Physical activity and child health: Can school-based interventions make a difference?

ANNETTE QUINTO ROMANI^{1,*}

¹ Department of Sociology and Social Work, Aalborg University

Abstract: Childhood obesity and inactivity is a significant public health problem that also has economic consequences. Therefore, economists have a role to play in identifying their potential impact. The influence of childhood background on outcomes can usefully be divided according to the effects of family, school and peers. Schools have been promoted as a potential arena to combat the increasing childhood obesity rate. This study analyses whether increasing physical activity in the school context can contribute to health improvement exploring four different types of interventions. We address the issue using a unique longitudinal data set of 1087 (BMI) and 1047 (fitness) schoolchildren who attend 37 state schools in the Municipality of Aalborg, Denmark. The effect is identified using a randomised experiment that creates an exogenous increase in physical activity. Surprisingly, we find that the interventions did not have the expected impact on schoolchildren's health, and the scant evidence we have points towards a negative effect. We pay particular attention to possible explanations for this lack of results. Our analysis suggests that the interventions had a heterogeneous impact on children's health when only parts of the distribution are considered. Even more convincing are the results, which indicate that schoolchildren engage in compensating behaviour implying a crowding-out of leisure-time activity. Consequently, a supportive cost-benefit case might exist if parental behaviour is assumed to be affected by school-based interventions.

JEL codes: I14, I12, C18

Key words: child health, experimental data, difference-in-differences measure, heterogeneity, crowding-out

1 Introduction

There is a widespread and increasing realisation that childhood obesity and inactivity has become a growing problem. In the Danish context, an increase in childhood overweight and obesity has been observed. The prevalence of overweight among 11–15-year-olds in Denmark increased from 6.9 % for girls and 8.3 % for boys in 1998 to 9.9 % and 12.1 % in 2002, respectively. Similarly, the incidence of obesity has increased. From 2002 to 2010, the occurrence of overweight and obesity has demonstrated a tendency to level off or even decline. The prevalence of overweight was 8.3 % for girls and 11.3 % for boys in 2010. Similarly, the incidence of obesity was between 2 and 3 % (Rasmussen and Due, 2010; Morgen et al., 2013). Despite the flattening incidence of overweight and obesity, the level is still high, which creates concern among policy makers for a number of reasons.

* **Correspondence to:** Annette Quinto Romani, Department of Sociology and Social Work, Aalborg University, 5 Kroghstræde, Aalborg Øst 9220, Denmark, Phone: + 45 9940 8117, E-mail: aqr@socsci.aau.dk

Obese and inactive children are more likely to develop health problems like Type 2 diabetes and high blood pressure. Even if those diseases are not present during childhood, there is an increased risk that they will develop during adulthood. Moreover, obese children are more likely to suffer from psychological problems like depression and poor self-esteem (Daniels, 2006). From a societal point of view, these problems impose a substantial burden on the health care system.

This realization has created a sense of urgency in the implementation of health interventions and the corresponding need for relevant health research. Schools often have been recommended as a potential venue to combat childhood inactivity due to the obvious fact that education is mandatory and that schoolchildren spend a substantial number of their waking hours in school. Advocates of school-based health interventions argue that it can both increase the activity level in general among all schoolchildren and that it can provide more equitable access to physical activity, which may be limited among schoolchildren having a disadvantaged background. However, school-based interventions are expensive, and therefore, an evaluation also needs to take into account any unintended impact that may result in no net increase in schoolchildren's health.

School-based interventions have been researched extensively (for an overview, see Katz, 2009; Kropski et al., 2008). Studies narrowly focusing on interventions directed towards physical activities have been addressed to a lesser extent (for an overview, see Naylor and McKay, 2009; Pate et al., 2006; Trudeau and Shephard, 2005). The central outcomes in these analyses were body mass index (BMI) and body fat (BF) measured by bioelectrical impedance as well as triceps skinfold thickness (TSF). Age- and sex-specific indicators of overweight and obesity were the other main outcomes. A few studies find that school-based interventions reduce children's weight (Gortmaker et al., 1999; Mosuwan et al., 1998; Sallis et al., 2003), but many find no impact (Caballero et al., 2003; Luepker et al., 1996; Sallis et al., 1993). However, the literature also suffers from a number of methodological difficulties. The most serious problem has been the possibility that schools may *self-select* into interventions. Schools with very physically active children may be more apt to participate in the interventions, which may result in an upward bias in the results. In contrast, other schools may try to compensate for inactivity among children, thus resulting in a downward bias of the results (Kropski et al., 2008). Another serious problem is the potential influence of an *omitted variable* that may spuriously arise if randomisation is imperfect. Suppose, for example, that the intervention schools had a majority of schoolchildren with advantaged family backgrounds; in that case, there may be a false conclusion that the interventions have improved child health, leading to incorrect policy implications. As a result, there is a growing interest in studies that attempt to address these two potential sources of an endogeneity problem. A careful reading of the literature reveals that, although randomly assigned experimental studies are the most credible research design when the aim is to measure causal effects, a truly random assignment is difficult to obtain. At best, the selection problem has been solved, but the omitted problem has not. The critical question is whether the randomisation successfully balances child and parental characteristics across different treatment groups. Previous studies considering school-based resources should therefore be viewed as tentative. The first goal of this study is to examine the impact of school-based interventions on schoolchildren's weight and fitness outcomes, while addressing the critical methodological issues that have cast doubt on earlier findings. Our difference-in-differences approach compares health outcome before and after the interventions for schoolchildren attending schools assigned to either intervention or control. The difference-in-differences method is based on the assumption that the trends in the intervention and control schools are the same. The selection bias is eliminated by random assignment.

Nevertheless, imperfect randomization may still be an issue if the sample size is not large enough, as on average there will be a difference in the observed and unobserved characteristics of the intervention and control schools. A possible solution to the omitted variable bias is to control for confounding variables assuming that the unobserved variables are correlated with the observed. Accordingly, we pay particular attention to the potential endogeneity problem by applying various adjusted difference-in-differences methods to address the selection and omitted variable bias.

A second goal of this study is to provide explanations for the lack of results often found when considering school-based interventions. One plausible explanation for the lack of results could be that the average masks important *heterogeneous impacts* (for an overview, see Doak et al., 2006). Most analyses evaluating school-based interventions consider only mean impacts, yet the ‘mean’ may overlook a great deal and may conceal major heterogeneity. Thus, it is relevant to uncover the extent to which some subgroups may benefit more than others in comparison with their initial health status. For example, if the physical cost of exercise is higher for schoolchildren who are already obese, the interventions may have an undesirable impact on the weight of unhealthy children. Another plausible explanation is that schoolchildren engage in *countervailing behaviour* (Dale et al. 2000; Donnelly et al., 1996; Mallam et al., 2003). The implicit assumption, which is true of almost all empirical studies to date, is that leisure-time behaviour is unaffected by school-based resources. If schoolchildren or their parents engage in countervailing behaviour, then an important benefit is ignored in the present calculation as public investment crowds-out private investment. For example, if exposed to demanding physical activities during physical education (PE) classes, parents might spend less time on arranging fitness activities for their children, thereby resulting in an underestimation of the effect of school-based resources.

