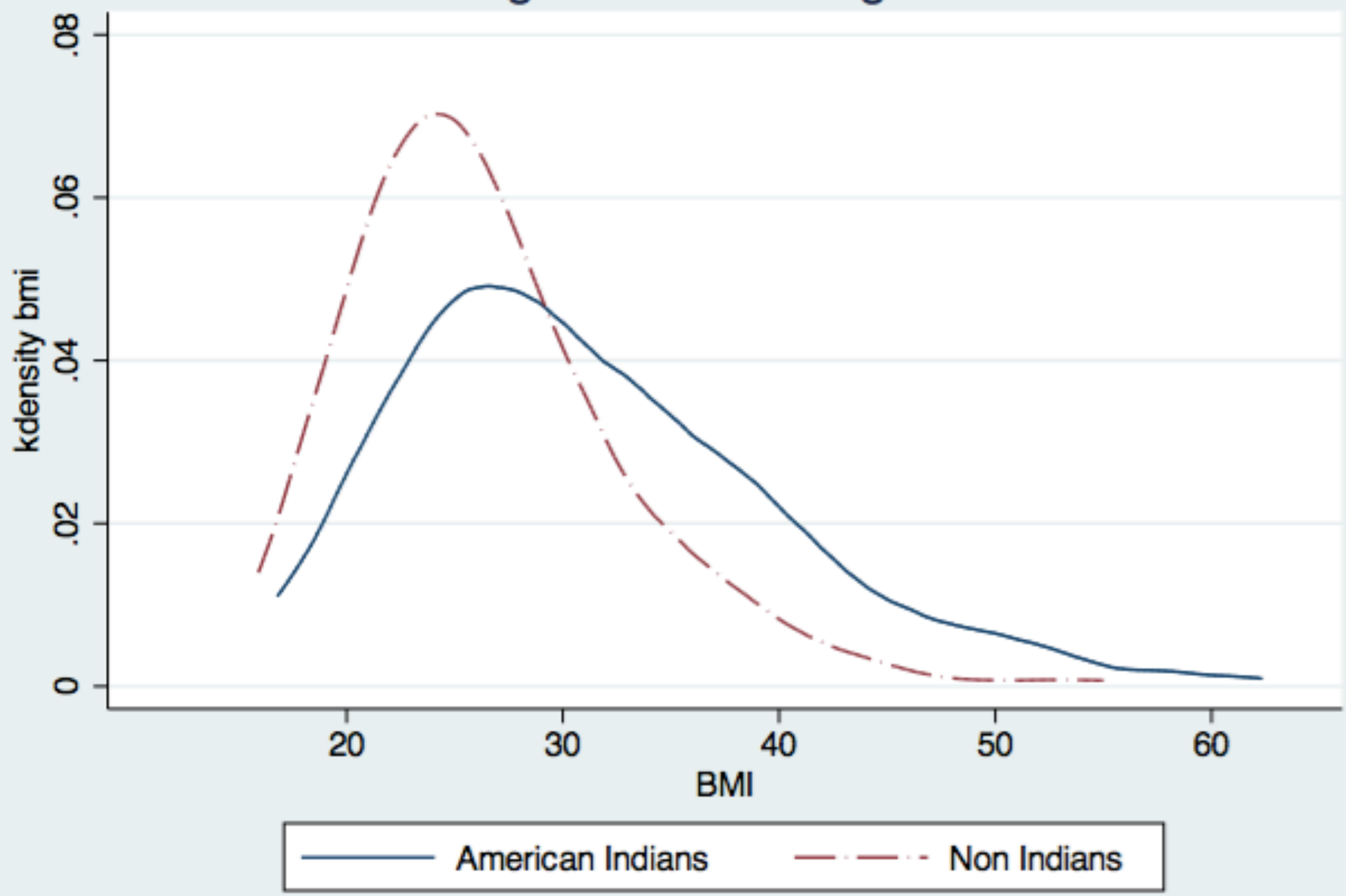


Figure 2: BMI at Age 12/13
kdensity bmi
0
.02
.04
.06
.08
10
20
30
40
50
60
BMI
American Indians
Non Indians

