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UNIVERSITY OF CAPE TOWN

MASTERS DISSERTATION

Evaluating the South African National School Nutrition Programme

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Abstract

This paper examines the effects of the South African National School Nutrition Programme on nutritional outcomes using a regression discontinuity design applied to the first wave of the National Income Dynamic Study. There is tentative evidence to suggest that the programme has a positive effect on weight-for-age and BMI-for-age z-scores, but data constraints necessitating the employment of a proxy selection variable and potentially unobserved discontinuity in other variables around the cut-off call into question the validity of the identification strategy. As such, these results should be interpreted with caution. The paper also serves to communicate ideas for identification strategies and estimation techniques that are conditional on the imminent release of new data, and also aims to communicate relevant aspects of funding and nutrition policy that could inform further attempts at rigorously investigating the National School Nutrition Programme.

February 10, 2012

1 Introduction

Poor educational and nutritional outcomes are a serious impediment to economic growth and undermine legislatively enshrined national objectives of social inequality redress, wealth redistribution and access to basic human rights. One of the measures adopted by the South African government, with the aim of combating these outcomes, is the adoption of the National School Nutrition Programme (NSNP). The NSNP provides school-based meals and encourages participants to learn about nutrition and sustainable food cultivation. The basic idea is that school-based feeding programmes increase alertness and the ability to concentrate in class by giving learners the energy they need to do so, improves enrolment and attendance as children are incentivised to obtain their free daily meal, or some combination of the two.

The NSNP draws considerable funding in South Africa, despite little hard evidence of its impact on nutrition, educational achievement or school participation. This paper has multiple components which coalesce towards the ultimate goal of rigorously assessing the NSNP in the broadest sense. To clarify, the aim of this paper is not solely to provide some set of estimates that relate evidence as on the effectiveness of the programme, but to provide information on the structure of the policy, the dimensions of the treatment, potential identification strategy pitfalls and ideas for further analysis pending the release of more data. This paper also includes its own analysis of the programme, which employs a regression discontinuity design (RDD) approach. The results of this analysis tentatively suggest that the programme has a positive, weighted-average intention-to-treat effect on weight-for-age and BMI-for-age z-scores across all individuals in a sample of primary school aged children.

That paper commences as follows. Section 2 relates the history of the nutrition programme and draws attention to differences between the modern conception of the school based intervention and its ancestor, the PSNP. This part of the paper contains information on the nuances of the policy that would be of interest to researchers looking to investigate the programme econometrically, as it contains details on policy nuances that could be informative in the construction of identification strategies. Section 3 highlights the need to investigate the efficacy of NSNP by communicating results that suggests the persistence of undernutrition and low educational achievement, despite ever increasing investment in policy designed to combat these problems. It also summarises the hypothesised benefits of school feeding programmes and past research into similar programmes elsewhere.

The paper goes on, in Section 4, to summarise the basics of the RDD, provides information on the data used (which is drawn from the National Income Dynamic Study, NIDS) and includes further information on identification strategy. Section 5 contains the results of our RDD estimation and interprets these results. Finally, Section 6 discusses those results in light of the potential shortcomings of the identification strategy employed and offers several ideas for further research pending the release of more data. Section 7 concludes.

2 History of Interacting Educational and Nutritional Policy

2.1 The Early Years

The South African Primary School Nutrition Programme (PSNP) was first announced in May 1994 and implemented for the first time in September of the same year. The first version of the programme was targeted at achieving the dual developmental objectives of improving the health status and improving educational outcomes of South African children. It was defined as a component of the Integrated Nutrition Programme which was, in turn, defined under the broad purview of the national Reconstruction and Development Programme (RDP) (McCoy *et al.*, 1997; Labadarios *et al.*, 2005). The Department of Health (DoH) held the principle responsibility of administrating the programme, which included the design of targeting, nutritional, and implementation guidelines that were then communicated to provincial administrations (Kallmann, 2005). Provincial departments were then responsible for procurement systems and specific menu options (Meaker, 2008).

The initial version of the policy was influenced by data suggesting pervasive undernutrition amongst South African children by illuminating the nature and scale of the shortfall in the provision of basic human rights¹. In an anthropometric survey (the Department of National Health and Population Development (1994)) of primary school children ($n = 97,790$), 9.0% were classified as underweight², 2.6% wasted³ and 13.2% stunted⁴. Not surprisingly, these nutritional problems were exaggerated within previously disadvantaged racial strata (McCoy *et al.*, 1997, p. 7).

The decision to introduce a *school-based* nutrition programme, given these descriptive data, was heavily informed by the World Bank's 1993 World Development Report. The document identified school-based health programmes as a cost effective method of achieving the broad objective of improving disability-adjusted life years (DALYs)⁵ (The World Bank, 1993). Particular emphasis was placed on controlling parasitic worm infections, family planning, nutrition, drug and alcohol abuse education, improving the environment, HIV/AIDs prevention and life skills education. Of all of these interventions, as well as the numerous other peripheral interventions advocated by UNICEF, the United Nations Development Programme, and other World Bank sources, application of deworm-

¹In particular, sections 27, 28, and 29 of the Bill of Rights in the South African Constitution, which stipulate the inalienable rights of each citizen of South Africa to health care, food, water and social security (Republic of South Africa, 1996; Public Service Commission, 2008)

²Defined as more than two standard deviations below median in a healthy population, based on weight-for-age, and implies a mix of chronic and acute malnutrition

³Defined as more than two standard deviations below median in a healthy population, based on weight-for-height, and implies nutritional stress

⁴Defined as more than two standard deviations below median in a healthy population, based on height-for-age, and implies chronic undernutrition

⁵The DALY represents a measure that expresses years of life lost due to ill-health, disability or early death (Murray & Lopez, 1997)

ing medication and micronutrient supplementation (particularly iron, vitamin A and iodine) were identified as the most cost effective (McCoy *et al.*, 1997, p. 22-23). These recommendations were explicitly included as official strategic objectives within the PSNP which include

- the expansion of learning capacity, school attendance and punctuality through the provision of an early morning snack containing at least 25% of the recommended daily allowance (RDA) of energy for 7-10 year olds, and at least 20% of the RDA of energy for 11-14 year olds
- provision of micronutrient supplementation
- facilitation of parasite control/parasite eradication
- education of learners on health and nutrition

2.2 Early Evaluations of the Programme

The PSNP received criticism from several evaluations in its adolescence. The most notable of these was a qualitative national study of 149 schools by Louw *et al.* (2001), which was commissioned by the directorate of nutrition (Labadaros *et al.*, 2005). The study aimed to assess the school feeding programme “in terms of targeting, coverage, menu options, cost effectiveness, and food quality and safety (Labadaros *et al.*, 2005, p. 103).” The most notable shortfall identified by Louw *et al.* (2001) was the apparent “dilution” caused by the targeting strategies employed. Provinces were politically motivated to target as many schools as possible and, in doing so, were forced to spread resources thinly across included schools. This resulted an underprovision of meals, both in terms of quantity and quality. School meals were only provided on 80% of school days in six of the nine provinces, and in eight provinces those meals provided less than the 20% of the RDA - short of the programme goal of 25%. The main recommendation of Louw *et al.* (2001) was to transfer control of the programme from the DoH to the DoE, as “the DoH was implementing a program in an environment in which it had a limited mandate (Labadaros *et al.*, 2005).”

To expand on this, the DoH was constructing fairly complex operational guidelines, that were then open to interpretation by the individual provincial departments. Critically, there was no national directive determining the targeting priorities employed by each of the provinces⁶ (Kallmann, 2005). Despite this (and several other problems), there were some anecdotal reports gleaned from the investigation which suggested that the programme had made some positive contribution. Children were reported as being more alert and seemed to enjoy learning/cognitive benefits as a result of the programme.

⁶These divergent targeting priorities are summarised in Table 3 (Appendix A)

2.3 Transition

In April 2004, the PSNP was renamed the NSNP and the DoE assumed control of the programme. This crossover marked a shift in focus from the broad objectives of the PSNP, which explicitly included improving micronutritional outcomes and parasite control amongst its objectives, to a policy that was aimed squarely at improving educational outcomes through protein-energy based nutritional interventions. The list of objectives was whittled down considerably as a result, and the NSNP now identifies its objectives as

- improving learning capacity
- promoting self-supporting school gardens
- promoting healthy lifestyles among learners (Republic of South Africa, 2009)

Despite discarding the explicit micronutritional objectives of the PSNP in its official set of objectives, the NSNP still pays some lip-service to micronutritional improvement as a desired objective of the programme in policy documents and reports detailing its progress. What is also clear, from these reports, is that school meals, as recently as 2008, are not provided in any uniform way. Furthermore, the administrative and capacity constraints also seem to differ between various provinces and local governments, creating variation in the extent to which the programme is rolled out. To provide an example of this variability,

Eastern Cape, Meals: *“The provincial department served only an uncooked menu (bread menu). However, from July 2008, the PED initiated a pilot project of a cooked menu in 230 schools, targeting 10 schools per District. This was progressively increased to 2 031 schools by the end of the 2008/09 financial year. In assessment of the current menu, significant changes will be necessary in line with prescribed menu options with a variety of food including fruit and vegetables (Republic of South Africa, 2008, p. 15).”*

Western Cape, Meals: *“The PED provided 4 cooked and 1 uncooked meal per week in line with the prescribed menu specifications. However there is still room for improvements in providing a variety of protein sources such as pilchards or milk as provided in specifications. Dried vegetables can be substituted with fresh vegetables because of the higher nutritional value (Republic of South Africa, 2008, p. 41).”*

These quotes are drawn from reports from each of the PEDs provided to the central administrators of the NSNP. These reports vary considerably between *all* PEDs not only in terms of the nature of the content, but the extent to which the department communicates any details on the particulars of their roll-out process. This means that provinces are not only likely to be different, but we will be unable to check or account for those differences in some cases. This heterogeneous treatment has implications for the interpretation of the estimated effects of the programme in the econometric assessment conducted in the latter part of this paper.

The targeting strategy of the PSNP was also altered considerably after transition. The previously diverse set of province specific targeting priorities, outlined in Table 3, have been simplified. The programme now extends to all schools identified as being within certain quintiles (as of early 2012, quintiles one to three in both secondary and primary schools). The way in which these schools are assigned to these quintiles is detailed in the following section.

2.4 Assigning Schools to Quintiles, the Poverty Score, and the National Norms and Standards for School Funding Act (NNSSF)

The quintile assignment procedure is guided by a set of directives outlined within the National Norms and Standards for School Funding Act (NNSSF). Under the NNSSF, originally gazetted in 1998 as a component of the South African Schools Act (SASA), implemented in 2000, and amended on numerous occasions subsequently, each provincial education department (PED) is required to compile a resource targeting list of schools which is determined by the poverty levels of the community each school serves. PEDs do this by assigning a poverty score to each electoral ward which allows them to rank those wards from poorest to least poor. Resources are then progressively divided based on how that ranking assigns those wards into quintiles. The directives for the calculation of the poverty score are determined at the national level. These rankings are used to assign funding and to determine their eligibility for certain pro-poor policies, amongst which is the NSNP. Schools in the bottom three quintiles have been designated “no fee schools” as of the 1st of January, 2012 (Republic of South Africa, Department of Education, 2011).

Previously, the NNSSF allowed PEDs to create their own poverty indices that were then used to place individual schools into quintiles. The only financial benchmark imposed was that the allocations to learners in the poorest quintile must be seven times larger than the allocation to those learners in the wealthiest one (Wilderman, 2008). Apart from that, provincial departments were largely independent in terms of their choice of funding allocations between groups.