This study uses data from Project 3A (Activity and Awareness for All), which followed all schoolchildren attending 6th grade (ages 11–13 years) in 2008 in the Danish Municipality of Aalborg. Measures were obtained in 2008 and again in 2010 from a total of 2174 schoolchildren. These data include body mass index (BMI) and physical fitness, and they were measured at the beginning and the end of the programme. Compared to earlier research using this dataset (Quinto Romani, 2011; 2013), the focus is not on the impact of family, but rather, on the potential impact of schools. Hence, this study presents the first findings from Project 3A, that takes advantage of the randomised experiment to determine whether school investment can contribute to health improvement.

This data set provides us with a unique opportunity to examine the effects of four different types of school-based interventions on children’s health status. The interventions: 1) “individual health coaching,” 2) “high-intensity training,” 3) “physical tests,” and 4) “including organised sports,” are all based on physical activity. Using various adjusted difference-in-differences methods, we consistently find that school-based resources have no impact on children’s weight and fitness. Our results, using proper methods, indicate that the school-based interventions did not have a significant impact, and the evidence that exists reveals a negative health impact. Estimates that consider subgroup indicate that the average may mask any important heterogeneous impact. However, a larger scale study is needed to provide more convincing results. In contrast, in considering the impact of leisure time activities, we found clear evidence of a crowding-out effect, implying that the children or their parents appropriate some of the benefit for themselves by engaging in countervailing behaviour.

The rest of the study is laid out as follows: the next section describes the study and the data; Section 3 provides the conceptual and empirical framework; Section 4 provides and interprets the results; and Section 5 discusses the results and provides conclusions.

2 Data

The data used in this study come from a school-based intervention study following randomly selected schoolchildren attending all state schools in the Municipality of Aalborg. Data were collected when the children attended 6th grade (ages 11-13 years) in 2008 and again after 18 months of intervention when they attended 8th grade (ages 13-15 years) in 2010. In an attempt to eliminate the *selection problem*, schools were stratified into upper, middle and lower schools based on average grade marks calculated using the average test scores from five years before,¹ and a random allocation was determined for each stratum. Although stratification based on parental socioeconomic status would be more accurate, these data were not available when the study started. A total of 37 schools were randomly assigned to intervention (25 schools) and control (12 schools) groups. To mimic laboratory conditions as closely as possible, data were collected under almost identical circumstances, that is, at the same date, time and place. Schools in the intervention group were randomly selected for one of four interventions: 1) “individual health coaching,” 2) “high-intensity training,” 3) “physical tests,” and 4) “including organised sports,” but physical activity was at the core of all interventions.

The “**individual health coach**” intervention is a *target* intervention directed individually towards overweight and inactive schoolchildren: girls with a BMI above 22 and fitness below 39; boys with a BMI above 22 and fitness below 43.² The intervention took place *after school*, and the coach, in cooperation with the needy schoolchildren and their parents, sought to raise awareness and to enable these children to change both their exercise and eating habits.

The “**high-intensity training**” intervention is a *universal* intervention that took place *in school*. The intervention is based on the concept of intensity training as a method of improving physical abilities. Schoolchildren selected for this intervention were supposed to exercise at about 75–85% or more of their maximum heart rate for 20 minutes twice a week.

The “**physical test**” intervention is a *universal* intervention that was supposed to examine the potential behavioural consequences of physical tests. The tests took place *in school*, but it was left to the schoolchildren to improve their test scores by practising *after school*. A series of fitness tests was completed with the aim of making the children more aware of their own fitness, but they were not trained in any way.

The “**including organised sports**” intervention is a *target* oriented intervention. It offers sports opportunities and moderate physical activity to schoolchildren who are especially inactive by embedding sports *in school*. The intervention is designed to increase the sports participation of inactive schoolchildren by introducing them to a variety of sports that do not demand any prior sports experience and that are available in their neighbourhoods.

The “**control group**” was divided into a true control group and a control group that received sealed personalised letters containing information about their fitness. The control group was structured in this way to test whether information per se has an impact on the

¹ https://www.cepos.dk/uploads/media/skolekarakterliste_2006.pdf

² BMI is calculated as weight in kilograms divided by height in meters squared (kg/m²). Fitness was assessed as volume of oxygen extracted at exhaustion per kg body mass per minute (ml O₂/kg/min), measured in a watt max test (see Appendix 2 for thresholds).

results. As there were no significant differences between the two control groups, we treated them as one throughout the rest of the study.

The data were collected with permission from the Danish Data Protection Agency. Any publications based on this data protect the anonymity of individuals and schools. The data used in this study include three different types: the *test data*, the *survey data* and the *register data*. They have been linked through a unique personal identification number.

The *test data* include information on the main outcomes of interest: BMI and fitness.³ In addition to estimating the models using BMI expressed logarithmically, we constructed binary indicators for at-risk schoolchildren to uncover any *distributional impact*. “Underweight,” “overweight” and “obesity” in schoolchildren was defined according to the international age- and sex-specific cut-off points as a BMI of 17, 25 and 30 at age 18, respectively⁴ (Cole et al., 2000; Cole et al., 2007). Hence, for example, “obesity” is constructed taking into account the age- and sex-specific cut-off point, implying that a dummy variable equal to one was generated for girls age 11 if BMI was above 25.42 and equal to zero otherwise. Another dummy variable equal to one was generated for girls age 12 if BMI was above 26.67 and equal to zero otherwise, etc. A unique obesity dummy variable was constructed that is equal to one if any of these age- and sex-specific dummies are equal to one, and equal to zero otherwise. As participation in an intervention may increase the BMI of schoolchildren at the lower end of the BMI distribution and decrease the BMI of schoolchildren at the upper end of the distribution, the average BMI may reveal no impact of the intervention, and thus, may mask important but offsetting changes in the BMI distribution. As a result, it is important to explore the *heterogeneous impact*. For fitness, the categories developed by Åstrand (1952), further supplemented by data from Andersen et al. (1987) and Wedderkopp et al. (2004),⁵ were used to define very “good fitness,” “bad fitness” and “very bad fitness” (see Appendix 2 for the thresholds).

