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Effects of the Built Environment on Childhood Obesity:
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by

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Abstract

We study the effects of urban environment on childhood obesity by concentrating on the effects of walking trails and crime close to children's homes on their BMI and obesity status. We use a unique dataset, which combines information on recreational trails in Indianapolis with data on violent crimes and anthropomorphic and diagnostic data from children's clinic visits between 1996 and 2005. We find that having a trail near a home reduces children's weight. However, the effect depends on the amount of nearby violent crimes. Significant reductions occur only in low crime areas and trails could have opposite effects on weight in high crime areas. These effects are primarily among boys, older children, and children who live in higher income neighborhoods. Evaluated at the mean length of trails this effect for older children in no crime areas would be a reduction of two pounds of the body weight. Falsification tests using planned trails instead of existing trails, show that trails are more likely to be located in areas with heavier children, suggesting that our results on effects of trails represent a lower bound.

Keywords: Childhood obesity, built environment, crime

JEL codes: H4, I12, I18.

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1. Introduction:

The extent and the dire health consequences of the U.S. child obesity epidemic are well documented (Anderson and Whitaker, 2009, Hannon et al., 2005). The alarming growth in child obesity has generated many proposals, some of which have been implemented at local and state levels. These proposals have been primarily aimed at schools and food sellers. They include: state and national taxes on sugared soft drinks (Salant, 2009), bans on such drinks in schools (Price, 2006), bans on building new fast food restaurants, increases in mandatory physical education requirements, and healthier school lunch menus (Trust for America's Health, 2009). Almost all of these proposals have been made in the absence of evidence that they would have a beneficial effect or in spite of evidence that they would have no benefit. Doubts about the effectiveness of specific mechanisms for countering child or adult obesity have been raised by: Cawley et al. (2007) on physical education classes; Millimet et al. (2010) on changing school lunch programs; Sandy et al. (2011) and Anderson and Matsa (2011) on bans on new fast food restaurants; and, Whatley Blum et al. (2008) on banning sugared soft drinks.

There have been proposals to use differential health insurance pricing to reduce adult obesity (Johnson, 2009). For adults with any health insurance, an obesity surcharge on their health insurance premium is similar to a direct tax for being obese. However, even if they might be effective in altering parents' child-rearing behavior, applying differential health insurance pricing to children or limiting their access to health insurance is unlikely to be politically feasible. An incident that occurred in October of 2009 illustrates the public's reaction. A private insurer, the Rocky Mountain Health Plan, refused to sell health insurance to a Colorado family on the grounds that the family's four-month old baby was obese (Lofholm, 2009). Within two days a tsunami of national unfavorable publicity caused the company to reverse its decision (Sandell, 2009). It is similarly difficult to find politically feasible policies to reduce children's at home sedentary activities, such as television viewing or playing video games. Obesity report cards, i.e. reports on the child's BMI percentile sent from schools to a child's parents, are an example of a policy that tries to reach into the child's home. Obesity reports cards have generated a great deal of resistance (Kantor, 2007).

Reversing the child obesity epidemic requires policies that are both effective and politically feasible. A broad category of potential interventions is the built environment around the children's homes. The aforementioned ban on the construction of new fast food restaurants is an example of altering the built environment to reduce obesity. Subsidies for or public provision of potentially weight-reducing built amenities would be much easier to implement than either differential pricing of health insurance for children or obesity report cards. An additional advantage of weight-reducing recreational amenities is that they have smaller negative spillovers on individuals who are at healthy weights. While individuals who are in a healthy weight range would be taxed to support the recreational amenities, they are at least as likely to use them as obese individuals. In contrast, taxes on sugared soft drinks and bans on fast food restaurants have substantial spillovers on the non-obese.

Proposals for altering the built environment run the gamut from adding sidewalks to encourage walking to such recreational amenities as pools, soccer fields, basketball courts, and trails, to zoning laws requiring mixes of residences and retail outlets, to locating schools within walking distance of the homes (King et al., 1995, Sallis, 1998, Margetts, 2004). Proposals addressing the built environment are also running well ahead of the evidence. Although the American Academy of Pediatrics Committee on Environment recommendations include: "Fund research on the impact of the built environment at neighborhood and community levels on the promotion of overall health and active lifestyles for children and families" it nevertheless recommends a host of interventions that have little empirical support (Committee on Environmental Health, 2009).

A crucial problem for identifying public policies that can counter the child obesity epidemic via the built environment is the endogeneity of household and amenity location choices. Households who chose to live near an amenity would be expected to have stronger preferences for that amenity. Moreover, the locations of public recreational amenities are a political decision that can be influenced by the lobbying of the households most interested in using the amenity. Thus, cross-sectional studies of built environment may reveal more about the preferences of the families who live near an amenity than they reveal about its impact. Private companies, such as fast food restaurants, place outlets where, ceteris paribus, they expect to have the most customers. An example of this endogeneity problem is the conclusion, formed on the basis of many cross-sectional studies, that urban sprawl contributes to obesity. This result was not supported in either a study of people who moved between cities with different levels of sprawl (Plantinga and Bernell, 2007) or in a study of changes in the level of sprawl over time in a given city (Ewing et al., 2006).