The policy, as originally conceived, was problematic in that it led to uneven fund allocation rules and amounts between provinces. Some learners that were classified as non-poor within certain provinces were receiving more money than those classified as poor in others. This is because each province was allowed to

set varying funding ratios between quintiles (within the bounds of the lone financial benchmark rule). This led to situations in which two learners in different provinces, who were identical under some other, uniformly applied poverty scale, were assigned completely different funding allocations. The non-standardisation of quintile assignment and quintile fund allocation made the initial incarnation of the NNSF particularly ill-suited to satisfying the objectives of redress and redistribution (in the broadest sense) embodied within its parent policy, the SASA. These problems of inter-provincial inequality were further exacerbated by large differences in the finance capacity between PEDs (Wilderman, 2008).

The NNSF received heavy criticism for these shortfalls and, as a result, a review of the funding policy was commissioned in 2003. The fundamental concluding recommendation was that school funding norms followed a national targeting list, and that there be some drive towards ensuring that the bottom two school quintiles were exempt from school fees.

The amended NNSF attempted to internalise the recommendations in the DBE's "Plan of Action" that was created in response to the 2003 review. Most notably, the DBE now provides a set of centrally determined principles governing the determination of the school poverty score. This score is based around the relative poverty of the surrounding community. The determination of this measure may employ census or household survey data (from Statistics South Africa) and is based on income, dependency ratio (unemployment) and education levels of the community (literacy rate). In practice, the determination of poverty scores is done per electoral ward, which are observable in the non-anonymised version of South African census data. Variables upon which these poverty scores are calculated are also extracted from that data set. The amended NNSF recommends that the basic methods used to determine this score be defined nationally in order to facilitate equal treatment of schools that are within different provinces (Republic of South Africa, Department of Education, 2006).

There are a few provisos under which PEDs may deviate from the principles of poverty score calculation outlined above

1. If there are not enough places available in local schools, and the PED has determined that the community should make use of schools further away.
2. Cases in which local schools are struggling to meet some minimum objective in terms of quality of teaching and learning to the extent that less-poor parents are advised to send their children to schools elsewhere. These conditions can either arise due to acute or chronic issues within the first-best local school and the suggestion, much like the capacity proviso outlined previously, hinges on parents sending children to schools outside their community (Republic of South Africa, Department of Education, 2006).

There are good reasons for including this discretionary component of the quintile allocation procedure. First, is that schools within electoral wards with high levels of heterogeneity may appear to be better off than they actually are. Inequality within wards would raise their mean socioeconomic status, and

the poorer parents (who send their children to local schools) would perhaps miss out on certain progressive policies because schools within their ward were misallocated to higher quintiles. Second, if schools in less-poor wards receive an influx of students from a less-poor ward (because of capacity issues in the ward from which those children originate, for example) then those learners would “miss out” on certain treatments and funding allocations (Wilderman, 2008; Republic of South Africa, Department of Education, 2006).

There is clearly tension between having a transparent, replicable process of assigning schools to quintiles (on a national level) and ensuring that there is sufficient manoeuvrability to achieve the intended outcome of progressive fund allocation. The myriad idiosyncratic characteristics specific to particular communities, and the households within those communities, mean that it is impossible to simply eschew any allowance for exceptional circumstance, but means the environment forbids any kind of truly uniform assignment rule.

This is made all the more complicated by the existence of no-fee schools, which were introduced in 2007. No-fee schools are those that fall into the bottom three quintiles (or the first two quintiles, before January 1st, 2012). These schools attract the best funding in terms of school safety, nutrition, classroom construction and Grade R expansion (Wilderman, 2008, p. 6). Following the introduction of these schools, there was increased competition to be designated within the bottom two quintiles. Schools that are just on the “wrong side” of the eligibility cutoff suffer adverse consequences as a result of this demarcation. To be slightly more succinct, “demarcation changes, the establishment of new schools, and re-ranking following successful appeals, have pushed out many poor schools into the less-poor poverty quintiles (Wilderman, 2008, p. 6).” One would expect that the efficacy of ward administration (at least in terms of capturing national resource assignments) and the ability to adjure provincial education departments to define schools within lower quintiles would be correlated with one another to some extent. This is not to say that the appeal procedure is corrupt or nepotistic. Rather, the discretionary nature of the allocation means that differences in ability, in terms of bureaucratic aptitude, rhetoric etc., apply to a variety of things that could render the provision of public goods within a ward more or less successful. As such, it is possible that those schools that are *just* ineligible for quintile three status (or quintile two status before 2012) would be disadvantaged in other ways beyond lower non-adjustable per-learner fund allocations. That is, there could be some *selection effect* on an electoral ward basis - wards that get one type of social grant might be more likely to also be in school quintiles that receive special funding (no-fees, NSNP, and others). The discretionary approach could also encourage some gaming of the system. Schools in wards with some political association to those responsible for the discretionary allocation of schools into quintiles could stand to benefit politically by ensuring their schools are defined in a way that perpetuates the receipt of funds. This paper remains agnostic on whether or not this actually occurs, but the discretionary proviso must be identified as it has strong implications on the choice of optimal identification strategy for assessing the NSNP, or any other programme based on these assignments. Summarily, it is possible that

the combination of discretionary allocation, and the incentives to be included in lower quintile communities created by well-meaning, progressive policy (that is targeted on that basis) lead to perverse outcomes that subvert the transformative objectives of those policies. Any attempts at assessing a South African, school-based, social programme which is based on quintile assignment need to be interpreted in this light.

It is also important to note that there is space in the NNSSF dedicated to the issues of fee exemptions. Parents may be exempted from paying school fees, by their School Governing Body (SGB), for a variety of reasons. Automatic exemptions are given to children that are either orphaned or abandoned, or receive any other kind of poverty-linked social grant. The calculation for total or partial exemption for parents is slightly more complicated. These conditional exemptions are informed by two principles in particular. First, the income threshold below which parents qualify for *total* exemption should be the same within schools, irrespective of the number of children for which those parents are charged school fees. This is so that it is impossible for similar parents to have differing exemption status within a school. Second, *partial* exemption *should* depend partly on the number of learners that parent supports. SGBs are also given license to offer discretionary exemptions, provided that the process is transparent and equitable (Republic of South Africa, Department of Education, 2006, p. 45).

3 Literature Review

3.1 The Persistence of Undernutrition and Low Educational Attainment

The same problems of undernutrition in primary school children that originally motivated the creation of the PSNP are still of concern now. Table 4⁷ summarises the proportion of children that were undernourished according to various criteria, the details of which are summarised in Table 1 below. These data were drawn from the first wave of the National Income Dynamic Study (NIDS) which was conducted throughout 2008 (although primarily in the first half of the year). The results are alarming, as they suggest that child undernutrition problems were either the same or worse in 2008 as they were prior to the creation of the PSNP⁸.

The racial and class stratification of undernutrition, particularly stunting, persist⁹. Given that public spending on education represents roughly 20% of the

⁷In Appendix B

⁸As reflected in data like the Department of National Health and Population Development survey, conducted in 1994 (Department of National Health and Population Development, 1994), and the Project for Statistics on Living Standards and Development (PSLSD) integrated household survey, conducted in 1993 (Ardington & Case, 2008)

⁹See Table 4 and Figures 5-7 in Appendix B, which provide descriptive statistics and the kernel density functions of the anthropometric measures by race and income quintile respectively

Table 1: Common anthropometric measures *Source: Cogill (2003)*

Anthropometric Measure	Term attributed to dangerously low z-scores¹	Description
Weight-for-height	Wasted	Good for measuring acute malnutrition, low weight-for-height z-scores are a good indicator of inadequate food intake, poor feeding practices, disease and infections or a combination of these.
Weight-for-age	Underweight	A mixed measure of both chronic and acute malnutrition
Height-for-age	Stunted	A measure of chronic malnutrition. Stunting may be irreversible in children above two years of age and is associated with chronic, long-term malnutrition, infection, micronutrient deficiency

¹ More than two standard deviations below the median of a healthy reference population

2011 budget (R189.5 billion) (Republic of South Africa, 2011), with R915 million of that allocation to the NSNP, the apparent resilience of child undernutrition requires some explanation.

It is impossible to tell whether or not the programme has been successful based on simple prevalence statistics. There are a multitude of other trends that could affect nutritional status in children (e.g. food prices, concurrent nutritional policy, etc.) Rigorous assessment of the NSNP should be couched in an understanding of what the NSNP is actually trying to achieve, as well as what the possible unintended consequences (good and bad) of the programme might be. This allows us to scrutinize the policy within the bounds of its own parameters, while avoiding blinkered analysis ignoring the other potential consequences of the programme.

At this stage, it is worthwhile reexamining what those explicit objectives are. First, the focus of the NSNP is squarely on improvement in nutritional status as a vehicle for improving the ability to concentrate and learn. This was made explicit after the transfer of the programme from the DoH to the DoE in 2004, with the only real nutritional guidelines incorporated into the programme embodied by the Food Based Dietary Guidelines (Republic of South Africa, 2008; Forster *et al.*, 2001). These guidelines are vague, and do not make any specific stipulates regarding micronutrient provision, although the programme does make implicit mention of micronutritional objectives in various

policy documents (Public Service Commission, 2008; Republic of South Africa, 2009, 2008). Second, some attention must be paid to the sustainable gardens and nutritional education components of the policy, but for the purposes of this paper those are assumed to be secondary.

The formal objectives of the NSNP exclude improvements in attendance and enrolment as positive potential outcomes of the program. This is somewhat strange given the explicit acknowledgements of that particular benefit in the documentation accompanying similar programmes worldwide. Countries like Brazil, Bangladesh, Swaziland, India and Jamaica (among others) all strive to increase enrolment through their school feeding programmes, and the World Food Program (2002) observes (albeit non-experimentally) that enrolment and attendance do seem to increase in schools that provide school meals (Vermeersch & Kremer, 2004). This omission is made all the more baffling when one considers that the attendance argument was explicitly included as an objective within the policy predecessor of the NSNP, the PSNP. Whatever the reason for this omission in official policy, it is worth recognising this as another potential benefit, even though it is excluded from the list of official objectives.

If we narrow our assessment to focus on the primary objective of the NSNP, improving learning ability through protein-energy nutrition, the anthropometrics of particular interest are measures like weight-for-age, weight-for-height or BMI-for-age based z-scores¹⁰. These measures provide more information on nutritional status contemporaneous with the point at which respondents are observed than a metric like height-for-age, which reflects chronic malnutrition (see Table 1). As pointed out at the beginning of this section, there has been little improvement in these nutritional outcomes since the PSNP was created in 1994.

Data on educational outcomes would also be desirable, but such data is relatively scarce. Ideally, we would like to compare educational outcomes in a way that is standardised across geographical and temporal locales, as it is challenging to interpret differences in local educational outcomes that are gleaned from tests that are constructed within some particular education system at a given point in time. Comparisons between cohorts subjected to differing curricula would not be comparable, as they would essentially represent different metrics for educational achievement with no way of converting one measure to another (like we would do with, say, kilograms and pounds or centimetres and inches). The little information that does exist (in the form of internationally comparable standardised tests) suggests that South African schools perform extremely poorly relative to other countries with similar (or lower) levels of expenditure on schooling¹¹ (van den Berg, 2007; Taylor, 2011; Glewwe & Miguel, 2008).