The *survey data* include information on eating and exercise habits. It will be used to try to uncover any countervailing behaviour. We capture the children’s participation in physical activity inside and outside of school through a set of dummy variables equal to one if the schoolchild participated in “PE classes” and equal to zero otherwise. “Organized sports” is likewise a dummy variable equal to one if the schoolchild participated and equal to zero otherwise. Similarly, the intensity of afterschool physical activity is measured by a question addressing leisure time activities, in which “intensively active” is a dummy equal to one if the schoolchild practices sports vigorously often during the week and equal to zero otherwise. “Sedentary activity” is a dummy equal to one if the schoolchild spends most of the time watching television, playing computer games, listening to music or in other seated activities and equal to zero otherwise. Further, the time spent on afterschool physical activity is measured by the time spent daily on bicycling, walking and organised sports; “much time” is a dummy equal to one if the schoolchild was active more than one

³ BF is also an outcome; however, this is hard to measure precisely in children.

⁴ The Danish reference values are comparable to those of the Netherlands in the Cole et al. (2000) values, except where the cut-off points for “overweight” (94.5 centile for boys and 93.5 centile for girls) and “obesity” (99.7 centile for boys and girls) lie above the Danish 90th and 99th centile. However, the measure is a conservative measure and is recommended for research purposes, whereas the national references are recommended for clinical purposes (Nysom et al. 2001). The cut-off points for the Netherlands in Cole et al. (2007) include “underweight” (2 centile for boys and 3 centile for girl). No corresponding values exist in the Danish context. The corresponding cut-off points in the US context are the 2th, 85th and 95th cut-off points.

⁵ No international standard exists, but US measures (Heyward, 1998) are likewise below cut-off points for Scandinavians (<http://www.motion-online.dk/konditionstraening/testning/konditalstabel>).

hour a day and equal to zero otherwise. “No time” is a dummy equal to one if the schoolchild is active less than a half an hour a day and equal to zero otherwise.

The *register data* contain information from a variety of administrative registers. The register data include child characteristics, e.g., frequency of early hospitalisation, birth weight and length, and gestational age and parental characteristics, e.g., date of birth, sex, ethnicity, frequency of hospitalisation, their highest completed education measured in number of months, and parental income. This allowed us to control for a substantial number of schoolchild and parental characteristics.

The population included 1638 schoolchildren in 2008 and 1369 in 2010. In 2008, the response rate was 94.4 %, reducing the 2008 sample to 1546 schoolchildren. The response rate in 2010 was 91.4 %, reducing the 2010 sample to 1251 schoolchildren. The sample linked to the register data comprises 1546 schoolchildren in 2008 and 1251 in 2010. We employed a conservative sample procedure including 1087 schoolchildren for whom the information was available in both 2008 and 2010.⁶ When measuring BMI and fitness, it includes, respectively, 1047 and 971 schoolchildren in 2008 and in 2010. We deal with schoolchildren who transferred to another school but stayed in the sample by systematically allocating them to the school they attended in the ‘first wave.’ Missing values are very low and are dealt with by imputing zeros for the missing values and creating dummies to control for the possibility of non-random imputation.

3 Conceptual and empirical framework

Our goal is to determine the relationship between school-based interventions and schoolchildren’s health. The health outcomes are determined by the health production function whose input includes parental background and school-based resources (Grossman, 1972; 2000). Hence, the health production framework includes investment of parental and school-based resources in line with rational choice models. In the framework used in this study, an increase in school health investment enters directly into the production function, increasing the likelihood that the schoolchild will be healthier.

Randomised school-based intervention studies are commonly used to identify the impact of school-based resources. However, none of these studies comes close to matching the most credible and influential randomised experiments, namely, the Perry Preschool Program or the Tennessee STAR experiment. The Perry Preschool Program was a randomised pre-school intervention study in which children were randomly assigned to an intervention that included education and a home visit. Tennessee STAR was a randomised school-based intervention study in which kindergarten children were randomly assigned to small classes, teacher’s aides or regular classes. The uniqueness of these brilliant studies is that they deal with two major problems: the omitted variable bias and the selection problem. A randomised-assigned design, focusing on the exogenous variation in pre-school or school investment, has considerable value in eliminating the *selection bias*. Despite the fact that both the Perry Preschool Program and the STAR experiment have been highly praised and have enjoyed substantial attention from policymakers, a careful reading of these studies reveals that, even here, imperfect randomisation is an issue. Nevertheless, their analyses stand out because they do not neglect imperfect randomisation, and thereby the *omitted variable bias*, but instead attempt to correct the error, controlling for the imbalances by adjusting for these variables. In the Perry Program, randomisation was complicated due to reassignment based on maternal employment and the consequent correlation between treatment assignment and

⁶ The data has been tested for the attrition bias and no significant difference was found.

baseline characteristics (Heckman et al., 2010a). Heckman et al. (2010b) control for this compromised randomisation by adjusting for parental background characteristics and assuming that unobservable variables are correlated with those that are observable. In the STAR project, schoolchildren were randomised within schools, but not across schools. To adjust for the omitted variable bias, Krueger (1999) included school fixed effects. The key advantage of the fixed effect is that it accounts for unobserved heterogeneity at the school level that affects the explained variable, for example, teacher ability.

The current study builds on these studies, as it attempts to solve not only the selection bias, but also the omitted variable bias. In our study, we attempt to deal with the endogeneity problem by applying various difference-in-differences methods adjusted for confounding variables.

From a methodological point of view, most research in this field considers *simple differences*. Here H_{ist} denotes the health outcome of child i , attending school s , at time t . η_{it} defines the mean health in group I , at time t . I indicates whether schools received any intervention ($I = 1$) and if not ($I = 0$), $t=0$ is the pre-intervention period and $t=1$ is the post-intervention period. One way to solve the selection problem is by using the difference in means between children attending schools receiving intervention and those in the control group, $\rho = (\eta_{11} - \eta_{01})$. The assumption making this a valid measure is that there is no difference between the intervention and control schools aside from the interventions. Another way to measure the impact is by using the difference in means before and after $\rho = (\eta_{11} - \eta_{10})$. The assumption making this a valid measure is that there is no difference over time aside from the interventions. These very strong assumptions are rarely fulfilled, e.g., we expect BMI to increase as the children become older. A related but more advantageous way to measure the impact is the *difference-in-differences estimator* $\rho = (\eta_{11} - \eta_{10}) - (\eta_{01} - \eta_{00})$, which is based on the weaker assumption that any changes between the intervention and control schools are a result of the interventions, implying that the trends in health are the same for the intervention and control schools. The first term is the changes in mean for the children attending intervention schools and the second term is the changes in mean for the children attending control schools.

To describe formally how we consider the endogeneity problem, we apply various *adjusted difference-in-differences models*. These methods allow us to control not only for the *selection bias* but also for the *omitted variable bias* caused by observed and unobserved factors related to imperfect randomisation.