The amenity that is the subject of this paper, recreational trails, presumably attracts families to locate nearby who value a trail as an exercise opportunity. These households would most likely have healthier diets and engage in more exercise than the average household, even without a nearby trail. Absent the random assignment of residential location, such as the Moving to Opportunity Experiment (Kling, 2004), an ideal research design is to either have an instrument that predicts location but not BMI or a natural experiment that moves households or amenities. Since body weight is influenced by so many factors, it is difficult to find a plausible instrument. Some natural experiments that have moved many households, e.g. Hurricane Katrina, also change other factors that are related to body weight. If the subjects of the natural experiment are clinically depressed it is difficult to say what their new recreational amenities did to their weights.

In this paper we are trying to investigate the effects of amenity in particular – urban recreational trails. The trails could potentially reduce the cost of exercise due to lower time cost, and, conditional on the individual having a strong preference to exercising outdoors vs. in the gym, trails can also increase the marginal utility of exercise. Both of those effects should lead to more exercise and potentially lower weight. However, one needs to consider the potential use of trails, particularly in inner-city environment. If in high crime area trails are used for criminal activity, and thus increase chances of victimization, the effects of trails could be exactly the opposite of what we predicted earlier, and lead to higher weight of neighboring residents. Thus, it is not clear *a priori* what the total effect would be and whether this effect would depend on the amount of crime in the area.

Empirically, we take advantage of the fact that the recreational trails in the City of Indianapolis had to be located on city owned land along an abandoned rail line or along several streams and rivers. That limited the usual political influence in public amenity locations. Also, given the short times between their announcement and construction, these trails could not have been factored into the location choices of most of the families who live nearby. Thus, throughout the paper we consider trails to be exogenous. To test this assumption we perform a falsification test using planned trails. Sandy et al. (2011) attempt to simultaneously estimate the effects of seventeen amenities on children's BMI. Among the seventeen amenities, they found (pps. 209-211) that trails, based on the absence of differences in BMI trends prior to the trail arrivals, were among the least likely to have locations dictated by either neighborhood politics or private information. An additional advantage of studying these trails is that they run through a variety of areas in terms of income, housing types, and land use.

Our study uses a unique dataset which combines clinical data on children, geographical data on the location of children addresses and locations of trails, as well as data and locations of violent crimes in the city of Indianapolis. We utilize a reasonably large sample of approximately 97,000 observations on children's BMI for approximately 37,000 children. Our initial research plan was to utilize a fixed effects model to estimate the impact on BMI of a trail being created near a given child's home. However, we had pre and post-trail arrival biometric measures on too few children who gained a trail while residing at the same address. Instead, we use fixed effects at census tract level.

We find that trails have a beneficial effect on children's weight, but any beneficial effects of a recreational trail depended on the nearby rates of violent crime. In addition, violent crimes alone appear to significantly raise children's weights, with or without a nearby trail. While we are not sure if that is the direct effect of crime or other characteristics of areas with more crime, we find these findings interesting. In addition, these weight gains are strongest for the younger children and for girls.

The balance of the paper is organized as follows: Section 2 reviews the literature on the weight effects of the built environment; Section 3 describes data used in this paper; Section 4 explains the estimation strategy and results; and Section 5 concludes.

2. Literature Review

Obesity epidemic has become a growing public concern. The model of "obesogenic environment" proposes a causal relationship between environmental characteristics and obesity (Egger and Swinburn, 1997, Hill and Peters, 1998, Poston and Foreyt, 1999, Swinburn and Egger, 1999). Contemporary literature is generally concerned with two aspects of the causal relationship. One set of studies focuses on the influences of the built environment (transportation, physical activities facilities, and local food environment etc.) on obesity (Ewing et al., 2006, Booth et al., 2005, French et al., 2000). Another set of studies concentrates on the impact of socioeconomic deprivation of the community on obesity (Oliver and Hayes, 2005, Liu et al., 2002, Gordon-Larsen et al., 2006).

Among aspects of the influential environmental, one key factor is the effect of unfavorable neighborhood characteristics for physical activities. The modern urban design mainly facilitates the automotive transportation (Saelens et al., 2003, Frank et al., 2004, Ewing et al., 2003, Jackson and Kochtitzky, 2003). It brings us convenience; however, it also pushes us toward a more and more sedentary lifestyle (Nelson and Gordon-Larsen, 2006, Boone et al., 2007).

Many studies have investigated the relationship between built environmental characteristics and obesity. Burdette and Whitaker (2004) explored the bodyweights of low-income children in a cross-sectional study. They found that accessibility of playgrounds and fast food restaurants, and the level of neighborhood safety had no association with children's overweight status. Hinkley et al. (2008) reviewed articles investigating the determinants of preschool children's physical activities, and found that BMI had no association with physical activities. Sen et al. (2011) utilized mothers' self-reported measures of neighborhood quality to examine whether there was any relationship between children's BMI and the built environment. They found that overall neighborhood quality did not significantly relate to children's bodyweight. However, their results showed that mothers' perception of neighborhood safety had important influences on Children's BMI.

However, Sandy et al. (2011) used panel dataset of clinical records to investigate whether changes in nearby physical or social environmental factors could be the reason for changes in children's weight. They found amenities, including fitness areas, kickball diamonds, and volleyball courts, helped to reduce children's BMI. Stafford et al. (2008) utilized a structural equation modeling approach to explore the causal relationship between neighborhood characteristics and obesity. They found that BMI was negatively related to physical activity participations, though they couldn't claim that this correlation was causal. In 2006 Gordon-Larsen et al. found in a cross-section analysis that, children who grew up in neighborhoods with more recreational facilities within a 5-mile buffer around the child's home had a lower probability of being overweight. Many researchers agree that living in a walking-friendly neighborhood was beneficial to residents' health (Li et al., 2005, Giles-Corti et al., 2003).