¹⁰These are generated by checking $BMI = \frac{weight(kg)}{height(m)^2}$ against referenced, median scores in healthy populations. Any respondent greater than two standard deviations below the relevant median z-score is classified as underweight by either of these two measures. There are also certain relative cutoffs above which the BMI-for-age z-score classifies respondents as obese or overweight

¹¹See Table 7 in Appendix B for a telling, albeit outdated, inter-country comparison of standardised mathematics (TIMSS) scores

The NSNP aims to improve educational outcomes by improving nutrition, but there has been little visible improvement in either of the two areas. Furthermore, the persistence of unequal distributions of these undesirable outcomes defy the progressive mandate(s) underpinning the formulation of the NSNP. These provide strong motivators for a rigorous investigation of the programme, as we need to know if the observed stagnation in social mobility is being driven, counteracted or unaffected by the NSNP. This requires that we isolate its effect on nutritional and educational outcomes from treatments external to the programme, including time trends and shocks to things like food prices and disease prevalence. Further impetus is provided by the year-on-year increases in the value of the conditional grant given to the NSNP. If the programme is failing, it would be desirable to reallocate funding to something more developmentally productive. Even if such a reallocation of resources is politically infeasible (the NSNP has been active for close to 8 years, while the PSNP was around for 10), it may be worth figuring out where the programme is constrained and attempt to at least improve it. To the author's knowledge, there has been no rigorous quantitative assessment of the NSNP to date.

3.2 The Hypothesised Benefits of School Feeding Programmes

There are two main arguments for the creation and maintenance of school feeding programmes, in general. The first of these is that school meals lead to improved nutrition, which leads to improved cognition, learning ability and improves educational outcomes. The second is that school meals provide an incentive to attend school in the first place, which is especially valuable in developing countries that observe high rates of absenteeism (Vermeersch & Kremer, 2004).

The first of these arguments relies on two connections. The first is that school meals actually improve nutrition, while the second is that there is a connection between nutritional status and educational performance. These links are difficult to demonstrate non-experimentally because nutritional status, child health, and schooling are likely to be correlated with various household preferences, many of which may be unobservable to the econometrician. Independent results, across various settings, documenting the validity of each of these connections will be briefly explained in no particular order.

There is some experimental evidence of the positive effects of morning nutrition on short-term attention and learning ability. Pollitt *et al.* (1998) review a set of experiments conducted on nine to eleven year old children subjected to morning and overnight fasting. These children were then required to submit to a set of tests aimed at assessing their abilities of recall and ability to discriminate between visual stimuli. The first two experiments were conducted on well-nourished, middle class girls and boys in the United States, while the third was conducted in Huarez, Peru. The children were separated into treatment and control, with treated subgroups served breakfast the morning before the test and non-treated groups deprived of that breakfast. The children arrived the night before to ensure that they all began from the same caloric baseline at the time of treatment. Pollitt *et al.* (1998) discovered that the chil-

dren in control groups responded slower to stimulus, made more errors and had slower memory recall. This effect was more pronounced in groups of children from the Peruvian group who were less well-nourished than their American counterparts. Despite these promising results in a laboratory setting assessing one of these connections, experiments that look at the entire causal chain (School feeding → Improved Nutrition → Improved Educational Outcomes) are rare.

There is some experimental evidence that school feeding programmes improve nutritional status in a South African primary school children. Napier *et al.* (2009) randomly allocate three sets of children age six to thirteen in an informal settlement in Gauteng and took various anthropometric and biochemical measurements after a time to assess the differences in various nutritional strategies. In the first group (n=60) children were given a whole wheat pilchard and spinach vetkoek, in the second (n = 60) children received food according to the PSNP, whereas in the third group (n=40) children were given fruit. The study concluded that all three groups had improved significantly on weight-for-age and micronutritional metrics. The presence of a control group was conspicuously absent from this study.

Despite this evidence, it is possible that school meals do not actually improve nutritional status at all. One concern is that there could be intrahousehold reallocation of resources that arise as a result of the provision of school meals. Parents may reallocate caloric intake optimally between their children and themselves. Jacoby (2002), in an experimental study of the Phillipino equivalent of the NSNP, provides some convincing evidence that the caloric intake transferred to treatment recipients is “sticky”. That is, school meals are not perfectly reallocated within the household and the recipient of the school meal enjoys the improved nutritional status afforded by exposure to the treatment. The extent to which this applies in a South African context requires separate assessment, as the external validity of the paper hinges on, most notably, cultural norms (in terms of food sharing customs within the household especially) that are bound to be contextually unique. For the purposes of this paper, the conclusion of Jacoby (2002) is less important than the hypothesis, as it has implications for the interpretation of any econometric assessment of the programme and could explain why experiments assessing the effects of programmes on education do not produce positive results.

The second argument is that school feeding programmes increase attendance and enrolment. The idea is that parents are either more inclined to enrol their children, enrol their children sooner, or encourage regular attendance in schools that receive a free or subsidized meal (Vermeersch & Kremer, 2004; McEwan, 2010). Casual observation on the part of the World Food Program (2002) suggests that this is the case, but the causal effect of school feeding programmes on enrolment and attendance is difficult to pin down econometrically. Schools that are eligible to receive government programmes are, generally, poorer than those that are not, so comparing enrolment rates between schools observationally would not really reveal anything about the effect of school feeding programmes because of selection bias. Schools in which feeding is paid for by par-

ents are likely to be intrinsically different from those that receive government programmes, or no school feeding at all (Vermeersch & Kremer, 2004). With that said, there is at least some positive experimental evidence suggesting that school feeding programmes positively affect enrolment and attendance rates.

To summarise, the experimental studies that have been conducted assessing the effects of school feeding programmes on either attendance, enrolment or educational outcomes (or some mixture) reveal mixed results (see Table 6 in Appendix B). More specifically, these studies suggest modest, positive returns on attendance and enrolment and no return to educational outcomes statistically different from zero (McEwan, 2010).

3.3 Motivation

The NSNP represents an important and potentially valuable tool in the arsenal of social policy available to the DBE. To make educated allocative decisions regarding the funding of the NSNP, it is necessary to have a firm understanding of the entire array of potential programme effects. Numerous qualitative studies of the NSNP have been conducted, but even those critical of various aspects of the policy tend to end on a conciliatory note by casually observing an improvement in cognition of learners within the programme.

The extent to which these casual observations offer any insight in to the actual causal effects of the NSNP need to be appraised quantitatively, mediated by tenacious self-censorship on the part of researchers given the data constraints, flexible treatment assignment rules, the implications of the results, and the array of biases that can confound any econometric analysis that strives to make some causal inference. In the absence of rigorous scientific examination of the policy, the state is blindly investing a considerable amount of money in a programme that may not even have any effect, instead of another project that might. Even if we discover that the programme works effectively, there could be ways to improve it that can only be uncovered through considered, unbiased assessment.

Given the severity, persistence and regressiveness of both nutritional and educational outcomes in South Africa, complacently ignoring the potential for improvement by neglecting to do this is undesirable. Duflo & Banerjee (2011) succinctly paraphrase Amartya Sen: “poverty leads to an intolerable waste of talent...poverty is not just a lack of money; it is not having the capability to realize one’s full potential as a human being. A poor girl from Africa will probably go to school for at most a few years even if she is brilliant, and most likely won’t get the nutrition to be the world-class athlete she might have been, or the funds to start a business if she has a great idea.” The current array of social policies aimed at affording equality of opportunity along nutritional and educational dimensions, in line with Sen’s argument (crudely summarised above), seem to have failed. This latter point provides the strongest motivation to assess the NSNP of all.

4 Methodology

4.1 Regression Discontinuity Design¹²

A regression discontinuity design (RDD) is useful in identifying treatment effects if the probability of receiving some treatment is a discontinuous function of one or more of the underlying variables (Cameron & Trivedi, 2005). There are two types of RDD. The first of which is the sharp RDD, which can be seen yielding a selection on observables kind of estimator, and the fuzzy RDD, which is more akin to an instrumental variables model (Angrist & Pischke, 2009).

Usually, we are wary of comparing treatment to non-treatment groups in estimating the causal effect of some policy. However, if there is information on selection into treatment which is based on an administrative rule that assigns that treatment, at least in part, based on some underlying variable, we have more information on the selection process and can use that information to derive causal effects quasi-experimentally. Imagine an assignment process that allocates individuals into a programme if, say, log of income is below a certain level. The corresponding probability of getting treated would be discontinuous at that threshold. Figure 1 intuitively illustrates how data generated using this data generating process would look. The jump in regression lines at the cutoff point gives RD its name.

Figure 1: RDD Example (*Adapted from Cameron & Trivedi (2005)*)



For the sake of generality, we will refer to the selection variable (log of

¹²This section closely follows Cameron & Trivedi (2005) and Angrist & Pischke (2009)

income, in our example) as S with distinct cutoff \bar{S} , for the remainder of this section. In the sharp RDD case, individuals are assigned to treatment solely based on the observed and continuous values of S . Those falling below the cutoff receive treatment ($D = 1$), and those above do not ($D = 0$) based on the measured and deterministic decision rule: $D_i = 1[S_i \leq \bar{S}]$, where i represents the unit of observation. We further define

$$E[u|D, S] = E[u|S] \quad (1)$$

where u denotes the error term in the outcome equation. “Because S is the only systematic determinant of D , S will capture any correlation between D and u (Cameron & Trivedi, 2005).” If the error term (u_i) is correlated with the treatment variable (D_i) OLS would yield an inconsistent estimator of the treatment effect α . One solution would be to include some “control” for the conditional mean function $E[u|D, S]$ in the outcome equation so that

$$y_i = \beta + \alpha D_i + k(S_i) + \epsilon_i \quad (2)$$

where $\epsilon_i = y_i - E[y_i|D_i, S_i]$. If $k(S)$ is specified correctly and is linear, then we can estimate α as being given by the distance between the two parallel regression lines. This will provide an unbiased estimate of the common treatment effect *if the control function is linear* (Cameron & Trivedi, 2005). In fact, if the underlying function is truly linear, the best linear unbiased estimator of the coefficient on D will be given by OLS. In the more general case of varying treatment effects, in which α represents $E[\alpha|\bar{S}]$ and $k(S)$ is a specification of $E[u|S] + (E[\alpha_i|S] - E[\alpha_i|\bar{S}])1[S \leq \bar{S}]$, where $1[S \leq \bar{S}]$ is equal to one if the condition in square brackets is satisfied, the incorrect specification of $k(S)$ leads to inconsistent estimators. In other words, if we incorrectly specify $k(s)$ as a linear function, the inclusion of $k(s)$ will be insufficient in conditioning the estimation of α on the non-linearity of the error term (i.e. it will not accurately capture the relationship between the error term and the selection variable). This problem can be ameliorated by semiparametric specification of $k(S)$.

The general intuition behind RDD is that observations around the cutoff have essentially the same value for S . Therefore, the sample of individuals around that cutoff will resemble a properly randomised experiment. Those just on either side of the cutoff are expected to be similar to one another. The goal is to compare the conditional means of the outcome variable, y , around the cutoff point. The choice of interval around the cutoff biases estimates as it expands, but defining the cutoff as a straight line (the cutoff point) will mean that $Pr[D = 1|S] \in [0, 1]$. As a result, we need to tradeoff some bias for variance to draw conclusions from treatment effects estimated in this way (especially if there is a relationship between the selection variable and the outcome variable).