The pooled difference-in-differences model is summarised by the following equation:

$$H_{ist} = \beta_0 + \beta_1 I_s + \beta_2 T_t + \rho I_s T_t + \varepsilon_i \quad (1)$$

where H_{ist} denotes the health outcome of schoolchild i , in school s at time t . I_s is a group-indicator equal to 1 if the school was selected for any intervention and equal to 0 if it was selected as a control. T_t is a time-indicator equal to 1 in 2010 and equal to 0 in 2008. This difference-in-differences model implicitly accounts for all school-constant effects as well as all time-constant individual effects. ε_i is the unobserved characteristics of the individual, and here, it is assumed to be independent of group selection and to have the same distribution over time. The coefficient β_1 measures special components of intervention schools that are time-invariant. The coefficient β_2 measures the time component common to both intervention and control schools. The parameter of interest $\rho = (\eta_{11} - \eta_{10}) - (\eta_{01} - \eta_{00})$ is the *difference-in-differences estimator*, which measures the effect of any intervention when controlled for any prior differences (Imbens and Wooldridge, 2009). In a pooled model, the results are not affected by whether or not

the confounding variables are included, as the model implicitly accounts for all time-constant individual effects.

According to Wooldridge (2006) and Imbens and Wooldridge (2009), the above approach based on pooled independent cross-sectional data does not take into account that the same cross-sectional unit appears in each time period. The panel data models based on a *difference-in-differences measure* and the *unconfoundedness measure* are more appropriate. The difference-in-differences model taking advantage of the panel nature of the data can be described as:

$$H_{is1} - H_{is0} = \beta_0 + \pi I_{st} + \beta_1 Z_{is} + \beta_2 X_{is} + \varepsilon_i \quad (2)$$

where H_{is1} and H_{is0} are the health of schoolchild i in school s at time t , $t=0$ represents year 2008 when the schoolchild attended 6th grade, and $t=1$ represents year 2010 when the schoolchild attended 8th grade. I_{st} is a dummy for whether the school was selected for any intervention. $\pi = (\eta_{11} - \eta_{10}) - (\eta_{01} - \eta_{00})$ is the *difference-in-differences estimator*. X_{is} and Z_{is} are vectors of schoolchild and parental characteristics, available only in 2008. ε_i is the idiosyncratic error term. Thus, equation (2) assumes unconfoundedness conditional on X_{is} and Z_{is} . The *difference-in-differences estimator* of equation (2) is similar to equation (1). However, equation (2) allows control variables to affect the change in the health outcome between the two periods.

Alternatively, we directly exploit the panel nature of the data, specifying the relationship in the context of a lagged dependent variable model using the following *unconfoundedness model*:

$$H_{is1} - H_{is0} = \beta_0 + \pi I_{st} + \beta_1 H_{is0} + \beta_2 Z_{is} + \beta_3 X_{is} + \varepsilon_i \quad (3)$$

where the notation is similar, apart from the results conditional on lagged dependent variables $\tau = (\eta_{11} - \eta_{10})$ being the *unconfoundedness measure*, allowing a fee coefficient of η_{i0} . Model (3) is very similar to model (2), except that model (3) considers the specific features of panel data conditional on lagged health status. Thus, equation (2) assumes unconfoundedness conditional on X_{is} and Z_{is} , whereas equation (3) assumes unconfoundedness conditional on X_{is} , Z_{is} and H_{is} . However, model (3) may actually compromise the comparison because H_{is0} may be correlated with ε_i if the idiosyncratic error term is not group and time independent. If H_{is0} is correlated with the unobserved characteristics that determine selection into intervention groups, then the *difference-in-differences model* would be the preferred specification. Whereas if H_{is0} is uncorrelated with the unobserved characteristics that determine selection into intervention groups, then the *unconfoundedness model* would be the preferred specification (Imbens and Wooldridge, 2009; Frisvold and Lumeng, 2011). Although choosing between (2) and (3) is difficult, our preferred estimation method is model (2), which does not include lagged dependent variables, because this would add further complications if correlated with the error term. All models are estimated by ordinary least squares for the log of different health measures. The ordinary least square (OLS) standard errors are corrected for within school correlations using the cluster option.

4 Results

Table 1 provides the descriptive statistics across different intervention groups obtained when the schoolchildren attended 6th grade in 2008. Columns 1 and 2 present the descriptive statistics of those being selected to any intervention or control schools, respectively. Columns 3 to 6 show the descriptive statistics of those being selected to the

independent interventions. The schoolchildren attending intervention schools were significantly worse off with respect to parental socioeconomic background variables than were schoolchildren attending the control schools. In particular, the schools exposed to interventions 3 and 4 had relatively more schoolchildren with low-income parents; furthermore, schools exposed to intervention 3 had significantly more schoolchildren with low-educated and immigrant parents. Our analyses will attempt to surmount the observed differences between groups in a variety of ways. First, all our models are adjusted for confounding variables controlling for child and family factors. Secondly, considering the specific features of panel data conditional on past health, as in model (3), is an effective way to solve the omitted variable bias, as many of the same unobserved factors have contributed to schoolchild health, both in the past and currently (Frisvold and Lumeng, 2011).

Table 1. Sample means of selected variables by control and intervention schools

	I	C	I1	I2	I3	I4
Outcomes						
BMI	18.524 (3.173)	18.388 (3.167)	18.660 (3.003)	18.345 (3.352)	18.755 (3.540)	18.309 (2.701)
Fitness	45.074 (7.681)	45.640 (7.911)	45.534 (7.968)	44.722 (7.663)	44.135* (7.412)	45.945 (7.415)
Parental Characteristics						
Income very low (%)	0.198* (0.399)	0.150 (0.358)	0.183 (0.388)	0.159 (0.366)	0.231** (0.423)	0.238** (0.427)
Income low (%)	0.219** (0.414)	0.159 (0.366)	0.224* (0.418)	0.236** (0.426)	0.231** (0.423)	0.176 (0.382)
Income median (%)	0.194 (0.395)	0.202 (0.402)	0.224 (0.418)	0.183 (0.388)	0.176 (0.382)	0.183 (0.388)
Income high (%)	0.194 (0.395)	0.190 (0.393)	0.183 (0.388)	0.241 (0.429)	0.164 (0.371)	0.176 (0.382)
Income very high (%)	0.192*** (0.394)	0.296 (0.457)	0.183*** (0.388)	0.178*** (0.384)	0.195** (0.397)	0.224 (0.418)
Immigrant (%)	0.075 (0.264)	0.056 (0.232)	0.067 (0.251)	0.062 (0.243)	0.115** (0.321)	0.061 (0.240)
Low education (%)	0.116 (0.320)	0.082 (0.275)	0.089 (0.286)	0.125 (0.332)	0.158** (0.366)	0.095 (0.294)
Median education (%)	0.406 (0.491)	0.336 (0.473)	0.452 (0.498)	0.415 (0.493)	0.353 (0.479)	0.380 (0.487)
High education (%)	0.477*** (0.499)	0.581 (0.494)	0.457*** (0.499)	0.458** (0.499)	0.487 (0.501)	0.523 (0.501)
N	741	346	223	207	164	147