The utilization of community facilities is closely related to neighborhood safety. According to a report concerning neighborhood safety and physical inactivity by CDC (Weinstein, Feigley et al., 1999), residents are significantly less active in less safe neighborhoods than residents are in more safe neighborhoods. Neighborhood insecurity impedes physical activity (Romero et al., 2001, Duncan et al., 2009). Even without considering the use of recreational amenities, safety could be considered to be an independent factor that correlates with obesity. Parents' perceptions of sound neighborhood safety are associated with less obesity risk (Lumeng et al., 2006, Burdette, Wadden and Whitaker, 2006).

The present study singles out the recreational trails as a particular environmental factor, which may correlate with physical activities. As stated in the previous section, the advantage of using trails as a built environmental indicators is that, the allocations of trails in Indianapolis could be considered as "exogenous" to subjective decisions. Of the seventeen amenities in Sandy *et al.* (2011), the trails were the least likely to have locations dictated by either neighborhood politics or private information. It is clear in Figure 1 that, most trails within Indianapolis locate along waterways, except some waterways along the western and eastern boundaries which are thinly populated. There is also one trail placed on an unused railroad track which is the vertical trail segment labeled as the Monon Trail.

Another advantage to studying trails is their popularity, and their commonly agreed positive effects on promoting walking and cycling (Merom et al., 2003, Librett et al., 2006). Trails are being heavily used in Indiana (Lindsey et al., 2002). The Monon Trail has been described as perhaps the most heavily used urban recreational trail in the United States (Ottensmann and Lindsey, 2008, Reynolds et al., 2007). If a recreational trail had a beneficial effect we would expect it to show up in these data.

3. Data:

We use a unique dataset which includes data from children's clinic visits, their home addresses, linked to the locations of recreational trails as well as dataset of locations of violent crimes. The main sources of our data are: (1) clinical records from pediatric ambulatory visits to the Indiana University Medical Group between 1996 and 2005; (2) reports of violent crimes from the Indianapolis Police Department and the Marion County Sheriff's Department;

(3) data on the initial year and the length of trails within a quarter mile of the child's home. These data sources are described in more detail below.

Figure 1 shows the locations of trails in the city of Indianapolis. Figure 2 shows the locations of children's residences superimposed on the trails from Figure 1. A gray dot indicates a single residence. And shaded gray areas indicate groups of residences that are too close to each other to individually distinguish. The blue shading shows the residential locations of children who live within a quarter mile of a trail. To make it impossible to identify actual addresses, each dot on the map was randomly shifted a small distance. The data used in the regressions used the actual point locations.

(1) *Clinical records*

The Regenstrief Medical Records System (RMRS), in existence since 1974, is an electronic version of the paper medical chart. It has now captured and stored 200 million temporal observations for over 1.5 million patients. Because RMRS data are both archived and retrievable, investigators may use these data to perform retrospective and prospective researches. The RMRS is distributed across 3 medical centers, 30 ambulatory clinics, and all of the emergency departments throughout the greater Indianapolis region. RMRS supports physician order entry, decision support, and clinical noting, and is one of the most sophisticated and most evaluated electronic medical record systems in the world.

Using the RMRS, we identified medical records in which there are simultaneous assessments of height and weight in outpatient clinics for children ages 3-18 years inclusive. For these clinic visits, we extracted the visit date, birth date, sex, race, insurance status, and visit type (e.g. periodic health maintenance versus acute care). We found that too few patients had private insurance for this variable to have any predictive power. Because height and weight measurements are routinely performed as part of pediatric health maintenance, these measures should be present for virtually all children receiving preventive care at each of the study sites. The data generated by pediatric visits in the RMRS include higher representation of low-income and minority households compared to the demographics of the study area because the associated clinics serve a population that is mostly publicly insured or has no insurance.

The initial age range of subjects in this study is three to eighteen years. National guidelines for well-child visits advocate annual visits between ages 3-6 years and at least biannual visits thereafter. We observed much more frequent well-child visits for girls age 16 or above than for boys, presumably because the former often use these visits to obtain gynecologic care, such as a prescription for contraception. We extracted ICD-9 codes or other diagnoses list data for identifying children who may have systematic bias in growth or weight status (i.e. pregnancy, endocrine disorders, cancer, congenital heart disease, chromosomal disorders, and metabolic disorders), and excluded observations for such children. We also excluded patient encounters prior to 1996 because the RMRS did not archive address data before this date.

(2) *Recreational Trails*

The Indianapolis Parks and Recreation Department provided data on the opening date of each segment of each trail. Figure 3 shows the date of opening of each trail segment. The opening dates are all recent. Thus, it is highly unlikely that much of the pattern of residential location among the stayers whom we first observe in clinic visits shortly after the trails open was influenced by the presence of a trail. The summary of trails availability is presented in Table 1. We can see that between 1995 and 2006 the city of Indianapolis gained around 50 miles of trails with major trails openings taking place in years 1997 and 1998.

Trail access metrics:

We created a measure of the length of any trails, trails4, scaled per 100 meters, within a circle of radius 0.25 miles centered on the child's home. The minimum value for this variable was 0 for children represented by the gray dots or gray shading in Figure 2. The mean amount across all children with or without a nearby trail was 0.122, i.e. 12.2 meters. The maximum length was 8.44, i.e. 844 meters. We also created a variable that measured the length of any planned but not yet constructed trails, again scaled per 100 meters. This variable takes the value zero when no trail was planned or when a trail already exists.