Figure 3: BMI at Age 21



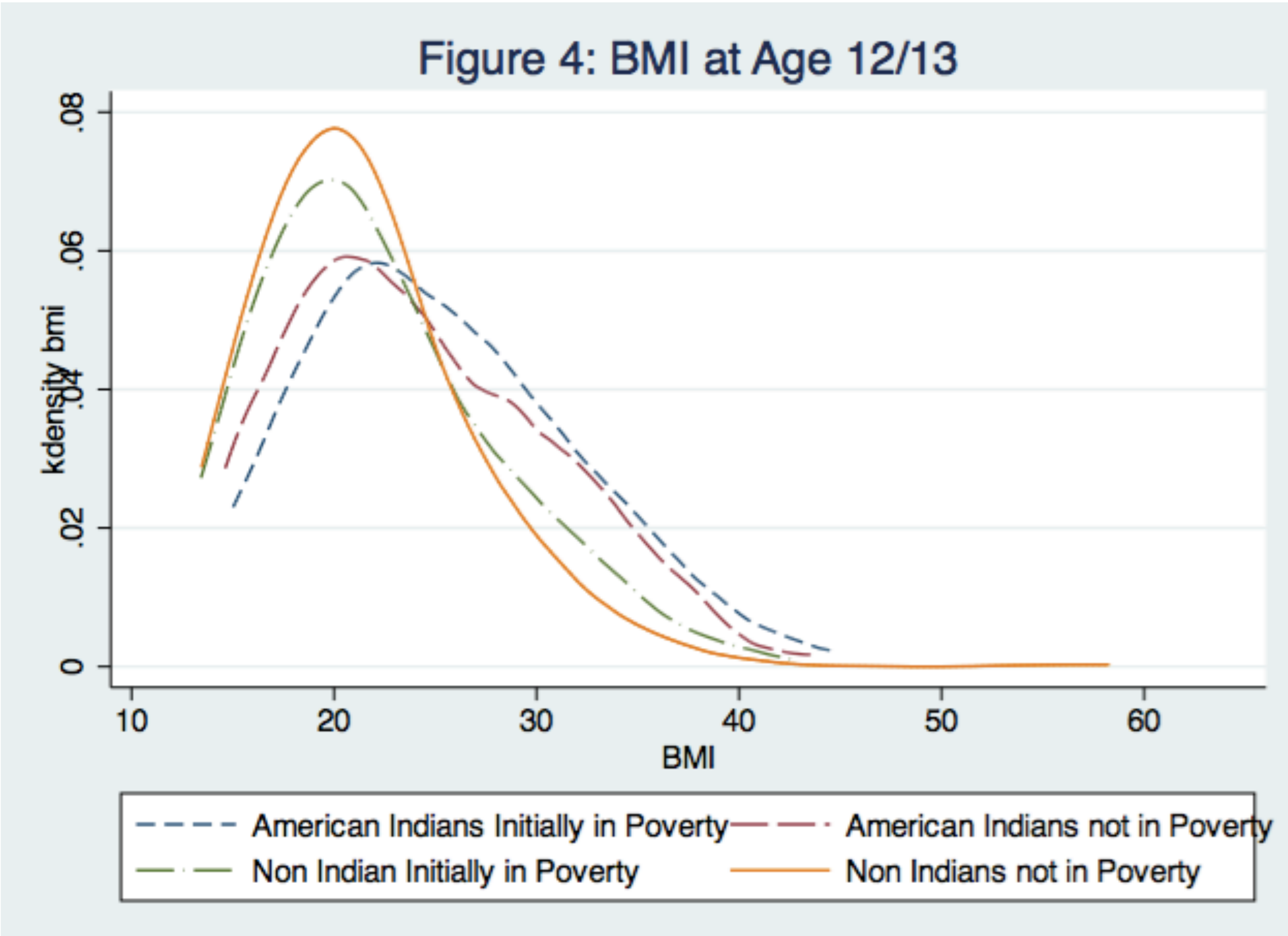


Figure 4: BMI at Age 12/13
kdensity bmi
0
.02
.04
.06
.08
10
20
30
40
50
60
BMI
American Indians Initially in Poverty
American Indians not in Poverty
Non Indian Initially in Poverty
Non Indians not in Poverty