Note: Present means for the sample in 2008. Standard deviations in parentheses. *, **, *** indicate that the difference in means for the control and intervention schools is statistically significant at 10%, 5% and 1% levels, respectively. I is a dummy equal to one if the school was selected for any intervention. C is a dummy equal to one if the school was selected for the control. I1 is a dummy equal to one if the school was selected for the intervention "individual health coaching." I2 is a dummy equal to one if the school was selected for the intervention "high-intensity training." I3 is a dummy equal to one if the school was selected for the intervention "physical test." I4 is a dummy equal to one if the school was selected for the intervention "including organised sports."

We begin by presenting the mean changes in schoolchildren's health. Table 2 summarises the *simple difference* in the means of any intervention and control schools

$\rho = (\eta_{11} - \eta_{10})$ and $\rho = (\eta_{01} - \eta_{00})$. Columns 1 and 2 present the overall impact of being selected for any intervention or control schools. Columns 3 to 6 show the coefficients of the independent interventions. The table clearly indicates that exposure to any intervention reduced not only BMI but also fitness, as compared with the control schools. Among the four types of interventions, the descriptive data indicate that some types of interventions were better at diminishing the weight gain, whereas others were more successful in improving physical fitness. Nonetheless, the interventions in row 3 and 6 are barely significant from the control schools and these disparities may not reflect the true effects as the measures are unadjusted.

Table 2. Pre and post difference in mean by control and intervention schools

	I	C	I1	I2	I3	I4
BMI						
Pre	18.52	18.38	18.66	18.34	18.75	18.30
Post	20.02	19.96	20.06	19.98	20.14	19.89
Dif	1.50	1.58	1.40	1.64	1.39	1.59
Fitness						
Pre	45.07	45.64	45.53	44.72	44.13	45.94
Post	44.59	45.49	45.11	45.28	44.27	43.12
Dif	-0.48*	-0.15	-0.42	0.56	0.14*	-2.82*

Note: Simple difference for intervention and control schools according to the health outcome BMI and fitness measured in level. The coefficients are hardly statistically significant. I is a dummy equal to one if the schoolchild was selected for any intervention. C is a dummy equal to one if the school was selected for the control. I1 is a dummy equal to one if the school was selected for the intervention “individual health coaching.” I2 is a dummy equal to one if the school was selected for the intervention “high-intensity training.” I3 is a dummy equal to one if the school was selected for the intervention “physical test.” I4 is a dummy equal to one if the school was selected for the intervention “including organised sports.” *, **, *** indicate that the coefficients are statistically significant at 10%, 5% and 1% levels, respectively.

Table 3 shows the regression results using different models. The results are adjusted for confounding variables (see Appendix 1). Column 1 presents the overall impact of being selected for any intervention. Columns 2 to 5 show the coefficients of the independent interventions. Panel A presents the results using the pooled difference-in-differences model from equation (1). The results presented in Panel B and C take advantage of the panel nature of the data measuring the panel difference-in-differences model, equation (2), and the unconfoundedness model conditional on lagged health status, equation (3). All results are similar, both in magnitude and impact, as may be expected when the average outcome of the intervention and the control groups are similar in the first period. The results indicate that the school-based interventions did not have the expected impact on schoolchildren’s health, as the results are insignificant. The only intervention that had a significant impact was intervention 4, which reduced fitness by 5.5 %. Throughout the rest of the study, we present only the results from the panel difference-in-differences model, as this seems to be the preferred model.

Table 3. Estimated health effect of the interventions

	I	I1	I2	I3	I4
Panel A. Pooled DID model					
lnBMI	-0.0049 (0.0044)	-0.0095* (0.0057)	0.0015 (0.0064)	-0.0101* (0.0057)	-0.0011 (0.0059)
Lnfit	-0.0094 (0.0160)	-0.0075 (0.0167)	0.0108 (0.0173)	0.0070 (0.0182)	-0.0602** (0.0291)
Panel B. Panel DID model					
lnBMI	-0.0039 (0.0042)	-0.0088* (0.0052)	0.0019 (0.0068)	-0.0081 (0.0060)	-0.0001 (0.0058)
Lnfit	-0.0057 (0.0160)	-0.0037 (0.0164)	0.0128 (0.0176)	0.0109 (0.0176)	-0.0554** (0.0284)
Panel C. Unconfoundedness model					
lnBMI	-0.0042 (0.0039)	-0.0079 (0.0052)	0.0003 (0.0060)	-0.0074 (0.0062)	-0.0018 (0.0052)
Lnfit	-0.0041 (0.0158)	0.0024 (0.0159)	0.0093 (0.0183)	0.0082 (0.0182)	-0.0484** (0.0244)

Notes: Robust standard errors corrected for school cluster are in parentheses. Panel A presents estimates from the *pooled DID model*, see equation (1). Panel B presents estimates from the *panel DID model*, see equation (2). Panel C present estimates from the *unconfoundedness model*, see equation (3). The explained variable of schoolchild health is measured as log BMI and log fitness. All regressions include controls from Appendix 1. *, **, *** indicate that the coefficients are statistically significant at 10%, 5% and 1% levels, respectively. I is a dummy equal to one if the school was selected for any intervention. I1 is a dummy equal to one if the school was selected for the intervention “individual health coaching.” I2 is a dummy equal to one if the school was selected for the intervention “high-intensity training.” I3 is a dummy equal to one if the school was selected for the intervention “physical test.” I4 is a dummy equal to one if the school was selected for the intervention “including organised sports.” Full estimation results based on all models are available upon request.

Having found that the interventions have no impact on schoolchildren’s health, we were concerned that this was due to inaccurate measurements. To check this, we conducted a *robustness check*. One concern was that the lack of impact was due to the receipt of information by part of the control group, which may have clouded the results. Table 4 presents the results when we include only the true control group. The true control group includes 6 schools and 156 schoolchildren. The results are similar to those obtained from the full sample, indicating an insignificant impact. The impact that exists indicates that intervention 4 reduces fitness by 5.5 %. In the rest of the analysis, the combined control group is used.