There is no established metric for representing trail availability. It is not clear that living immediately adjacent to a trail that follows a straight line provides any less of a recreational opportunity than living immediately adjacent to a trail that follows a zigzag path. Our length-within-a-circle metric treats a zigzagging trail as providing more recreational opportunity than a straight-line trail when both are adjacent to the child's home. However, most of the children who live near a trail are near segments that are reasonably approximated as a straight line. To get a sense of how circles of 0.25-mile radius would fit in to Figures 1 and 2, Indianapolis is approximated by a square of 20 miles on each side. Thus, even the trails along winding paths next to rivers are reasonably approximated by a straight within these small circles.

We experimented with different metrics including a dummy variable for having any trail and a count for the number of trails (some children lived near intersections of trails). The distance within the circle metric performed better than the dummy or the count. We also experimented with the square of the distance to see if there was a non-linear effect to length of nearby trails. It was not significant in any specification. We have approximately 6,492 biometric observations on approximately 1,800 children who had a trail within a quarter mile.

(3) *Crime data*

During the study period, the primary law enforcement responsibility for Marion County was divided between the Indianapolis Police Department (IPD), which had responsibility for the area within the original Indianapolis boundary, the Marion County Sheriff's Department (MCSD), which had responsibility for most of the outlying areas of the county, and the police departments of the four small excluded municipalities of Speedway, Lawrence, Southport, and Beech Grove. When the city limits of Indianapolis were expanded to the border of Marion County in 1970, the original police jurisdictions were not affected. In 2007 the Indianapolis Police Department and the Marion County Sheriff's Department were merged into the Indianapolis Metropolitan Police Department.

From the Indianapolis Police Department, for the IPD service area in which they had primary responsibility, we have a dataset of the geocoded locations of all crimes reported for the Federal Bureau of Investigation's Uniform Crime Reports (UCR), from 1992 through 2005. From the Marion County Sheriff's Department, for the area in which they had primary responsibility, we have a dataset on the point locations of a wide range of crimes and other incidents, including the UCR crimes, from 2000 through 2005. We are using information on the crimes from both datasets that are included in the UCR violent crime categories: criminal homicides, rapes, robberies, and aggravated assaults. The dataset includes the date and time of the crime, and more detailed information on the specific type of crime within each of those four categories. Because of the manner in which these data have been assembled, we have reason to believe that these are accurate locations and that the classification of the type of crime is accurate.

To summarize, we have the following coverage for violent crimes:

1) Up through 1999, for the IPD service area only.

2) From 2000 through 2005, for both the IPD service area and the MCSD jurisdiction.

No crime data are available for any time period for the jurisdictions of the four small excluded municipalities that are within Marion County.

Data cleaning

In examining the height and weight data from the clinical records we found highly improbable patterns, such as a child shrinking five inches in height from one well-child visit to the next. We calculated *z*-scores for height and weight measures based on year 2000 US Centers for Disease Control and Prevention (CDC) growth charts. We used CDC statistical programs to identify biologically implausible values for heights and weights (CDC, 2000). Figure 4 shows the histograms of heights and weights, excluding biologically implausible values with *z*-scores greater than +3.0.

Visually, there is a small amount of truncation for the heights in the right tail of the height distribution. As can be seen in the second graph, the truncation in the right tail of the body weight distribution is substantial and may reflect inappropriate exclusion of high weight-for-age children. The CDC Growth Chart reference population spans the period 1963 to 1994, and thus does not fully cover the epidemic in child obesity of the past two decades. Another visual indicator of the extent of the epidemic is how much the distribution has shifted to the right relative to the mean of the reference population. We treated observations with weight-for-height and weight-for-age *z*-scores equal to or exceeding +5.0 as outliers likely resulting from data entry error or measurement errors.

The data were restricted to children in the age range 3 through 16. Age was recorded in years since birth. The average age in our sample is 8.26. Additional restrictions were based on CDC growth charts. Children whose BMI z-score (the variable BMIz, based on pre-epidemic mean and standard deviations) were above 5 or below -5 were dropped as being likely data recording errors. The average BMI for the sample is 19.26.

Summary statistics are presented in Table 2. The variable *Well Visit* is an indicator variable for a clinic visit being a well child visit and not for seeking treatment. Majority of observations in our dataset come from African American children, while only 29% of observations are for white children. Over twenty percent of children in our sample are obese with an average BMI of 19.26. On average there were 20 counts of Class A violent crimes within a quarter mile of the child's home in a calendar year. We also have data on the census tract in which the child resides.

4. Estimation and Results:

In the base line regression we estimate the effect of trails and crime and the interaction of the two on children's BMI controlling for age, age squared, race and gender of the child, as well as year and census tract fixed effects. Table 3 has the results of fixed effects regressions on the full sample, and for children above three and under eight years of age, and for children age eight or more and under sixteen. The explanatory power is much higher for the older sample. The R squared is 0.16 for the older sample and 0.06 for the younger. Trails are not significant for the younger children but are highly significant for the older children. If there were no crime near the child's home (thus eliminating the crime term and the interaction with crime and trails), gaining a trail would be beneficial. For example, if the trail were to pass right by child's home (thus there would be 800 meters of trail within the buffer) it would reduce BMI by 0.200 * 8 = 1.6 BMI points. For a child of average height among the older children this would translate into reduction of approximately 8 pounds (Δ-pounds = Δ-BMI * inches^2/703). Evaluated at the mean length of trails (among those with any trails close to home) of 200 meters this effect would be a reduction of 2 pounds.