If it is possible to accurately define the functional form of the relationship between the outcome and selection variables, we can “use more observations and extrapolate from above and below the cutoff point to what a tie-breaking randomised experiment would have shown. This double extrapolation, combined with exploitation of the ‘randomised experiment’ around the cutoff points, has

been the main idea behind regression discontinuity analysis (van der Klaauw, 2002; Cameron & Trivedi, 2005).” More formally, in our RDD example

$$\lim_{S \uparrow \bar{S}} E[y|S] - \lim_{S \downarrow \bar{S}} E[y|S] = \alpha + \lim_{S \uparrow \bar{S}} E[u|S] - \lim_{S \downarrow \bar{S}} E[u|S] \quad (3)$$

Assumption A 1 *The conditional mean function $E[u|S]$ is continuous at \bar{S}*

Assumption A 2 *The mean treatment effect function $E[\alpha|S]$ is right continuous at \bar{S} :*

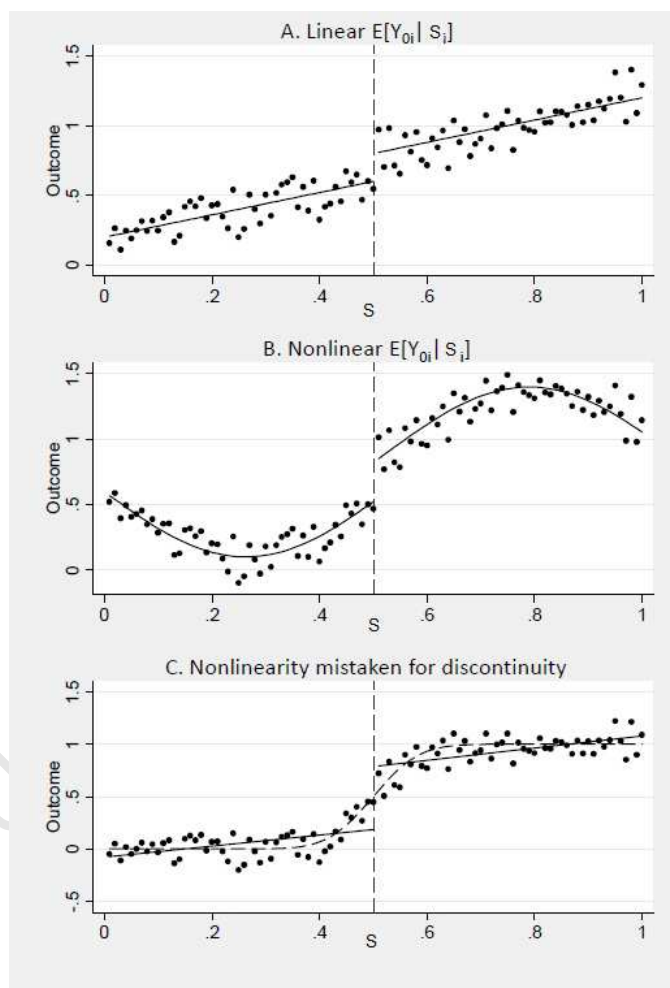
$$y_i = \beta + \alpha D_i + k(S_i) + \epsilon_i,$$

where $\epsilon = y_i - E[y|D_i, S_i]$

These assumptions are a formal statement of the requirement that observations around the cutoff, \bar{S} , must be likely to have similar average outcomes to ensure that the result in Equation 3 holds.

Imbens & Lemieux (2008) point out that with sharp RDD, there are no values of the selection variable for which we observe both treatment and control variables. In the sharp RDD, the validity of the method relies on our willingness to extrapolate across covariate values in the neighbourhood of the discontinuity (Angrist & Pischke, 2009). Figure 2 illustrates how non-discontinuities may appear to be discontinuous if we do not take care in our specification of $k(S)$.

Figure 2: Sharp RDD and non-linearity of S (Adapted from Angrist & Pischke (2009))



4.1.1 Nonparametric Regression at the Boundary

Estimating α at the cutoff is effectively a nonparametric regression problem Imbens & Lemieux (2008). This would be relatively simple were it not for the discontinuity that is, by definition, a single point at the cutoff. This is because standard nonparametric kernel regression has poor convergence properties at the boundary, which is always present in this method of estimation. To illustrate this, let us define two conditional means

$$\mu_l(x) = \lim_{S \uparrow x} E[y|S], \text{ and } \mu_r(x) = \lim_{S \downarrow x} E[y|S]$$

Estimated α would be equal to the difference between these two conditional means with the cutoff point \bar{S} as their argument,

$$\alpha = \mu_l(\bar{S}) - \mu_r(\bar{S})$$

Using standard nonparametric regression to estimate these conditional means, using kernel $K(u)$ with $\int K(u)du = 1$, the regression functions at x can be estimated as

$$\hat{\mu}_l(x) = \frac{\sum_{i: X_i < \bar{S}} y_i \cdot K\left(\frac{X_i - x}{h}\right)}{\sum_{i: X_i < \bar{S}} K\left(\frac{X_i - x}{h}\right)}, \text{ and } \hat{\mu}_r(x) = \frac{\sum_{i: X_i \geq \bar{S}} y_i \cdot K\left(\frac{X_i - x}{h}\right)}{\sum_{i: X_i \geq \bar{S}} K\left(\frac{X_i - x}{h}\right)}$$

where h is the bandwidth of the kernel.

This yields the estimator

$$\hat{\alpha} = \hat{\mu}_r(x) - \hat{\mu}_l(x) = \frac{\sum_{i: X_i < \bar{S}}^N y_i \cdot K\left(\frac{X_i - x}{h}\right)}{\sum_{i: X_i < \bar{S}}^N K\left(\frac{X_i - x}{h}\right)} - \frac{\sum_{i: X_i \geq \bar{S}}^N y_i \cdot K\left(\frac{X_i - x}{h}\right)}{\sum_{i: X_i \geq \bar{S}}^N K\left(\frac{X_i - x}{h}\right)}$$

This is essentially equivalent to the difference between non-weighted means of outcomes h units above and below either side of the cutoff. Imbens & Lemieux (2008) go on to show that this estimator is likely to be biased¹³.

Instead, this paper employs an RDD that derives estimates based on local linear regressions on the recommendation of Imbens & Lemieux (2008) and Fan & Gijbels (1996). This entails fitting linear regression functions to observations within distance h from the cutoff on either side:

$$\min_{\beta_l, \gamma_l} \sum_{i | \bar{S} - h < S_i < \bar{S}}^N (Y_i - \beta_l - \gamma_l \cdot (S_i - \bar{S}))^2$$

and

$$\min_{\beta_r, \gamma_r} \sum_{i | \bar{S} \leq S_i \leq \bar{S} + h} (Y_i - \beta_r - \gamma_r \cdot (S_i - \bar{S}))^2$$

¹³The technical component of this is detailed in Section C of the Appendix

which can be used to estimate the values of $\mu_l(\bar{S})$ and $\mu_r(\bar{S})$

$$\widehat{\mu_l(\bar{S})} = \hat{\beta}_l + \hat{\gamma}_l \cdot (\hat{S} - \bar{S}) = \hat{\beta}_l$$

and

$$\widehat{\mu_r(\bar{S})} = \hat{\beta}_r + \hat{\gamma}_r \cdot (\hat{S} - \bar{S}) = \hat{\beta}_r$$

the average treatment effect is then given by the difference between these estimates, or

$$\hat{\alpha} = \hat{\beta}_r - \hat{\beta}_l$$

These local linear regressions are rate optimal and have desirable bias properties that are preferred to their alternatives (Imbens & Kalyanaraman, 2010). Given this preference, there is still the question of kernel and bandwidth choice. As per the recommendation of Cheng *et al.* (1997), triangle kernels are used for their favourable boundary properties. Bandwidth choice is informed by the algorithm developed by Imbens & Kalyanaraman (2010), which is designed to minimise the mean squared error of the treatment estimator. Note that this is a gross oversimplification of the technical process of selecting appropriate bandwidths and the technically minded reader is encouraged to consult Imbens & Kalyanaraman (2010) for a thorough explanation of the derivation of optimal bandwidth and the properties of their bandwidth choice.

4.2 Other Methodological Considerations

Lee & Lemieux (2009) provide a set of practical guidelines for RDD estimation that are worth noting. First, *RDD can be invalid if individuals can precisely manipulate their selection variable*. Any social programme or grant that assigns individuals based on need will have some eligibility cutoff (unless the grant or programme is applied uniformly to the entire population, in which case the RDD is not applicable anyway). If individuals can perfectly manipulate their selection variable so that they are just within the treatment group then the selection effect is not properly dealt with (Lee & Lemieux, 2009).

Second, *if individuals can only partially manipulate their assignment, the variation in treatment is consequentially randomised as if from a randomised experiment*. Even if incentives to qualify for treatment exist (which is highly likely), this simply means that some individuals are more likely to have values of S around \bar{S} . Every individual will still have approximately the same probability of having a value of S that qualifies them for the treatment Lee & Lemieux (2009). Understanding this is crucial to understanding the identification strategy employed in this paper.

As an aside, imagine a scenario in which there is a distribution in the ability to influence selection into treatment across the entire population, which is positively correlated with S , but *treatment is imperfectly predicted by S* . This could occur in a real-life scenario quite easily (imagine someone manipulating their tax figures, which are recorded as “true” selection variables, in order to fall below a certain threshold). The likelihood of engaging in attempts to self-assign

are distributed in a way that is positively correlated with proximity to the cutoff point (i.e. it is more difficult, and less likely given incentives, for someone far above or below the cutoff to attempt to self-assign into the treatment), so we only consider individuals near the boundary. We also assume that the outcome of interest represents a “good” (higher is better; as opposed to a “bad”) and that the treatment positively influences the value of y_i . Finally, we assume that those that covertly self-select into treatment still have values of S that place them within the bandwidth around the cutoff, so as to be included as observations in the calculation of the RDD estimate using our local linear regression approach.

If there is a positive relationship between actual treatment status and the selection variable the RDD estimator of treatment effect can be interpreted as an upper bound. This is because individuals that self-assign to the left of the cutoff will drag the conditional mean up (and those to the right would drag it downwards). We would expect the most likely misspecification of treatment to occur at values of S that are extremely close to \bar{S} . Given that we are comparing individuals near the cutoff on the basis that they are extremely similar, their partial ability to self-select into treatment should not bias the RDD estimator a huge amount, if at all. This hypothetical situation essentially relates RDD estimates in the presence of deviations in treatment assignment from the values of S relative to \bar{S} , and has direct implications for the interpretation of the results summarised in Section 5.

Third, *RDD designs can be analysed and tested like randomised experiments*. If variation in the treatment near \bar{S} is approximately randomised, all the characteristics of the respondents in that area should be roughly the same. A violation of this is the same as non-continuity of covariates at the threshold and further implies that the identifying assumption that individuals cannot perfectly manipulate the assignment variable is violated. We can test whether or not this assumption holds by checking sets of relevant covariates at the selection variable threshold. This point represents the key implication of the local randomisation results (Lee & Lemieux, 2009).

4.3 Data

The National Income Dynamic Study (NIDS) is the first ever South African national panel study. The data employed in this analysis comes from the first wave, which was conducted throughout 2008, although primarily in the first half of the month (91% of adult respondents were interviewed between February and June). This dataset is particularly suited to the purpose of this investigation as it contains a large sample of children ($n = 9616$) and includes anthropometric information that can be used to calculate various z-scores.