Table 4. Robustness test – estimated health effect of the interventions on a reduced sample

	I	I1	I2	I3	I4
lnBMI	0.0032 (0.0051)	-0.0013 (0.0064)	0.0093 (0.0073)	-0.0010 (0.0067)	0.0059 (0.0059)
Lnfit	-0.0085 (0.0215)	-0.0034 (0.0233)	0.0128 (0.0238)	0.0110 (0.0236)	-0.0549* (0.0317)

Notes: Robust standard errors corrected for school cluster are in parentheses. All present estimates from the *panel DID model*, see equation (2). The sample is reduced by including only the pure control group, which received no information. The explained variable is schoolchild health measured as log BMI and log fitness. All regressions include controls from Appendix 1. *, **, *** indicate that the coefficients are statistically significant at 10%, 5% and 1% levels, respectively. I is a dummy equal to one if the school was selected for any intervention. I1 is a dummy equal to one if the school was selected for the intervention “individual health coaching.” I2 is a dummy equal to one if the school was selected for the intervention “high-intensity training.” I3 is a dummy equal to one if the school was selected for the intervention “physical test.” I4 is a dummy equal to one if the school was selected for the intervention “including organised sports.” Full estimation results based on all models are available upon request.

A plausible explanation is that the zero effect masks important distributional effects, implying that the interventions are associated with changes in BMI and fitness in only some parts of the distribution. A common way to address such heterogeneity is to perform a sub-sample analysis.⁷ The results are presented in Table 5, which represents the results from the regressions on the tails. Column 1 presents the overall impact of being selected for any intervention. Columns 2 to 5 show the coefficients of the independent interventions. Indeed, our results indicate a substantial heterogeneity.

Exposure to intervention 2 increases the risk of both “under”- and “overweight” by 2.0 and 5.4 percentage points, respectively, just as it increases the probability of “good fitness” by 4.6 percentage points; hence, intervention 2 seems to increase the health gap, especially with regard to weight. Nonetheless, the results are barely significant. Likewise, intervention 3 increases the risk of “underweight” by 2.9 percentage points. In contrast, intervention 4 increases the risk of “bad fitness” and “very bad fitness” by 16.7 and 7.0 percentage points, respectively, causing a negative net health impact. One concern when exploring heterogeneous impact is the sample size⁸, which implies that the data indicating the heterogeneous impact are not very convincing.

Table 5. Estimated health effect of the interventions on the tails

	I	I1	I2	I3	I4
Underweight	0.0183** (0.0087)	0.0152 (0.0123)	0.0201* (0.0122)	0.0291*** (0.0101)	0.0086 (0.0119)
Overweight	0.0271 (0.0180)	-0.0043 (0.0164)	0.0539* (0.0300)	0.0090 (0.0171)	0.0568** (0.0290)
Obesity	0.0072 (0.0088)	0.0207 (0.0167)	0.0074 (0.0114)	-0.0117 (0.0112)	0.0074 (0.0092)
Good fitness	-0.0133 (0.0267)	-0.0316 (0.0243)	0.0462* (0.0278)	-0.0313 (0.0351)	-0.0520 (0.0337)
Bad fitness	0.0561 (0.0460)	0.0673 (0.0472)	0.0350 (0.0507)	-0.0311 (0.0584)	0.1674** (0.0776)
Very bad fitness	0.0179 (0.0242)	0.0024 (0.0349)	0.0179 (0.0343)	-0.0067 (0.0268)	0.0700* (0.0388)

Notes: Robust standard errors corrected for school cluster are in parentheses. All present estimates from the *panel DID model*, see equation (2). The explained variable child health regarding BMI is measured as unity if the child is “underweight,” “overweight,” or “obese.” The explained variable child health regarding fitness is measured as unity if the child’s fitness level is “good fitness,” “bad fitness,” or “very bad fitness.” See Appendix 2 for the thresholds. All regressions include controls from Appendix 1. *, **, *** indicate that the coefficients are statistically significant at 10%, 5% and 1% levels, respectively. I is a dummy equal to one if the school was selected for any intervention. I1 is a dummy equal to one if the school was selected for the intervention “individual health coaching.” I2 is a dummy equal to one if the school was selected for the intervention “high-intensity training.” I3 is a dummy equal to one if the school was selected for the intervention “physical test.” I4 is a dummy equal to one if the school was selected for the intervention “including organised sports.” Full estimation results based on all models are available upon request.

Another plausible mechanism could be that increased activity in schools is crowding-out leisure-time activity. Table 6 shows the results of a range of outcomes, tracking the trade-

⁷ Sub-group mean impact may be imprecise if the within-group variation is substantial compared to the between-group variation (Bitler et al.; 2006). Quantile regression reveals that this is not an issue.

⁸ In 2008 and 2010 the percentage of “underweight” was 3.11 % and 3.20 %; “overweight” 15.46 % and 16.37, “obesity” 2.83 % and 2.37, respectively. Similarly, the percentage of “good fitness” was 10.10 % and 8.63 %; “bad fitness” 28.75 % and 29.53, “very bad fitness” 12.07 % and 11.18 %. Hence, only a limited amount of schoolchildren changed group from 2008 till 2010.

off with leisure-time behaviour. Column 1 presents the overall impact of being selected for any intervention. Columns 2 to 5 show the coefficients of the independent interventions. Panel A indicates the trade-off between in-school and out-of-school participation rates, proxied by participation in “PE classes” and “organised sports,” respectively. Nevertheless, exposure to intervention 4 implies that participation in “PE classes” increases by 6.2 percentage points, but the results are barely significant. Panel B considers the intensity of after-school physical activity, categorised as “intensively active” and “sedentary activity,” respectively. Here the likelihood of being “intensively active” has been reduced by 12.2 and 16.8 percentage points, respectively, among schoolchildren exposed to interventions 2 and 4. Further, intervention 2 implies that schoolchildren increase the time spent on “sedentary activity” by 7.2 percentage points. Panel C considers the time spent on physical activity categorised as “no time” and “much time,” respectively. Results reveal that exposure to intervention 2 increases the likelihood of spending “no time” on leisure-time physical activity by 6.2 percentage points.⁹ We can conclude that increasing physical activity “in school” crowds-out physical activity “out of school.”