The coefficient on crime is positive in all three regressions, but it is significant and has the highest value in the younger child sample. In the full sample, the magnitude of the coefficient on crime is twice the magnitude of the coefficient on trails, representing that 100 violent crimes in an area with no trails are associated with twice effects on children's BMI than 100 meters of trails in an area with no crime. In addition, the interaction between crime and trails is positive and significant, except for younger children. In the full sample this suggests that while adding a trail in an area with no crime is beneficial, in an area with violent crimes adding a trail would increase children's weight. Table 3.A shows the changes to children's weight at different levels of crime and trail lengths using coefficients for the full sample. In the full sample this suggests that while adding a trail in an area with crime levels at or below average is beneficial, in areas with crime above average could be detrimental.

The reason for the effects of trails depending so drastically on amount of nearby crime can be explained using Figure 5. Consider a child living in a low crime area. The child's home is represented by a dot in a middle of the quarter mile buffer around it. Having a short trail in the buffer corresponds to having a trail far from home, which means that the child will have to walk some distance to that trail. Thus length of trail represents the ease of access to the trail. However, if the child lives in a higher crime area, having a longer trail (or a trail closer to home) might mean having more nearby crime, if the trail is used for criminal activity, which in turn might result in less exercise and higher weight.

Since we are not controlling for endogeneity of residential choices by families we cannot claim that the crime coefficients are causal. They could be interpreted as suggestive of children in higher crime areas having fewer opportunities for outdoor play and exercise. But they can also be attributed to the differences in many unobserved characteristics of families who choose to reside in high and low crime areas.

The broad conclusions looking at the split between younger versus older children is that trails only have a weight reduction benefit among older children, which is intuitive. And at its mean level in our full sample, crime is more important in adding to weight than trails are in reducing it. Crime is significantly directly associated with higher weights for the younger children and through an interaction with trails significantly for the older children.

The remaining regressions split the full sample by levels of crime, income, gender, and into movers and stayers. The results are reported in Table 4. These splits show that the beneficial effects of trails are concentrated among males who are in higher income areas with below median levels of crime (below 15 violent crimes per year). Conceptually, the most important split is the movers versus stayers. Movers are defined as children who have changed addresses during sample years, while stayers are children who appear at least twice in the dataset and report the same address in all years. While there are some differences in the magnitude of the effects of trails and crime, the fact that all of the coefficients have the same sign is encouraging. Ideally, we would like to be able to distinguish between movers towards the trail and movers away from the trail we were unable to find any effects in these groups because of small sample sizes.

While there is a greater weight reduction for trails among the stayers, as was hypothesized, the difference between the movers and the stayers is small. Coefficients on trails are significant and the stayers' coefficient is highly significant. This result suggests that the weight reduction benefits to the trails would be present if a randomly selected child was given a nearby trail, or equivalently, that the weight reduction is not caused by the movement of families who prefer exercise to locations near a trail.

Falsification test: Do Planned Trails Reduce Child Obesity?

The regressions in Tables 3 and 4 estimate the effects of real trails on child obesity. Even though the time between the announcement and construction of these trails was short, these regressions do not rule out the possibility that weight reduction effects were due to the households that most value exercise opportunities relocating near a planned trails before we could observe them in our data, i.e. before they took a child to a clinic visit, or that lobbying by households that most valued the trails affected their locations. To address these two possibilities we investigated a counter-factual, what effect do the planned but yet-to-be-constructed trails have on children's weights? If planned trails are associated with weight reductions in a cross-sectional regression, then it is much more likely that our results in Tables 3 and 4 are due to self-selection of households into areas that will gain a trail or exercise-loving households lobbying for trails. Logically, a yet-to-be-built trail cannot reduce children's weights. Conversely, if there is no effect on children's weights from living near planned trails, our results cannot be due to either of these two forms of self selection. Table 5 reports the results of a regression that includes planned trails but codes the realized trails at zero.

The pseudo "effect" of announced but yet-to-be built trails alone is significantly higher BMI overall and for younger children. Crime has the same association as in the regressions with real trails, i.e. with higher BMI. The interaction of planned trails and crime is negative and significant in all three regressions. The interpretation of the interaction terms is that areas with higher crime rate which will receive trails in the future on average have slimmer children. Apparently, the new trails were planned for areas that have higher weight children in no crime areas. This could be interpreted as families with heavier children are lobbying to have better access to trails. But if that is the case our results from previous tables should be interpreted as lower bounds of the "true" effect.

Alternative Measures of Children's Weight Status:

Our main results—that boys in general and children in the age range of 8 to 16 appear to weight-losing if a trail is located within a quarter mile of their homes and that all children's weight gains are associated with higher levels of nearby violent crimes—might be sensitive to choice of weight measure. The other commonly used dependent variable is an indicator variable for obesity status. A child with BMI above the 95th percentile of children of the same age and sex during the pre-epidemic cohorts is considered to be obese[2]. Table 6 duplicates Table 3 but replaces BMI as the dependent variable with an indicator variable for obesity status. These regressions are linear probability models controlling for time and census tract fixed effects. Probit models (available from the authors) yield similar results.

As with the obesity status results, the effect of nearby trails on obesity is greater for the older children than for the younger children. While crime alone always has a positive coefficient, it is only significant in the full sample. The interaction between crime and trails is positive in all three regressions and significant in the full sample and the younger children sample. The broad pattern observed in the BMI regressions is also observed in the obesity regressions—trails reduce obesity more for older children and crime increases obesity more for younger children.