The NIDS sample survey design is a two-stage cluster type with stratification by district council. The primary sampling units (PSUs) are drawn from the master sample provided by Statistics South Africa, with households then are drawn from that subsample of PSUs. Design weights were calculated as inverse to the probability of inclusion, which entails a two-stage calculation. In the first

stage, the probability of sampling each PSU is calculated, and in the second, the probability of sampling each household within each PSU in the NIDS sample is calculated. The latter corrects for unit non-response.

The post-stratification weights adjust the design weights in such a way that age-sex-race marginal totals match 2008 midyear population estimates, constrained by the requirement that province population should correspond to the official provincial population statistics released within that same set of midyear estimates. These weights were also calculated with respects to the constraints that total population equal 48,687,000 and that each individual within a particular household is weighted the same.

The task of estimating the effect of the NSNP is bound by a number of constraints in the data. Most importantly, the basis upon which the NSNP is assigned differs from any units of observation (aggregated and individual) in our dataset. Furthermore, assignment into treatment groups (by electoral ward) is done on the basis of poverty score calculation, which is not observed. The DBE proved reluctant to communicate their calculation methods, and the exact calculation methods (to the knowledge of the author) are not contained within any publicly available policy documentation. Although we have *some* information on the poverty score calculation (it is based on the rank of mean household income, literacy and unemployment by electoral ward), attempts at mimicking this assignment would be beset by issues given the mismatch between the level of aggregation at which that poverty score is actually calculated and the available geographic specifications available in the data. The 400 PSUs drawn in the NIDS sample are pulled from a total sample of 3000 PSUs in the master sample, but there are 4277 electoral wards, in total, in South Africa. By the pigeonhole principle, at least some clusters will contain multiple wards. This mapping could be more complicated if PSUs partially contain multiple wards, with other parts of those wards contained in other PSUs. Even if this was not the case, the actual calculation of poverty scores could be weighted more towards some of its determining variables than others, which would render our approximation of the actual poverty score inaccurate. Moreover, the NNSF (detailed in Section 2.4) allows for exceptions to the poverty score to assignment mapping. As a result, a proxy for school quintile based on schooling fees was employed, and the value for the top of the third quintile was calculated as our base cutoff point. The implications of the use of this imperfect proxy are discussed in the following section and influence how the results are interpreted in Section 5.

Variables were also constructed to reflect a number of anthropometric z-scores including weight-for-age, height-for-age and BMI-for-age. This was done using a *Stata* macro, developed by de Onis *et al.* (2007), which constructs growth curves for school-aged children and adolescents that accord with the similarly calculated WHO child growth standards (World Health Organization, 2006). This helped illuminate a missing data issue in the NIDS, as a combination of unit non-response for the weight/height component of the survey (which required the respondent to be measured and weighed by a fieldworker) and measurement error for those height and weight variables mean many Z-scores were discarded.

The macro has an automatic process of discarding bogus observations if they fall outside the range of plausible human dimensions, so these measurement issues effectively reduce the sample size of our final estimation. This measurement error and non-response appears uncorrelated with any major demographic variables, and the unit non-response is “ignorable” (it can be corrected for using post-stratification weights).

4.4 Identification Strategy

For the RDD to be a plausible identification strategy, we need the selection variable to have error components that are continuously and stochastically distributed, which will occur if agents do not *precisely* sort around the discontinuity threshold. If this holds, then variation around the discontinuity cutoff will be “as good as randomised” Lee & Lemieux (2009). Assessing the plausibility of this requires us to think about the correlation between school fees and the *true* selection variable (poverty score), as well as the precision with which individuals and local ward administrators vary each of those selection variables. Furthermore, we also have to consider the ability of individuals to choose the ward in which they live.

In the NNSF, the logic of assigning schools to quintiles by electoral ward turns on those wards acting as school catchment areas. Individuals do select their living areas (within obvious constraints), and can therefore precisely manipulate their assignment into treatment if they really want to. Given the multitude of benefits (proximity to work, family, schools, markets, transport), costs (crime, pollution) and constraints that enter into the decision to live in a certain area, it is implausible that individuals precisely vary their selection into treatment near the boundary. The likelihood of this is also related to the extent to which individuals know which areas contain schools that receive the treatment. If we consider that the proclivity of individuals to precisely manipulate their treatment status is a determined by

- the magnitude of the importance of being in a quintile three or below area, relative to other determinants of choice of living area
- the information set of the decision maker

the assumption that individuals *do not* precisely sort into treatment in this way seems plausible.

A potential threat to this could be if individuals live in wards that do not receive the treatment (and are socioeconomically better endowed in a way that is reflected in the outcome of interest), but send their child or children to poorer quintile schools to enjoy the benefits of the treatment. Again, the decision to send a child to a certain school is multifaceted, with its own array of costs, benefits and constraints. It is unlikely that fourth quintile parents would deem the benefits of the treatment (if they are even aware of them) worth the transport costs and lower expected standard of schooling. Assignment into quintile three, in 2008, only qualified the school for inclusion in the NSNP (that is, did not

afford the benefits of any other incentivising treatments), although the inclusion in quintile three does qualify schools for more funding. As a result, the only likely motivation for parents near the cutoff to precisely sort below it would be inclusion in the programme or access to a better funded school. The implications of this for our RDD estimates could be that fewer poor children are included in treatment, and they would probably have higher baseline z-scores.

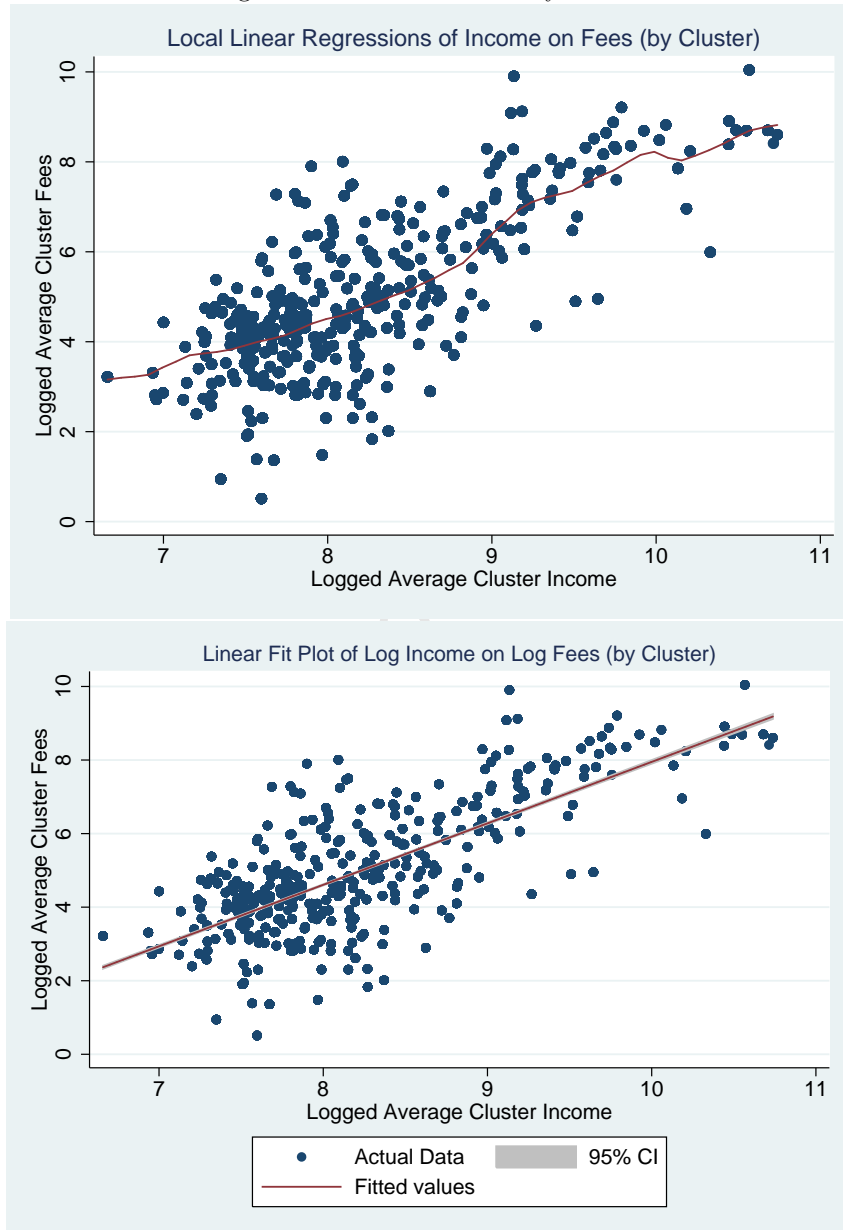
Acting against this threat are a number of economically intuitive relationships. First, transport costs would mean that only individuals physically proximate to treatment areas would be likely to send their children into other wards to receive the treatment. Second, given that the demarcations of quintile area are essentially arbitrary, the distribution of socioeconomic status between nearby houses is more likely, in reality, to be closer than that assignment suggests. As a result, individuals that find it worthwhile to send their children into another quintile are likely to be close to the socioeconomic status of those living in that quintile anyway, given that they would need to be proximate to make the travel worthwhile. This argument is undermined by the proximity of the household to cheap, fixed-cost forms of public transportation, but even then the cost of transportation would be roughly the same as the value of the provided meal. This could be offset by the commensurate reduction in fees likely to be observed in those lower quintile schools (something upon which our RDD partially depends!), but this undermines the notion that individuals can *precisely* sort around the discontinuity threshold. This discussion of potential trade-offs highlights how selection into treatment through school choice is quickly swarmed by a set of other considerations that go into that decision. If this argument holds, the covariates around the cutoff are likely to be continuous and the RDD estimation strategy valid.

Another potential fragility of this identification strategy is the ability of the coalitions of school administrators to self-select their wards into the treatment. If the provisos outlined in Section 2.4 are granted often, this might prove especially problematic. This is because coordination between local education administrators and political shrewdness may be correlated with the ability to provide other services. Given that the NSNP and extra funding embody the set of returns to schools designated within the third quintile in 2008, the extent to which this is a problem turns on the ease with which wards are granted exemption and the extent to which local administrators are motivated to qualify for inclusion in quintile three. This marks the strongest challenge to successful identification, as these administrators are not varying their selection variable (the poverty score) to get into treatment, but self-assigning to treatment through other means.

Given the above detailed set of caveats, we turn to an assessment of the extent to which the fees variable is likely to correspond to the true selection variable. The true poverty score is essentially a measure of the socioeconomic status of the electoral ward. Given our ignorance of the poverty score calculation procedure, we need to find some way to check if the fees within an area are correlated with the average household income of that area. Although electoral wards are smaller than the clusters in the NIDS, these clusters are the small-

est level of geographic aggregation available. To get an idea of the extent to which fees correlate with average income within a community, we can examine the correlation as summarised by the graphic in Figure 3. The hope is that the individual units of observations are geographically corralled in an arbitrary way. Of course, this is not necessarily true given that certain policies (including the NSNP) are assigned to administrative areas, but this is the best available strategy given the data constraints. Clearly, there is a strong positive correlation between the average wealth of the geographic area and the average school fees paid in that area. The fees variable was chosen as our selection variable (as opposed to household income) because of the likelihood of electoral wards being heterogeneous to some extent. School fees may provide a localised approximation of the socioeconomic status of the surrounding area and, as a result, a better approximation of the quintile assignment of that area. It is also hoped that they accurately reflect the quintile of the school more accurately given the exceptions to the rule detailed in Section 2.4. Implicit in the use of this fees variable is a strong assumption that demand for schooling is not altered by the programme in a way that leads to a commensurate increase in fees.