Table 6. Estimated effect of the interventions on leisure-time behaviour

	I	I1	I2	I3	I4
Panel A. Participation in PE classes and organised sports					
PE classes	0.0011 (0.0183)	-0.0164 (0.0218)	-0.0331 (0.0230)	0.0137 (0.0173)	0.0622* (0.0336)
Organised sports	-0.0136 (0.0325)	-0.0207 (0.0523)	0.0184 (0.0388)	-0.0195 (0.0489)	-0.0415 (0.0394)
Panel B. The intensity of afterschool physical activity					
Intensively active	-0.0997** (0.0456)	-0.0997* (0.0538)	-0.1217** (0.0558)	-0.0087 (0.0588)	-0.1678*** (0.0565)
Sedentary activity	0.0388 (0.0256)	0.0539 (0.0354)	0.0720** (0.0310)	-0.0339 (0.0379)	0.0483 (0.0370)
Panel C. The time spent on afterschool physical activity					
No time	0.0215 (0.0265)	0.0448 (0.0294)	0.0621** (0.0323)	-0.0023 (0.0448)	-0.0447 (0.0467)
Much time	-0.0513 (0.0412)	-0.0403 (0.0514)	-0.0786 (0.0541)	-0.0986* (0.0555)	0.0215 (0.0769)

Notes: Robust standard errors corrected for school cluster are in parentheses. All present estimates from the *panel DID model*, see equation (2). Panel A presents the estimated effect of the interventions on participation in physical activity categorised as “organised sports” and “PE classes” (1). Panel B presents the estimated effect of the interventions on the intensity of afterschool physical activity categorised as “intensively active” and “sedentary active.” Panel C presents the estimated effect of the interventions on the time spent on afterschool physical activity categorised as “no time “ and “much time.” All regressions include controls from Appendix 1. *, **, *** indicate that the coefficients are statistically significant at 10%, 5% and 1% levels, respectively. I is a dummy equal to one if the school was selected for any intervention. I1 is a dummy equal to one if the school was selected for the intervention “individual health coaching.” I2 is a dummy equal to one if the school was selected for the intervention “high-intensity training.” I3 is a dummy equal to one if the schoolchild was selected for the intervention “physical test.” I4 is a dummy equal to one if the school was selected for the intervention “including organised sports.” Full estimation results based on all models are available upon request.

⁹ Similar regressions have been run considering the impact of increasing physical activity on both the frequency of eating food and on the density of food. Most results indicated no significant relationship.

5 Conclusion

The latest debate about childhood obesity has contributed to the realisation that the economic future of the Danish healthcare system is highly dependent on resources invested during childhood. Nevertheless, much previous research finds that there is no systematic and significant relationship between the level of school-based health resources and schoolchildren's health outcomes. Consequently, a number of observers have cast doubt on the effectiveness of school-based resources as a policy tool to combat childhood obesity. The findings in this study are relevant to the debate about whether schools are a viable platform for improving schoolchildren's health.

A health production framework is applied in which school investment is supposed to have a beneficial health impact for two reasons. First, it improves schoolchildren's overall health, contributing beneficially to their long-term development. Second, it is supposed to reduce the impact of parental background on schoolchildren's health by compensating for the lack of parental resources, thereby levelling the field. In this study, we seek to understand the relationship between school-based health interventions and schoolchildren's weight and fitness outcomes. Taken as a whole, our findings indicate that the interventions did not have a significant impact, and the evidence that exists reveals a negative health effect. Our findings lead to the conclusions discussed below.

One plausible reason for this lack of results could be the distribution effect, which implies that the average impact masks an important *heterogeneous impact*. Analyses of the tails reveal that the interventions have mixed impacts on schoolchildren in the lower and upper parts of the health distributions. In particular, intervention 2 is associated with a greater likelihood of being both "underweight" and "overweight," and the probability of "good fitness," while intervention 4 reduces the likelihood of "good fitness" and increases the risk of "bad fitness." The unintended impact on the lower and upper parts of the health distribution may well indicate that the interventions contribute to increasing health polarisation. The estimated heterogeneity in the intervention effect could indicate that the rational choice depends endogenously on, for example, the cost of a healthy lifestyle being higher for obese schoolchildren or the benefits in terms of social approval being heterogeneous, in line with the claims of the relative risk aversion theory. Nevertheless, considering the heterogeneous effects, the results may be due to a small sample size. A larger scaled study is needed to provide more solid evidence of this thesis.

Another reason for this lack of results could be that schoolchildren engage in *compensating activities*. Our findings indicate that schoolchildren respond to increasing physical activity in school with modest crowding-out. As physical activity in and out of school are close substitutes, an expansion of physical activity in school has proven to imply a reduction in out-of-school physical activity, diluting the effectiveness of school-based intervention directed towards improving schoolchildren's health. If parents are capturing some of the benefits of school-based investment for themselves by allowing countervailing behaviour, then this important benefit which often is ignored and it should be included in the calculations. If parental time invested in producing health capital has declined during the intervention, this could explain the absence of a strong correlation between school-based input and children's health outcomes. Thus, further research in this field should aim to present a cost-benefit analysis of school-based interventions that incorporates countervailing parental behavior, using the family as the unit for analysis.

Acknowledgement

This study was supported by Aalborg University, the Danish Health Insurance Foundation (grant number 2008A003), the Danish TrygFonden Foundation (grant number 5556-07) and the Danish Obelske Familiefond Foundation (grant number 12664). We thank the Danish Gymnastics and Sports Associations (DGI) in Northern Jutland and the Danish Municipality of Aalborg for financial support and for collecting the data.

The author thanks Professor Lisbeth B. Knudsen, Associate Professor Trond Beldo Klausen and Professor Michael S. Dahl (Aalborg University); Professor Mads M. Jæger (Aarhus University); and Assistant Professor Chris Herbst (Arizona State University) for helpful comments.