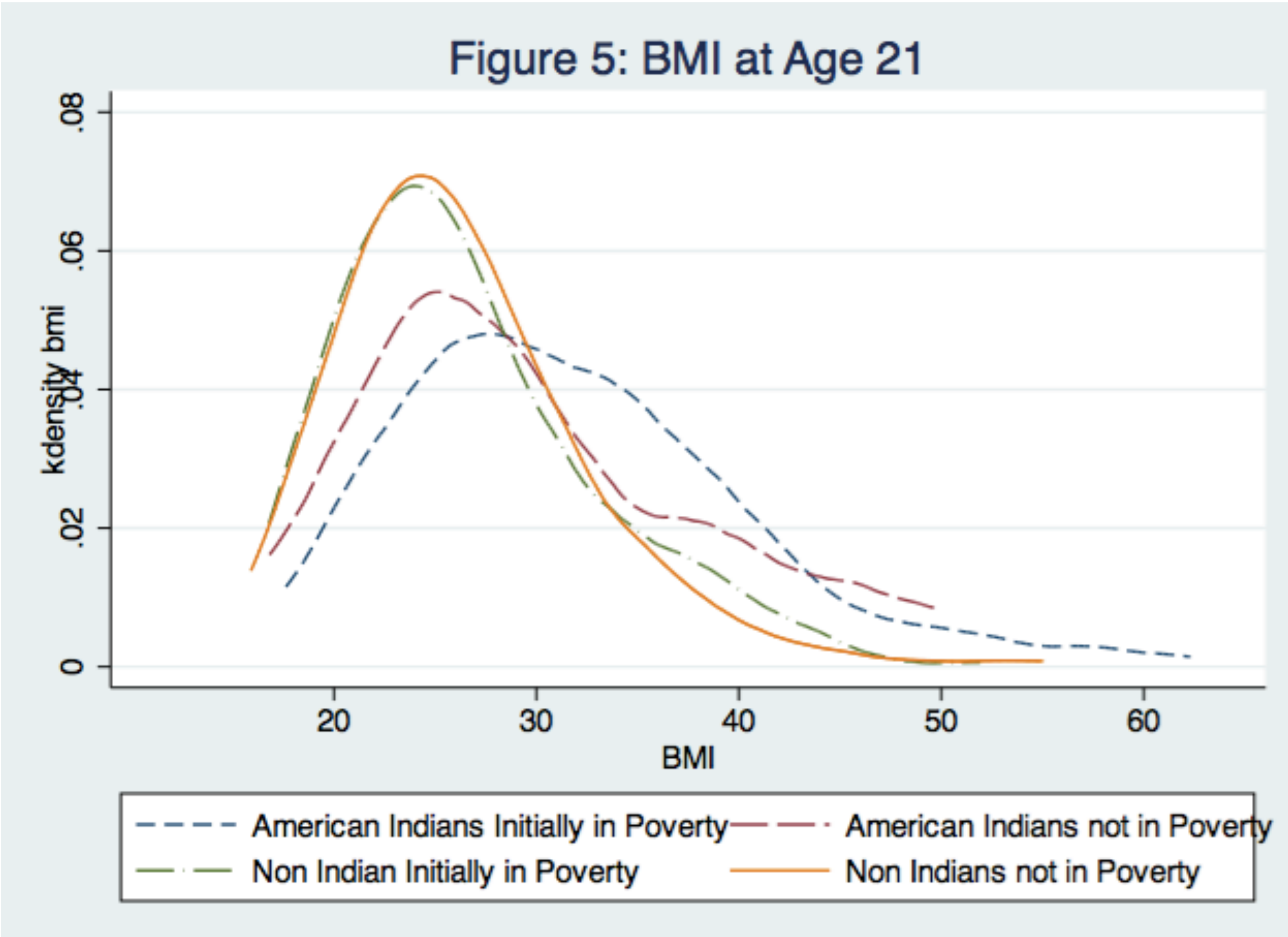


Figure 5: BMI at Age 21
kdensity bmi
0
.02
.04
.06
.08
20
30
40
50
60
BMI
American Indians Initially in Poverty
American Indians not in Poverty
Non Indian Initially in Poverty
Non Indians not in Poverty

Appendix Table 1: Difference in Difference Regression for Obesity at Ages 19 and 21 by Gender of Parent Receiving Casino Transfer Payment

VARIABLES	(1) Obese at Age 19?	(2) Obese at Age 21?
Age Cohort 1 x American Indian Mother	0.0188	-0.0460
	(0.0854)	(0.0928)
Age Cohort 2 x American Indian Mother	-0.00428	-0.0354
	(0.0912)	(0.0948)
Age Cohort 1 x American Indian Father	-0.0522	-0.0614
	(0.0788)	(0.0776)
Age Cohort 2 x American Indian Father	0.0813	0.116
	(0.130)	(0.117)
Age Cohort 1 (13 yo)	0.0401	0.0500
	(0.0466)	(0.0494)
Age Cohort 2 (15 yo)	-0.0125	0.0185
	(0.0404)	(0.0494)
Child has an American Indian Mother?	-0.0825	-0.0557
	(0.0770)	(0.0805)
Child has an American Indian Father?	-0.00408	-0.112**
	(0.0622)	(0.0570)
Average HH Income	-0.00978**	-0.00311
	(0.00461)	(0.00528)
American Indian race	0.319***	0.350***
	(0.0547)	(0.0619)
Sex	0.0121	0.0456
	(0.0342)	(0.0401)
Mother has a High School Diploma	-0.0107	-0.00734
	(0.0721)	(0.0930)
Mother has Some College or More	-0.0471	-0.158*
	(0.0689)	(0.0859)
Mother's Age	-0.00343	-0.00263
	(0.00263)	(0.00316)
Average Labor Force Participation of Mother	-0.0382	0.0566
	(0.0652)	(0.0606)
Constant	0.402***	0.342*
	(0.145)	(0.179)
Observations	911	909
R-squared	0.061	0.076

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 2: Difference in Difference Regression with Additional Controls at age 21; Marginal effects from Probit estimation