Table 7 duplicates the regressions founding Table 5 with obesity substituted for BMI. Again, most of the results observed for BMI hold up for obesity. Trails in low crime areas are much more likely to reduce obesity, as are trails in higher income areas. One difference is trails significantly reduce both male and female obesity, with a greater effect for females. For BMI the only significant effect of the trails variable was for males. Lastly, the most interesting of the splits, the movers versus the stayers, had about the same coefficient in the obesity regressions. Again, that supports the conclusion that the results are not due to households that most value exercise relocating to near a trail.

5. Conclusions:

In this paper we provide evidence of the effects of recreational trails on children's weight. We argue that the location of trails is exogenous due to the fact that trails follow riverbanks and abandoned railways. In addition, we try to account for differential effects of trails depending on levels of crime in the neighborhood. We recognize that families are not randomly selected into low and high crime areas and we do not give causal interpretation to crime coefficients, but recognize that those coefficients are likely to represent the effects of many unobserved family characteristics that govern the residential choice.

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[2] For detailed information, see http://www.cdc.gov/obesity/childhood/defining.html.

We show that recreational trails can have beneficial effects on children's weights in low crime areas, but those effects become detrimental in high crime areas. In addition, we show that the effects differ by age and gender of the child, but are qualitatively similar for both children's BMI and obesity status. We show that in areas with no crime the addition of 100 meters of trails next to child's home would lead to a reduction of one pound of weight among older children. Since our sample children are mainly from low-income or minority households, our findings may not be nationally representative.

The main contribution of this paper is showing that the effects of trails could be not only different in magnitude among different neighborhoods, but they can also have opposite signs. While many policy makers are interested in finding a solution to the rise in childhood obesity these solutions might differ by types of populations being targeted. What might have beneficial effects in one area might have opposite effects in another. We are showing that these results can vary substantially within an urban area in Indianapolis, but the differences of the effects of any recreational facility or built environment in general could be much larger when comparing urban and urban settings, as well as different areas of the country.

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Figure 1: Trails in Indianapolis

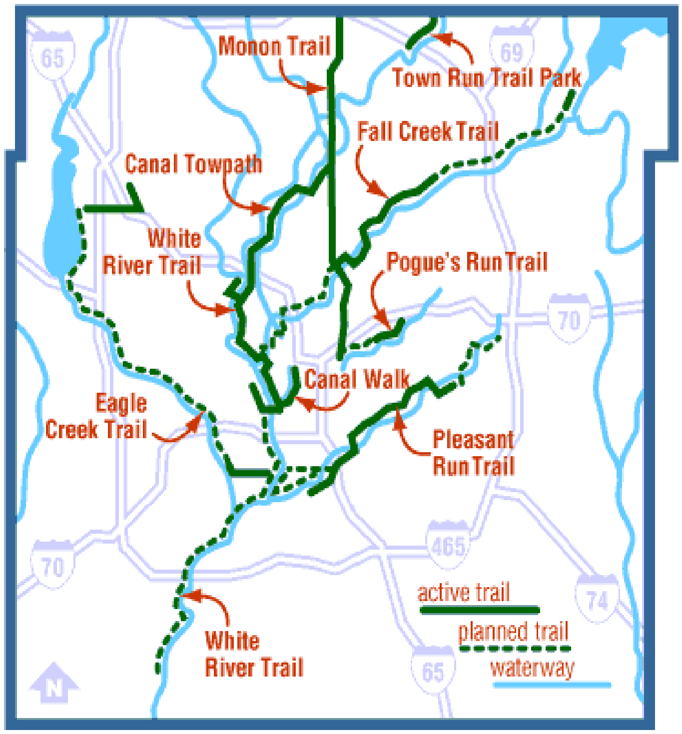

Figure 2
Residential Locations Within and Beyond a Quarter Mile of a Recreational Trail