Figure 3: Fees and Income by Cluster



It was necessary to invoke a set of qualitative arguments for the use of fees as a proxy variable for the poverty score assignment of electoral ward, but the actual process of identifying the treatment effect in the RDD is based on whether or not we observe continuous, stochastic errors in the selection variable employed. If we can safely assume that school fees approximate the poverty score of the ward and rule out precise selection around the discontinuity, our identification strategy is valid and we can let the data speak for itself. Note that the precise self-selection arguments made above relate only to instances in which wealthier individuals, or wealthier wards, precisely sort into the treatment. If this is implausible, we can interpret any observed discontinuity as equivalent to the local average treatment effect for the programme that we would observe in a randomised experiment.

5 Results

Table 5 and Figure 4 represent the results of our RDD estimates, with the sample limited to those children that attend school primary school (i.e. have non-missing values for their fees variable and are eligible to enter school given their age). The cutoff was originally specified at the top of the quintile three, but checks were performed around that cutoff given the imperfect relationship between the true selection variable and school fees. Near the top of the third quintile (a quarter of a standard deviation below, to be precise), there is evidence of a discontinuity in both z-scores. Given that treatment quality is heterogeneous, and given the likelihood of the selection variable imperfectly capturing assignment into treatment, the discontinuity gap can be interpreted as the weighted-average, intention-to-treat effect across all individuals Imbens & Lemieux (2008) of a magnitude roughly equal to -0.5 for both z-scores. Given that those below the cutoff are those exposed to the treatment, there appears to be some evidence that the NSNP has a positive effect on nutritional outcomes. These results are robust to alternative specifications of kernel and bandwidth, which are detailed in Tables 8 and 9 in Section D of the Appendix.

VARIABLES	(1) -.75 s.d	(2) -.5 s.d	(3) -.25 s.d	(4) Top of 3rd Quintile	(5) +.25 s.d	(6) +.5 s.d
Weight-for-age Z-score	0.599 (0.466)	-0.297 (0.237)	-0.455* (0.234)	-0.257 (0.213)	-0.132 (0.265)	0.470 (0.289)
Observations	657	657	657	657	657	657
BMI-for-age Z-score	0.331 (0.296)	-0.371 (0.236)	-0.528** (0.208)	-0.106 (0.230)	0.161 (0.307)	0.189 (0.294)
Observations	764	764	764	764	764	764

Standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

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Figure 4: Graphical Representation of RDD

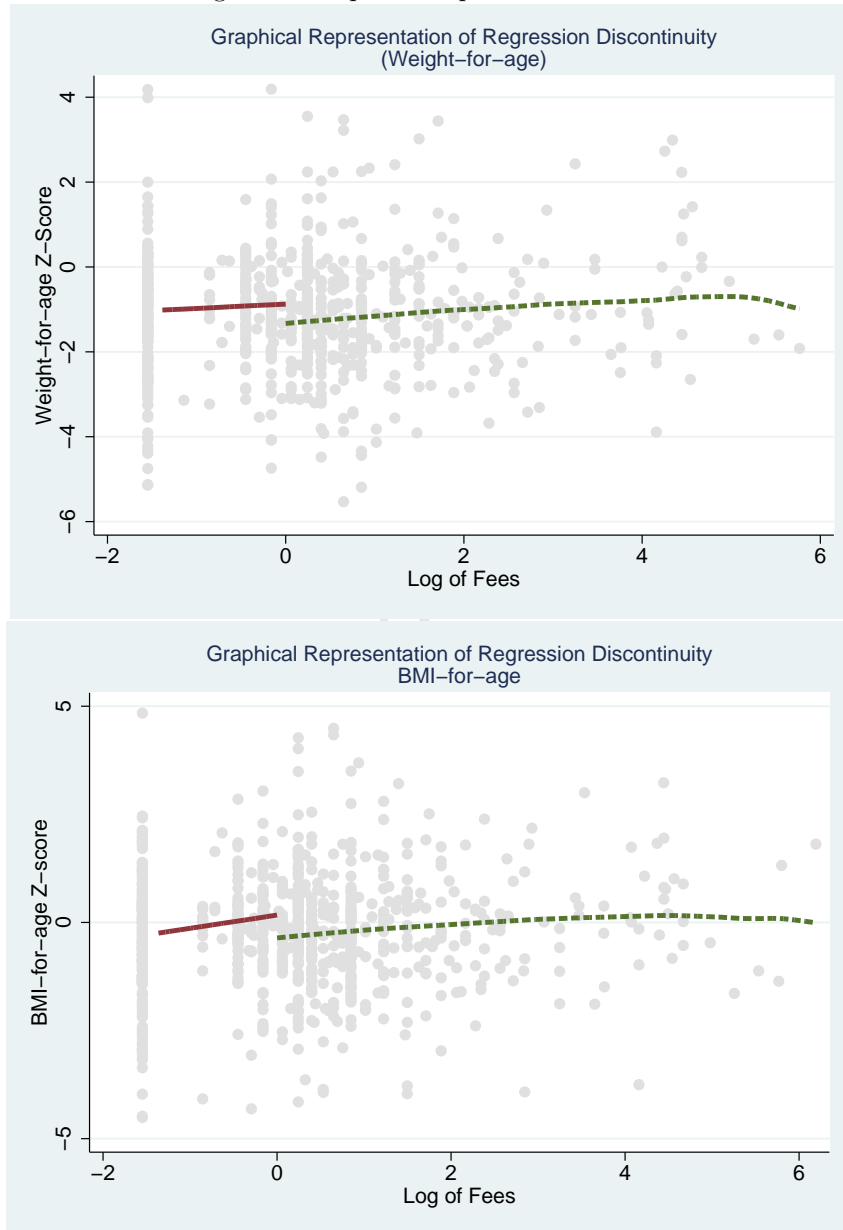


Table 2: Check for Household Income Discontinuity

	(1)
CUTOFF	-.25 s.d
Log HH Income	-0.0979 (0.149)
Observations	994
Standard errors in parentheses	
*** p<0.01, ** p<0.05, * p<0.1	

To appraise the validity of these results, we need to check for discontinuities in other continuous variables that may “jump” at the same cutoff for fees. The most likely is household income (for reasons outlined in Section 4.4). Table 5 below summarises the regression discontinuity results for this check, which are not significantly different from zero.

6 Discussion

The above results suggest that the NSNP does have some effect on weight-for-age and BMI-for-age z-scores. These are interpreted as reflecting the extent to which the respondent is underweight (for low scores) and overweight (for high scores). In the absence of data on educational outcomes, we cannot necessarily interpret this as meaning that the NSNP causes improved *educational* outcomes or conclude that the school feeding component of the programme is responsible for this observed treatment effect unless we invoke other studies that may not be externally valid (and are, therefore, perhaps not applicable to this context). As a result, these results merely represent the first step in rigorously assessing whether or not the programme achieves its objectives and must be interpreted cautiously, as there are a number of factors that could render the identification strategy imperfect. First, it is difficult to understand exactly why the cutoff is slightly below where we would expect. This may be attributable to the use of a proxy for the actual selection variable that is likely to be imperfect. Given our inability to test the relationship between school fees and the true selection variable, this remains an open question.

One possible explanation is that the presence of fee exemptions (total or partial) falsely includes a number of respondents on the “wrong side” of the discontinuity. As such, we have a number of false positives that are distributed in a way that only weakly influences the actual estimate. This would mean that the third quintile of the fees variable would not perfectly correspond with the third quintile of the poverty score and would shift our cutoff point slightly

to the left. This is but one of many potential explanations for this given the inexactness of our proxy.

More worrying would be if there was some unaccounted for covariate discontinuity biasing our estimates. It could even be that the discontinuity is unobservable given the available data. This poses the greatest threat to the validity of these results. For reasons of precise selection outlined in Section 4.4, we might expect to see some sorting of better nourished children to the left of the cutoff. If this is being driven by something other than household income, which we have tested for discontinuity at this point, our estimates cannot be interpreted as equivalent to a randomised experiment as some selection effect is preserved. This, in conjunction with the nebulous, essentially untestable relationship between proxy and true selection variable, demand that these results are treated with some scepticism.

Ideally, we would have the true selection variable and information on actual school quintiles, as well as information on educational outcomes. NIDS is set to release a second wave of data that is meant to track the same individuals through time. Furthermore, the second wave release is set to coincide with the release of a new version of the first wave dataset that contains information on school quintiles. This would allow us to properly check which respondents are actually within the treatment.

Future research into the effectiveness of the NSNP would also require that investigators acquire poverty score calculation information. On paper, this information is supposed to be freely available in line with the transparency goals enumerated in the NNSSF. Anecdotally, and in the experience of the author, acquiring this information is not simple. Government officials are either reluctant to release it or unaware of the actual underlying calculation process. It is possible that more information on poverty score calculation will become available soon after the release of the new version of the South African census, which was conducted in late 2011. The release of this data will prompt the DBE to recalculate the poverty score (as per the instructions of the NNSSF). The hope is that this poverty score calculation process will get properly documented.

If such data could be obtained, a fuzzy RDD could be employed as we would not only have a selection variable (the poverty score), but information on compliance as well. This would not only tighten up the identification strategy, but allow researchers to test the levels of non-compliance (or the extent to which wards are sorted into quintiles on the basis of something other than the ward poverty score). For this to work, one would need to obtain information on electoral wards in the NIDS. This data (and even GPS data on respondent location) exists, but is confidential. At the time of writing, the NIDS team were preparing for the release of the second wave, which is a labour intensive process. As a result, it was not possible to timeously acquire the necessary clearance to access this confidential data.

Even in the absence of the release of data on school quintiles, if one were to combine the GPS data in the confidential NIDS and the Education Management Information Systems (EMIS) data, which has GPS coordinates for each school, it might be possible to map respondents to schools in a way that exploited

information gleaned from differencing the GPS coordinates of proximate schools and the household (or something similar). The EMIS also has information on particular schools that could be conducive to a thorough evaluation of the NSNP.

Conspicuously absent from our analysis thus far are considerations of attendance and the “flypaper effect” observed by Jacoby (2002). This is simply because these relationships are untestable given the data limitations. Ideally, we would be able to assess the full suite of policy objectives and implications in our analysis to develop a rounder picture of the entire process.

7 Conclusion

Further research employing alternative methods of investigation like the ones detailed above are needed to add to the results detailed in this paper. Although this investigation represents a promising, if shaky, first step toward properly assessing the effectiveness of the NSNP, data constraints, identification challenges and myriad other issues leave the analysis open to a number of criticisms (which this paper has attempted to acknowledge throughout). As a result, the results contained herein should be interpreted as suggestive and are meekly interpreted as indicative that the programme, as a whole, has a positive effect on weight-for-age and BMI-for-age related nutritional outcomes.

The function of this paper is less geared towards the treatment effects estimated herein, and more towards the succinct communication of relevant policy components and identification challenges that could inform any further analysis of the programme. The goal was to create a paper that provided a starting point or baseline from which further analysis into the effectiveness of the NSNP could be conducted. Given the dearth of rigorous econometric analysis of the programme, the increasing amounts of money invested in the NSNP and the intransigence of educational and nutritional outcomes in the face of attempts at improving the living conditions of the poorest South Africans, this further research is imperative. Summarily, the hope is that this paper provides a springboard for further research into the topic and aims to adhere to the maxims of evidence based policy change and collaborative research which have increasingly become hallmarks of a modern microdevelopmental literature that emphasises the verisimilitude and internal validity of experimental and quasi-experimental methods.