	(1) Obese at Age 21?	(2) Obese at Age 21?	(3) Obese at Age 21?	(4) Obese at Age 21?
Age Cohort 1 x Number of AI Parents x Average HH Income	-0.176***	-0.179***	-0.196***	-0.159**
	(0.0656)	(0.0650)	(0.0696)	(0.0670)
Age Cohort 1 x Number of AI Parents x Average HH Income	0.0110	0.0108	-0.0195	0.00151
	(0.0675)	(0.0671)	(0.0725)	(0.0695)
Age Cohort 1 x Number of American Indian Parents	0.675*	0.698*	0.807*	0.666
	(0.399)	(0.403)	(0.417)	(0.413)
Age Cohort 2 x Number of American Indian Parents	-0.0418	-0.0496	0.163	0.0830
	(0.413)	(0.415)	(0.430)	(0.424)
HS graduate	0.151			
	(0.180)			
Low birth weight		-0.0942		
		(0.345)		
High birth weight		0.274		
		(0.325)		
County Fixed Effects?			Yes	
Independent Household Income				-0.0393
				(0.0499)
Constant	-0.651	-0.597	-0.448	-0.312
	(0.497)	(0.511)	(0.580)	(0.545)
Observations	912	912	911	817

Note: Includes all of the control variables from regression in Table 2
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 3: Fixed-Effects Regression for Changes in Mothers' and Fathers' Labor Force Participation Rates

VARIABLES	(2) Full-Time Employment: Mother	(4) Full-Time Employment: Father
Household Eligible for Casino Disbursement	0.0440	-0.0225
	(0.0307)	(0.0192)
Household Income	0.00377	0.00251
	(0.00385)	(0.00215)
Number of Children in Household Less than 6 Years Old	-0.00786	-0.0326
	(0.0159)	(0.0221)
Mother's Age	0.00306	0.000244
	(0.00304)	(0.00196)
Constant	0.728***	0.922***
	(0.115)	(0.0750)
Observations	3,399	1,848
R-squared	0.004	0.005
Number of gsms	1136	619

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 4: Effect of Cash Transfer on Child's Obesity at Ages 19 and 21 with Distance to Casino Interactions

VARIABLES	(1) Obese at Age 19?	(2) Obese at Age 21?
Interaction 1: Age Cohort 1 x Number of	0.0241	-0.125
AI Parents	(0.0799)	(0.0917)
Interaction 2: Age Cohort 2 x Number of	0.0460	-0.0223
AI Parents	(0.0774)	(0.0950)
Interaction 1: Age Cohort 1 x Number of	-0.0829	0.0709
AI Parents x Distance to Casino	(0.160)	(0.225)
Interaction 2: Age Cohort 2 x Number of	-0.160	-0.180
AI Parents x Distance to Casino	(0.189)	(0.274)
Observations	856	853

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: Includes all other covariates used in the main regressions as well as the distance measure and the distance measure interacted with the age cohort variables and distance interacted with the number of AI parents. Sample size decreases from previous cross-section regressions due to missing observations and covariates.

Appendix Table 5 : Falsification tests: BMI, Weight, Height and Birthweight at Age 13 Pre-Treatment Regressions

VARIABLES	(3) Obese at Age 13?	(4) Obese at Age 13?	(5) Obese at Age 13?
Age Cohort 1 x Number of AI Parents x Average HH Income	-0.918	-0.758	-0.118
	(2.982)	(1.381)	(0.246)
Age Cohort 2 x Number of AI Parents x Average HH Income	0.105	0.596	0.061
	(0.645)	(0.431)	(0.067)
Age Cohort 1 x Number of American Indian Parents	5.832	7.638	0.241
	(12.602)	(6.764)	(0.939)
Age Cohort 2 x Number of American Indian Parents	0.814	-2.730	-0.325
	(4.603)	(3.190)	(0.450)
Age Cohort 1 x Average HH Income	2.029	0.845	0.122
	(1.872)	(0.735)	(0.100)
Age Cohort 2 x Average HH Income	-0.428	-0.416	0.060
	(0.449)	(0.390)	(0.053)
AI Parents and Average HH Income	-0.149	-0.392	-0.122**
	(0.410)	(0.372)	(0.051)
Average HH Income	-0.249	0.330	-0.005
	(0.294)	(0.366)	(0.031)
Age Cohort 1 (13 yo)	-6.232	-1.747	-0.379
	(10.473)	(5.490)	(0.501)
Age Cohort 2 (15 yo)	5.747	4.231	-0.609
	(3.980)	(3.403)	(0.409)
Number of AI Parents	-4.878	-3.272	1.106**
	(4.376)	(3.679)	(0.535)
Observations	773	773	731
R-squared	0.0906	0.0319	0.0349

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Includes: gender variables, dummy variables for mother's education, labor force participation of the mother.