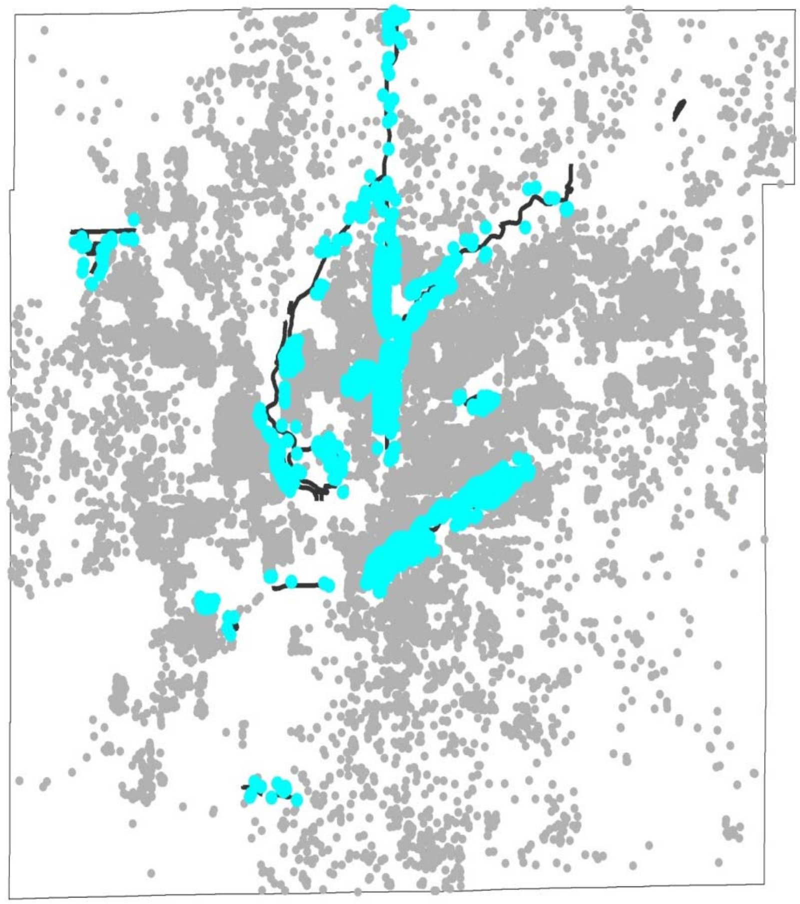

Figure 3
Opening Dates by Trail Segment

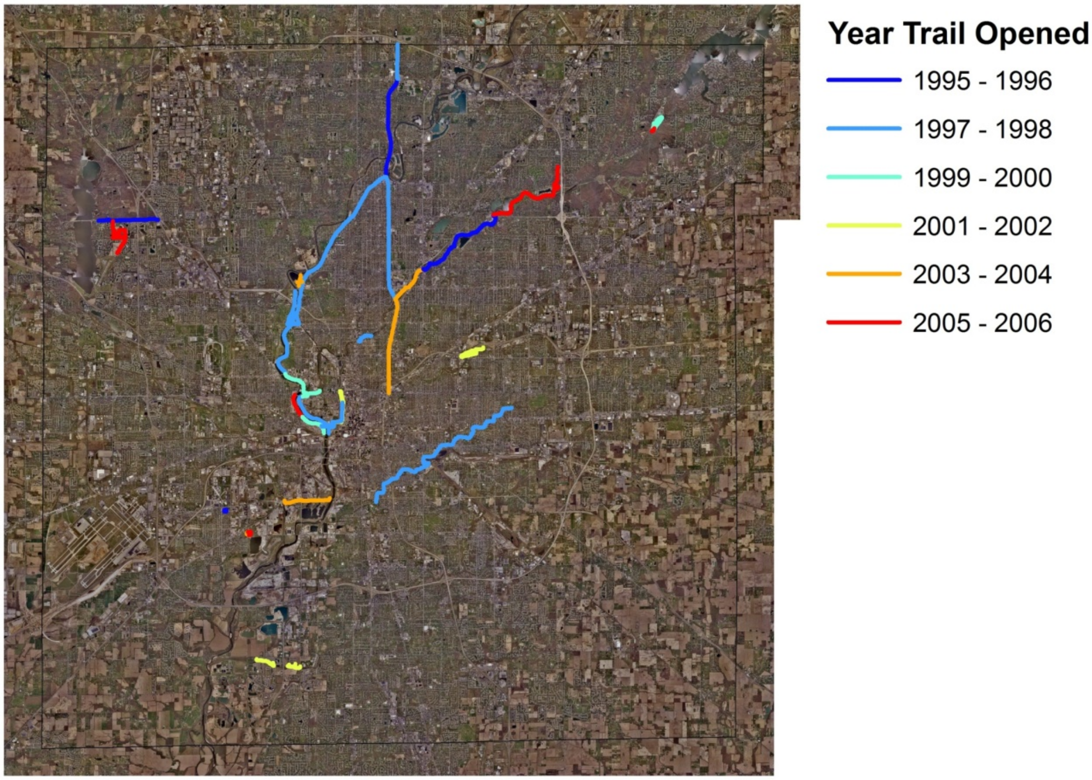

Figure 4

Histograms of Standardized Height (haz) and Weight (waz) Scores after Dropping Observations with *z*-scores at or Above 3

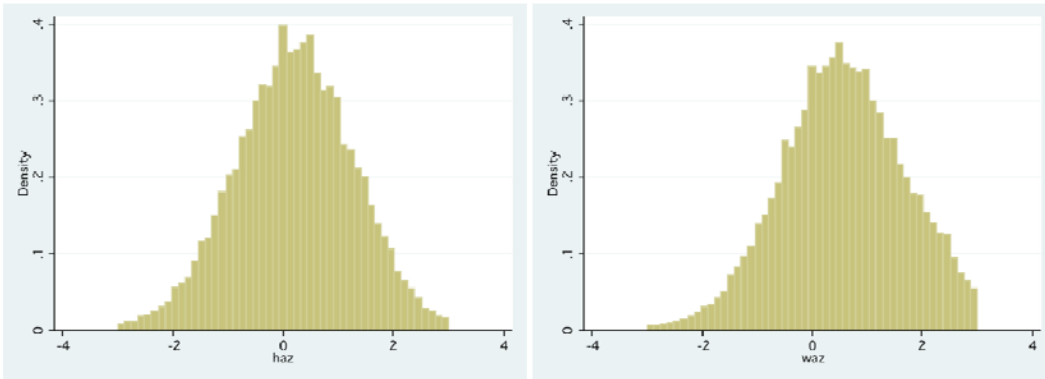

Figure 5. Locations of short vs. long trails with relation to child's address.