A

Table 3: Department of Healths provincial targeting strategies for identifying vulnerable schools (*Source: Kallmann (2005)*)

Province	Criteria
Eastern Cape	<ul style="list-style-type: none"> • All learners in all farm schools • Grade R learners to Grade 4 learners in 'needy' primary schools • Targeting guidelines, indicators/criteria not given
Free State	<ul style="list-style-type: none"> • Priority given to: <ul style="list-style-type: none"> – farm schools – schools in informal settlements; and – schools in small towns • Use percentage of learners paying development funds (school fees) and other donations as indicators to identify 'needy' schools • Feed all learners in targeted schools
Gauteng	<ul style="list-style-type: none"> • Invite all registered primary schools to apply. • All primary schools that apply receive school feeding. • Teachers identify needy children based on criteria such as financial status and nutritional status.

Table 3: Department of Healths provincial targeting strategies for identifying vulnerable schools (*Source: Kallmann (2005)*)

Province	Criteria
Kwazulu-Natal	<ul style="list-style-type: none"> • Feed in farm and rural schools and schools in informal settlements. • Feeding in township schools takes place according to the following criteria: <ul style="list-style-type: none"> – Majority of children come to school hungry. – High absenteeism rate. – Majority of learners are unable to pay school fees. – Majority of learners are not able to bring food boxes to school. – Learners come from homes that depend on a social grant for survival. – General lack of concentration and participation in school activities.
Limpopo	<ul style="list-style-type: none"> • Feeds all learners in rural a peri-urban schools
Mpumalanga	<ul style="list-style-type: none"> • Feed schools in poverty-stricken areas, including farming, rural, deep rural and informal settlement areas. • Use indicators and departmental assessments in collaboration with Department of Education: <ul style="list-style-type: none"> – social problems; – unemployment; – disease; – poor school performance; and – dropping out

Table 3: Department of Healths provincial targeting strategies for identifying vulnerable schools (*Source: Kallmann (2005)*)

Province	Criteria
Northern Cape	<ul style="list-style-type: none"> • Give priority to rural schools. • Feed all learners in participating rural schools. • Phasing out urban schools/feed only 50% of learners in participating urban schools. • Use results of 1994 anthropometric survey in primary schools to identify 'needy' schools
North West	<ul style="list-style-type: none"> • Schools in geographic areas with a poverty level of 70% and above are eligible for school feeding. • Schools in areas where the poverty gap is below the cut-off point (70%) but where there are pockets with poverty gaps of 70% or more are identified through a nutrition situation analysis taking into account community inputs/discussions and variables such as: <ul style="list-style-type: none"> – nutritional indicators; – vital statistics; and – household food security indicators • All rural and farm schools and schools in informal settlement areas are potentially eligible to school feeding. • The poverty gap is applied in a realistic and flexible manner, i.e. the cutoff point is not applied strictly and in isolation to other variables. • The maximum number of children to be fed is limited to the budget available, using the following formula: $\text{School feeding budget}/\text{number of feeding days}/\text{standardised cost} = \text{maximum number of children to be served}$. • The number of feeding days or quantity and quality of menu options will not be compromised to feed more children. Doing this would adversely affect the impact of school feeding.

Table 3: Department of Healths provincial targeting strategies for identifying vulnerable schools (*Source: Kallmann (2005)*)

Province	Criteria
Western Cape	<ul style="list-style-type: none"> • Schools are targeted according to the classification of the Poverty Index, used by the Department of Education • The Poverty Index classifies schools on a scale of 0 to 1, with 1 being the most impoverished • The province feeds all the learners in the poorest schools (which includes all rural and farm schools), half the children in slightly better-off schools, and a quarter of the children in the remaining schools with a poverty index above the threshold of 0.5.

B

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Table 4: Anthropometrics of Children Aged 6 months to 14 years - South Africa 2008 (Source: *Arthington & Case (2008)*)

	Underweight	Normal	Overweight	Obese	Stunted	Underweight for Age	Wasting	Missing Height or Weight	Number Valid
Population group									
African	0.133	0.475	0.12	0.098	0.176	0.096	0.048	0.181	5982
Coloured	0.128	0.362	0.082	0.146	0.195	0.129	0.038	0.32	816
Indian	0.224	0.311	0.12	0.024	0.084	0.125	0	0.327	65
White	0.149	0.356	0.131	0.099	0.076	0.02	0.07	0.285	143
Location									
Rural	0.149	0.507	0.118	0.088	0.193	0.101	0.042	0.143	4370
Urban	0.123	0.409	0.117	0.113	0.149	0.09	0.052	0.253	2636
Per capita income									
Quintile 1	0.142	0.473	0.11	0.083	0.196	0.102	0.064	0.196	1951
Quintile 2	0.149	0.468	0.121	0.09	0.184	0.104	0.054	0.178	2142
Quintile 3	0.128	0.48	0.112	0.109	0.174	0.114	0.036	0.183	1570
Quintile 4	0.135	0.47	0.111	0.112	0.131	0.078	0.019	0.182	889
Quintile 5	0.11	0.337	0.141	0.134	0.13	0.05	0.041	0.307	454
Age									
0-4	0.083	0.346	0.169	0.156	0.239	0.087	0.047	0.264	2079
5-9	0.141	0.517	0.091	0.07	0.12	0.102		0.185	2374
10-14	0.174	0.485	0.1	0.09	0.172			0.16	2553
Sex									
Female	0.128	0.455	0.123	0.105	0.158	0.099	0.048	0.199	3473
Male	0.143	0.456	0.112	0.095	0.184	0.092	0.046	0.201	3533
Total	0.136	0.456	0.117	0.1	0.171	0.096	0.047	0.2	7006