References

- Andersen, L. B., Henckel, P. and Saltin, B. (1987). Maximal oxygen uptake in Danish adolescents 16-19 years of age. *European Journal of Applied Physiology*, 56(1), 74-82.
- Bitler, M. P., Gelbach, J. B. and Hoynes, H. W. (2006). What mean impacts miss: Distributional effects of welfare reform experiments. *American Economic Review*, 96(4), 988-1012.
- Caballero, B., Clay, T., Davis, S. M., Ethelbah, B., Rock, B. H., Lohman, T., Norman, J., Story, M., Stone, E. J., Stephenson, L. and Stevens, J. (2003). Pathways: A school-based, randomized controlled trail for the prevention of obesity in American Indian schoolchildren. *American Journal of Clinical Nutrition*, 78(5), 1030-1038.
- Cole, T. J., Bellizzi, M. C., Flegal, K. M. and Dietz, W. H. (2000). Establishing a standard definition for child overweight and obesity worldwide: International survey. *British Medical Journal*, 320(7244), 1240-1243.
- Cole, T. J., Flegal, K. M., Nicholls, D. and Jackson, A. A. (2007). Body mass index cut offs to define thinness in children and adolescents: International survey. *British Medical Journal*, 335(7612), 1-8.
- Dale, D., Corbin, C. B. and Dale, K. S. (2000). Restricting opportunities to be active during school time: Do children compensate by increasing physical activity levels after school? *Research Quarterly for Exercise and Sport*, 71(3), 240-248.
- Daniels, S. R. (2006). The consequences of childhood overweight and obesity. *Future Child*, 16(1), 47-67.
- Doak, C. M., Visscher, T. L., Renders, C. M. and Seidell, J. C. (2006). The prevention of overweight and obesity in children and adolescents: A review of interventions and programmes. *Obesity Reviews*, 7(1), 111-136.
- Donnelly, J. E., Jacobsen, D. J., Whatley, J. E., Hill, J. O., Swift, L. L., Cherrington, A., Polk, B., Tran, Z. V. and Reed, G. (1996). Nutrition and physical activity program to attenuate obesity and promote physical and metabolic fitness in elementary school children. *Obesity Research*, 4(3), 229-243.
- Frisvold, D. E. and Lumeng, J. C. (2011). Expanding exposure: Can increasing the daily duration of Head Start reduce childhood obesity? *Journal of Human Resources*, 46(2), 373-402.
- Gortmaker, S. L., Peterson, K., Wiecha, J., Sobol, A. M., Dixit, S., Fox, M. K. and Laird, N. (1999). Reducing obesity via a school-based interdisciplinary intervention among youth: Planet Health. *Archives of Pediatrics & Adolescent Medicine*, 153(4), 409-418.
- Grossman, M. (1972). On the concept of health capital and the demand for health. *The Journal of Political Economy*, 80(2), 223-255.
- Grossman, M. (2000). The human capital model of the demand for health. *NBER Working Paper*, 7078, 1-98.
- Heckman, J. J., Moon, S. H., Pinto, R., Savelyev, P. A. and Yavitz, A. (2010a). The rate of return to the High/Scope Perry Preschool Program. *Journal of Public Economic*, 94(1-2), 114-128.
- Heckman, J. J., Moon, S. H., Pinto, R., Savelyev, P. A. and Yavitz, A. (2010b). Analyzing social experiments as implemented: A reexamination of the evidence from the HighScope Perry Preschool Program. *Quantitative Economics*, 1(1), 1-46.

- Heyward, V. H. (1998). *Advanced fitness assessment and exercise prescription* (6th ed). Champaign, IL: Human Kinetics.
- Imbens, G. W. and Wooldridge, J. M. (2009). Recent developments in the econometrics of program evaluation. *Journal of Economic Literature*, 47(1), 5-86.
- Katz, D. L. (2009). School-based interventions for health promotion and weight control: Not just waiting on the world to change. *The Annual Review of Public Health*, 30, 253-272.
- Kropfski, J. A., Keckley, P. H. and Jensen, G. L. (2008). School-based obesity prevention programs: An evidence-based review. *Obesity*, 16(5), 1009-1018.
- Krueger, A. B. (1999). Experimental estimates of education production functions. *The Quarterly Journal of Economics*, 114(2), 497-532.
- Luepker, R. V., Perry, C. L., McKinlay, S. M., Nader, P. R., Parcel, G. S., Stone, E. J., Webber, L. S., Elder, J. P., Feldman, H. A. and Johnson, C. C. (1996). Outcomes of a field trial to improve children's dietary patterns and physical activity. The Child and Adolescent Trial of Cardiovascular Health (CATCH). *Journal of the American Medical Association*, 275(10), 768-776.
- Mallam, K. M., Metcalf, B. S., Kirkby, J., Voss, L. D. and Wilkin, T. J. (2003). Contribution of timetabled physical education to total physical activity in primary school children: Cross sectional study. *British Medical Journal*, 327(7415), 592-593.
- Morgen, C. S., Rokholm, B., Brixval, C. S., Anderen, C. S., Andersen, L. G., Rasmussen, M., Andersen, A. N., Due, P. and Sørensen, T. I. A. (2013). Trends in prevalence of overweight and obesity in Danish infants, children and adolescents – Are we still on a plateau?. *PLoS ONE*, DOI:10.1371/journal.pone.0069860.
- Mo-suwan, L., Pongprapai, S., Junjana, C. and Puetpaiboon, A. (1998). Effects of a controlled trail of a school-based exercise program on the obesity indexes of preschool children. *American Journal of Clinical Nutrition*, 68(5), 1006-1011.
- Naylor, P. J. and McKay, H. A. (2009). Prevention in the first place: Schools a setting for action on physical inactivity. *British Journal of Sports Medicine*, 43(1), 10-13.
- Nysom, K., Mølgaard, C., Hutchings, B. and Michaelsen, K. F. (2001). Body mass index of 0 to 45-y-old Danes: Reference values and comparison with published European reference values. *International Journal of Obesity*, 25(2), 177-184.
- Pate, R. R., Davis, M. G., Robinson, T. N., Stone, E. J., McKenzie, T. L. and Young, J. C. (2006). Promoting physical activity in children and youth: A leadership role for schools. *Circulation*, 114(11), 1214-1224.
- Quinto Romani, A. (2011). Children's weight and participation in organized sports. *Scandinavian Journal of Public Health*, 39(7), 687-695.
- Quinto Romani, A. (2013). Parental socioeconomic background and child behaviour. *Social Indicator Research*, DOI 10.1007/s11205-013-0281-3.
- Rasmussen, M. and Due, P. (2010). Health Behaviour in School-aged Children (HBSC) 2010.
- Sallis, J. F., McKenzie, T. L., Alcaraz, J. E., Kolody, B., Hovell, M. F. and Nader, P. R. (1993). Project SPARK. Effects of physical education on adiposity in children. *Annals of the New York Academy of Sciences*, 699, 127-136.
- Sallis, J. F., McKenzie, T. L., Conway, T. L., Elder, J. P., Prochaska, J. J., Brown, M., Zive, M. M., Marshall, S. J. and Alcaraz, J. E. (2003). Environmental interventions for eating and

- physical activity: A randomized controlled trial in middle schools. *The American Journal of Preventive Medicine*, 24(3), 209-217.
- Trudeau, F. and Shephard, R. J. (2005). Contribution of school programmes to physical activity levels and attitudes in children and adults. *Sports Medicine*, 35(2), 89-105.
- Wedderkopp, N., Froberg, K., Hansen, H. S. and Andersen, L. B. (2004). Secular trends in physical fitness and obesity in Danish 9-year-old girls and boys: Odense School Child Study and Danish substudy of the European Youth Heart Study. *Scandinavian Journal of Medicine and Science in Sport*, 14(3), 150-155.
- Wooldridge, J. M. (2006). *Introductory econometrics. A modern approach*. Thomson Higher Education. Mason: OH.
- Åstrand, P. O. (1952). *Experimental studies of physical working capacity in relation to sex and age*. Munksgaard: Copenhagen.
- https://www.cepos.dk/uploads/media/skolekarakterliste_2006.pdf [1 November 2008].
- <http://www.motion-online.dk/konditionstraening/testning/konditalstabel> [1 April 2010].