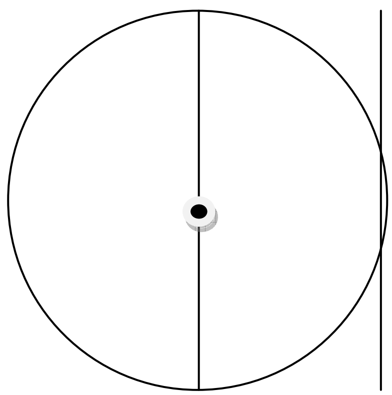

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Table 1: Miles of Trails by Year

Year	New Miles	Cumulative Miles
1995	5.14	5.14
1996	3.01	8.15
1997	11.87	20.02
1998	11.76	31.78
1999	1.21	32.99
2000	2.31	35.3
2001	2.84	38.14
2002	1.62	39.76
2003	4.45	44.21
2004	1.66	45.87
2005	1.9	47.77
2006	4.58	52.35

Table 2: Descriptive Statistics

Variable	Mean	St.Dev.
BMI	19.26	5.32
obese	0.21	0.41
age	8.26	3.79
Well Visit	0.83	0.38
black	0.53	0.5
hispanic	0.12	0.33
white	0.29	0.46
female	0.48	0.5
trails x 100m	0.122	0.54
planed trails	0.119	0.98
crime x 100	0.202	0.21
Observations	96,955	
Children	36,936	

Table 3: FE Regressions on BMI

	Full sample	Age<8	Age>8
trails	-0.144***	-0.048	-0.200**
	(0.05)	(0.04)	(0.09)
crime	0.302**	0.324***	0.247
	(0.13)	(0.12)	(0.25)
trails*crime	0.517**	0.224	0.754**
	(0.21)	(0.16)	(0.36)
Observations	96,955	50,836	46,119
R-squared	0.327	0.063	0.155

Note: Robust standard errors in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%
All regressions include year and census tract fixed effects
Control variables: age, agesq, wc, black, hisp, white, female

Table 3.A: Effects of crime and trails in the full sample.

crime	trails							
	100	200	300	400	500	600	700	800
0	-0.144	-0.288	-0.432	-0.576	-0.72	-0.864	-1.008	-1.152
10	-0.0621	-0.1544	-0.2467	-0.339	-0.4313	-0.5236	-0.6159	-0.7082
20	0.0198	-0.0208	-0.0614	-0.102	-0.1426	-0.1832	-0.2238	-0.2644
30	0.1017	0.1128	0.1239	0.135	0.1461	0.1572	0.1683	0.1794
40	0.1836	0.2464	0.3092	0.372	0.4348	0.4976	0.5604	0.6232
50	0.2655	0.38	0.4945	0.609	0.7235	0.838	0.9525	1.067

Table 4: Split Sample FE Regressions on BMI

	Below Median Crime	Above Median Crime	Females	Males	Lower Income < 35K	Higher Income > 35K	Movers	Stayers
trails	-0.345***	-0.067	-0.08	-0.178***	-0.038	-0.208***	-0.133*	-0.294***
	(0.08)	(0.10)	(0.08)	(0.07)	(0.09)	(0.07)	(0.0782)	(0.0815)
crime	0.16	0.15	0.480**	0.10	0.19	0.27	0.434**	0.220
	(0.63)	(0.17)	(0.21)	(0.17)	(0.17)	(0.24)	(0.177)	(0.275)
trails*crime	3.378***	0.27	0.33	0.623**	0.14	1.158**	0.295	1.632***
	(0.98)	(0.33)	(0.31)	(0.27)	(0.29)	(0.48)	(0.280)	(0.403)
Observations	48,034	48,921	46,523	50,432	49,434	47,521	52,912	27,988
R-squared	0.33	0.33	0.36	0.30	0.33	0.33	0.319	0.350

Note: Robust standard errors in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%
All regressions include year and census tract fixed effects
Control variables: age, agesq, wc, black, hisp, white, female

Table 5: FE Regressions on BMI for Planned Trails

	Full sample	Age<8	Age>8
planned trails	0.0623**	0.0648**	0.06
	(0.03)	(0.03)	(0.06)
crime	0.387***	0.370***	0.35
	(0.13)	(0.12)	(0.25)
interaction	-0.302***	-0.158*	-0.466**
	(0.10)	(0.09)	(0.19)
Observations	96,955	50,836	46,119
R-squared	0.327	0.063	0.155

Note: Robust standard errors in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%
All regressions include year and census tract fixed effects
Control variables: age, age squared, wc, black, hisp, white, female

Table 6: FE Regressions on Obesity

	Full sample	Age<8	Age>8
trails	-0.0152***	-0.0110*	-0.0160**
	(0.00)	(0.01)	(0.01)
crime	0.0220*	0.02	0.02
	(0.01)	(0.02)	(0.02)
trails*crime	0.0471**	0.0468*	0.04
	(0.02)	(0.02)	(0.03)
Observations	96,955	50,836	46,119
R-squared	0.029	0.028	0.023

Note: Robust standard errors in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%
All regressions include year and census tract fixed effects
Control variables: age, agesq, wc, black, hisp, white, female

Table 7: Split Sample FE Regressions on Obesity

	Below Median Crime	Above Median Crime	Females	Males	Lower Income < 35K	Higher Income > 35K	Movers	Stayers
trails	-0.026***	-0.01	-0.018***	-0.0119*	-0.01	-0.019***	-0.017**	-0.023***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
crime	0.04	0.003	0.0479***	-0.004	0.01	0.0435*	0.008	0.072***
	(0.06)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
trails*crime	0.30***	0.04	0.067**	0.03	0.01	0.0697*	0.034	0.128***
	(0.10)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)
Observations	48,034	48,921	46,523	50,432	49,434	47,521	52,912	27,988
R-squared	0.035	0.031	0.038	0.036	0.031	0.033	0.040	0.047

Note: Robust standard errors in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%
All regressions include year and census tract fixed effects
Control variables: age, age squared, wc, black, hisp, white, female