Figure 5: BMI-for-Age

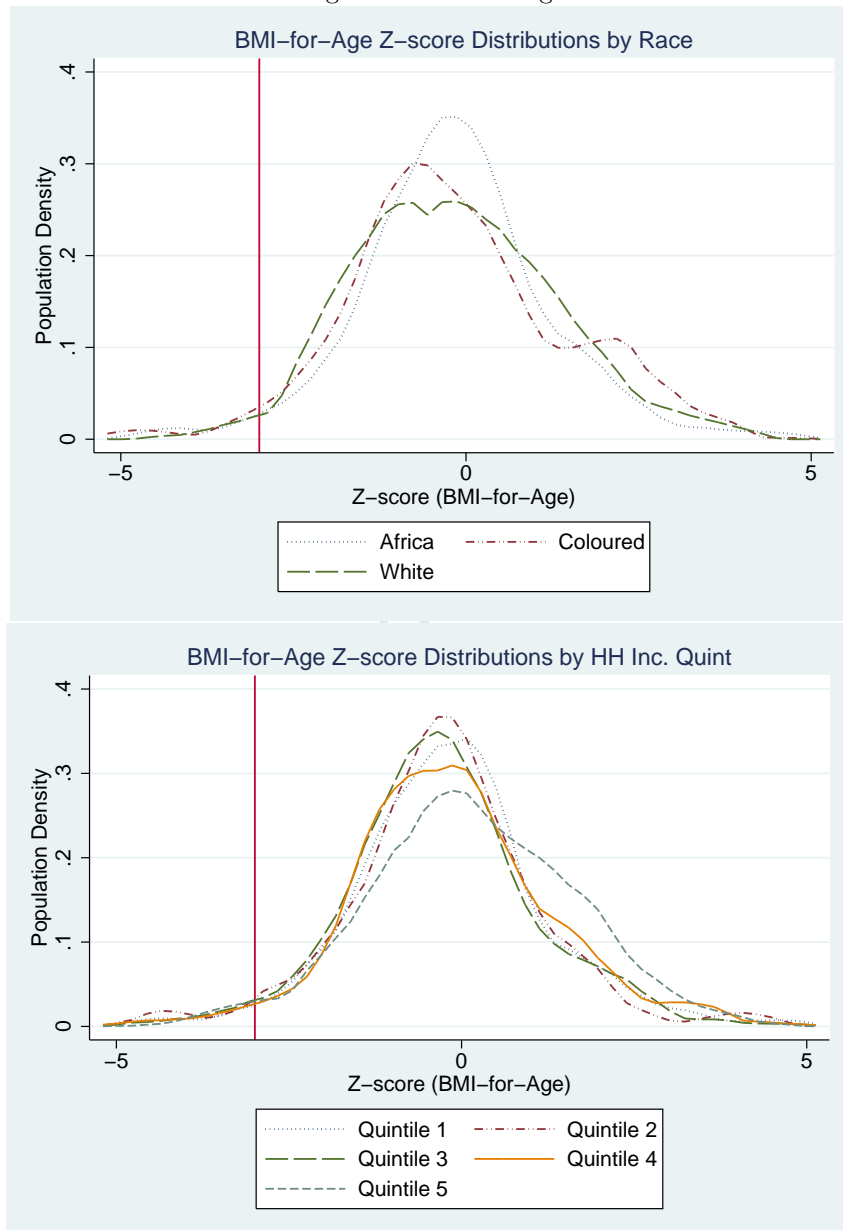


Figure 6: Height-for-age

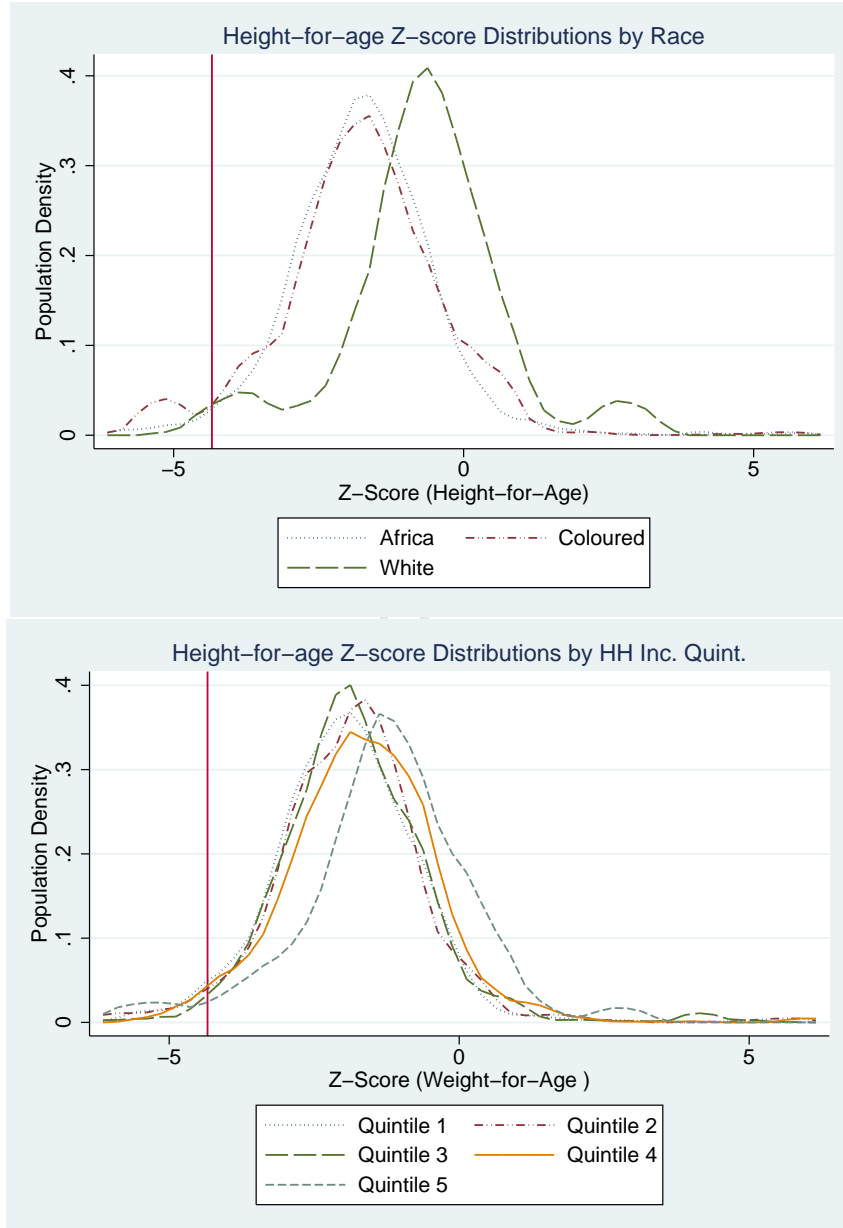


Figure 7: Weight-for-age

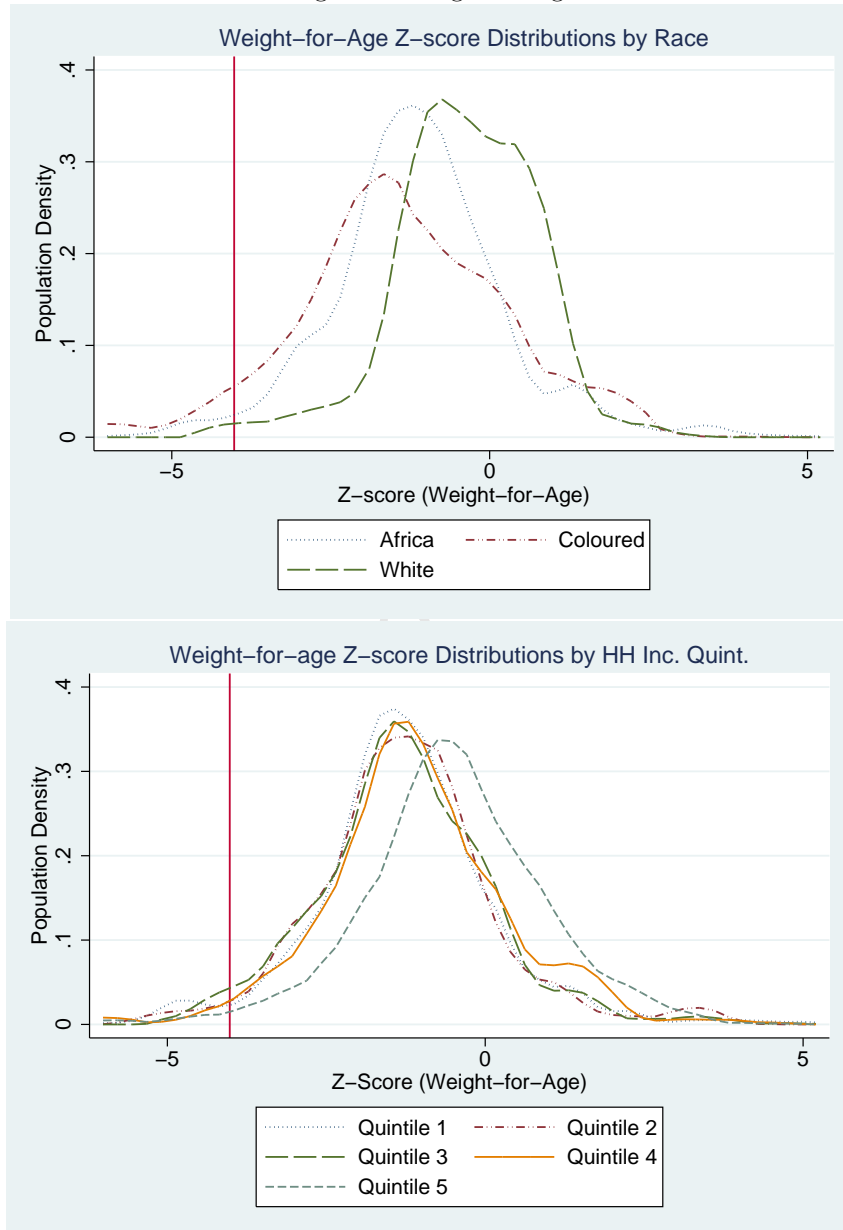


Table 5: Randomised Evaluations of School Meal Programmes (Source: *McEwan (2010)*)

Kazianga et al. (2009)	Burkina Faso (northern)	2006 (1 school year)	Primary, ages 6-15	Lunch; take home ration of 10 kg flour/month per girl	15 schools (lunch); 16 schools (take home); 14 control schools	Enrollment: n.s. (lunch); n.s. (rations) Attendance: -.7 days (lunch); n.s. (rations) Math test: n.s. (lunch and rations) WISC\Raven's and digital span tests: n.s.
Alderman et al. (2008);	Uganda	2005 (1 year)	Ages 6-17	Snack and lunch (1049 kcal); take-home ration equivalent to school meals	11 refuge camps (meals); 10 camps (take-home rations); 10 control camps	Enrollment: n.s. Morning attendance: n.s. Afternoon attendance: 9.3 percentage points (meals) Enrollment age and grade repetition: n.s. Math, literacy, Ravens, Digit Span Forward tests: n.s.
Vermeersch and Kremer (2005)	Kenya (Busia/Teso districts)	2000 (2 school years)	Preschool, ages 4-6	Breakfast (422 kcal)	25 treatment schools; 25 control schools	Digit Span Backward test: positive effects for both treatments (no s.d. reported) School participation: 8.5 percentage points (1 yr.) and 5.5 percentage points (2 yrs.) Oral cognitive and oral/written curriculum tests: n.s.
Whaley et al. (2003)	Kenya (Embu district)	1998 (2.33 school years)	Primary, Grade 1	Snacks (240-313 kcal); meat-based, or milk-based, or energy-based Breakfast (576-703 kcal)	3 schools in each of 3 treatment groups; 3 control schools	Ravens: .13 s.d./year (meat-based); other groups n.s. Verbal test: n.s. Math test: .11 s.d./year (meat-based); .15 s.d./year (energy-based); milk-based n.s.
Powell et al. (1998)	Jamaica (rural)	1994 (1 school year)	Primary, Grades 2-5	Breakfast (600 kcal)	Child randomization within 16 schools	Attendance: 2.3 percentage points Reading, spelling, and math tests: n.s.
Jacoby et al. (2006)	Peru (Huaraz)	1993 (12 school days)	Primary, Grades 4-5	Breakfast (600 kcal)	5 treatment schools; 5 control schools	Attendance: 3.5 percentage points Reading, vocabulary, math, and coding tests: n.s.

Table 6: This table makes reference to Adelman et al. (2008), Alderman et al. (2008), Whaley et al. (2003), Powell et al. (1998) and Jacoby et al. (1996)

Table 7: Mean mathematics and reading achievement, TIMSS and PIRLS studies (Source: Glewwe & Miguel (2008))

Country	Mathematics (TIMSS) 1999		Reading (PIRLS) 2001
	Grade 7	Grade 8	Grade 4
Japan	-	579	-
UK (England)	-	-	553
US	-	502	542
Argentina	-	-	420
Belize	-	-	327
Chile	-	392	-
Colombia	-	-	422
Indonesia	-	403	-
Iran	-	422	414
Jordan	-	428	-
Korea (South)	-	-	587
Kuwait	-	-	396
Malaysia	-	519	-
Morocco	337	-	350
Philippines	345	-	-
South Africa	-	275	-
Thailand	-	467	-
Tunisia	-	448	-
Turkey	-	429	449

C

C.1 Approximate Bias of the Simple Nonparametric Estimator Using a Rectangular Kernel

The approximate bias of the simple nonparametric estimator with bandwidth h can be best illustrated by taking the probability limits of each of the conditional means for treatment and control estimated under the special case in which we employ a rectangular kernel. Formally

$$\text{plim}[\hat{\mu}_r(\bar{S})] = \frac{\int_c^{c+h} \mu(x)f(x)dx}{\int_c^{c+h} f(x)dx} = \mu_r(\bar{S}) + \lim_{x \downarrow \bar{S}} \frac{\delta}{\delta x} \mu(s) \cdot \frac{h}{2} + O(h^2)$$

If this is combined with the corresponding probability limit of the estimated conditional mean of the control group, we can derive the bias

$$\text{plim}[\hat{\mu}_r(\bar{S}) - \hat{\mu}_l(\bar{S})] - \mu_c(\bar{S}) - \mu_l(\bar{S}) = \frac{h}{2} \cdot (\lim_{x \downarrow \bar{S}} \frac{\delta}{\delta x} \mu(x) + \lim_{x \uparrow \bar{S}} \frac{\delta}{\delta x} \mu(x)) + O(h^2)$$

The above equation shows that the bias is linear in bandwidth h , whereas nonparametric regression estimates in the interior of the support typically yield bias of order h^2 . Furthermore, given that the parenthesised part of the first term is unlikely to be zero if the cutoff rule is correlated with the outcome, which is likely, the bias for the simple kernel estimator is likely to be quite high (Imbens & Lemieux, 2008).

D

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Table 8: RDD Estimates for Different Bandwidths

	(1)	(2)	(3)	(4)	(5)	(6)
WEIGHT-FOR-AGE	-.75 s.d	-.5 s.d	-.25 s.d	Top of 3rd Quintile	+.25 s.d	+.5 s.d
Recommended BW	0.599 (0.466)	-0.297 (0.237)	-0.455* (0.234)	-0.257 (0.213)	-0.132 (0.265)	0.470 (0.289)
Half BW	0.487 (0.567)	-0.0683 (0.267)	-0.405* (0.242)	-0.305 (0.239)	-0.185 (0.387)	0.725** (0.366)
Double BW	0.341 (0.441)	-0.336 (0.223)	-0.428* (0.225)	-0.240 (0.209)	-0.0930 (0.255)	0.445 (0.284)
Observations	657	657	657	657	657	657
BMI-FOR-AGE	-.75 s.d	-.5 s.d	-.25 s.d	Top of 3rd Quintile	+.25 s.d	+.5 s.d
Recommended BW	0.331 (0.296)	-0.371 (0.236)	-0.528** (0.208)	-0.106 (0.230)	0.161 (0.307)	0.189 (0.294)
Half BW	0.702** (0.343)	-0.285 (0.257)	-0.559** (0.227)	0.0274 (0.292)	0.0114 (0.431)	0.352 (0.363)
Double BW	0.142 (0.280)	-0.387* (0.218)	-0.490** (0.199)	-0.0947 (0.212)	0.0311 (0.226)	0.186 (0.281)
Observations	764	764	764	764	764	764

Standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Table 9: RDD Estimates for Rectangular Kernels

	(1)	(2)	(3)	(4)	(5)	(6)
CUTOFF	-0.75 s.d	-0.5 s.d	-0.25 s.d	Top of 3rd Quintile	+0.25 s.d	+0.5 s.d
Weight-for-age Z-score	0.763 (0.479)	-0.251 (0.242)	-0.390 (0.239)	-0.270 (0.210)	-0.0854 (0.260)	0.370 (0.288)
Observations	657	657	657	657	657	657
CUTOFF	.zbf	.zbf	.zbf	.zbf	.zbf	.zbf
BMI-for-age Z-score	0.233 (0.307)	-0.331 (0.239)	-0.544*** (0.211)	-0.0851 (0.225)	0.237 (0.285)	0.143 (0.299)
Observations	764	764	764	764	764	764

Standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

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