Economics of Education Review 32 (2013) 122–139

Contents lists available at SciVerse ScienceDirect

Economics of Education Review

journal homepage: www.elsevier.com/locate/econedurev

The impact of Chile's school feeding program on education outcomes

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A R T I C L E I N F O

Article history:
Received 19 May 2012
Received in revised form 27 August 2012
Accepted 27 August 2012

JEL classification:
I25
I38

Keywords:
Chile
School feeding programs
Regression-discontinuity
Impact evaluation

A B S T R A C T

Chile operates one of the oldest and largest school feeding programs in Latin America, targeting higher-calorie meals to relatively poorer schools. This paper evaluates the impact of higher-calorie meals on the education outcomes of public, rural schools and their students. It applies a regression-discontinuity design to administrative data, including school enrollment and attendance, first-grade enrollment age and grade repetition, and fourth-grade test scores. There is no evidence, across a range of specifications and samples, that additional calories affect these variables. The paper suggests that the focus of Chilean policy should further shift to the nutritional composition of school meals, rather than the caloric content.

1. Introduction

Governments and development organizations devote substantial resources to the provision of free school meals to poor children, in both less-developed and richer countries. In 2008, the World Food Programme (WFP) operated school feeding programs in 68 poor countries, including most of Africa (WFP, 2008). In comparatively higher-income countries of Latin America, school feeding programs are just as common, and more likely to be funded and operated on a large scale by government agencies (Bundy et al., 2009). And, in the U.S., the National School Lunch Program (NSLP) subsidizes meal provision in 99% of public schools, with participation of more than half of students (Currie, 2003; Schanzenbach, 2009). Despite the ubiquity of school feeding programs, we know surprisingly little about their causal impact on education outcomes, especially academic achievement. This is especially true of the mature, large-scale school feeding programs in developed countries.

In the U.S., Hinrichs (2010) uses a 1960s modification of the NSLP funding formula to find that it had long-run effects on school attainment.[1] Using Virginia data, Fgilio and Winicki (2005) find that schools threatened with accountability sanctions tended to increase the caloric content of meals – but not other nutrients – and that this may have boosted high-stakes test scores. The effect could stem from a short-run effect of glucose levels on student cognition. Using a difference-in-differences strategy, Belot and James (2011) found that a British program that affected the nutritional content of school meals in a single borough of London apparently raised test scores and lowered absences.

In poorer countries, Afridi (2011) finds that a national meal program in India led to attendance increases among girls (but not boys), while He (2009) finds that a Sri Lankan

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[1] A larger literature focuses on the health outcomes of the U.S. breakfast and lunch programs. For example, Hinrichs (2010) finds no long-term health impacts; Bhattacharya, Currie and Haider (2006) finds that the School Breakfast Program increases nutrient intake but not calories, using a sibling difference approach; and Schanzenbach (2009) finds that school lunches increase obesity rates using a discontinuity strategy.

0272-7757/$ – see front matter
http://dx.doi.org/10.1016/j.econedurev.2012.08.006

national program led to increased enrollments, but that the increases were apparently the result of existing students sorting between treated and untreated schools. Randomized evaluations of small-scale interventions in very poor settings – usually WFP or researcher-initiated programs in Africa – show some effects on attendance rates, fewer on enrollments,[2] and from zero to small effects on measures of cognitive ability and academic achievement.[3]

This paper adds to the literature by rigorously evaluating one of the oldest, largest, and, some argue, most efficiently managed school feeding programs. In 2002, the World Food Programme selected Chile's School Feeding Program, operated by the government agency JUNAEB, as one of the top-five in the world.[4] JUNAEB has operated since 1964, with the explicit goal of improving education outcomes of low-income children attending publicly funded schools (JNUAEB, 2005; Kain, Uauy, & Taibo, 2002; Vial, Muchnik, & Kain, 1991). In recent years, JUNAEB has delivered free meals to one-third of primary school students, targeting schools using a poverty index. The poorest schools receive meal rations with relatively higher caloric content. The targeting rule is an implicit judgment that caloric intake is deficient among these students.

This paper leverages discontinuous variation in the food calories of meal rations to identify impacts on a broad set of primary education outcomes that include enrollment, grade repetition, attendance, and test scores on the national fourth-grade assessment. In the 2000 school year, the agency calculated a school-level "vulnerability" index (scaled 0–100) and used it to assign one of several types of meal rations during the 2001–2005 school years. Schools with an index of 68 or higher received individual meal rations of 1000 kcal/day. Schools with an index of less than 68 (but 20 or higher) received rations of 700 kcal/day. Schools with an index of less than 20 received one of several options, from zero to 700 kcal/day, further depending on conditions described in Section 4. The proportion of students within each school allocated meal rations varied continuously with the index, with more rations provided to poorer schools.

The rules unwittingly facilitated a regression-discontinuity design. Using administrative data from JUNAEB, I verify that actual program assignment followed the stated rules. In 2001–2005, the food calories of school meal rations increased sharply in the vicinity of each cutoff, although the proportion of students allocated meals near each cutoff was similar. Thus, this paper's estimates reflect the impact of providing meals with *different* caloric contents to *similar* proportions of students within schools. The estimates at the higher cutoff (68) are larger (about 170 food calories), more robust to alternate specifications, and more precisely estimated. Hence, this paper focuses on the local experiment at 68, in a sample dominated by rural, public schools. The magnitude of the treatment represents, on average, over 10% of the recommended daily caloric intake for children in early primary grades.[5]

I compare the education outcomes of schools and their students on either side of the assignment cutoff, using a rich set of administrative data from JUNAEB and the Ministry of Education collected between 2001 and 2005. Dependent variables measured at the school level include enrollment in grade 1, enrollment in all primary grades 1–8, and average daily attendance. Dependent variables measured at the student level include the first-grade enrollment age and grade repetition of multiple cohorts of first-graders, as well as the fourth-grade test scores of the 2002 cohort of first-graders. Overall, the results provide no evidence that an exogenous increase in food calories affected a range of primary education outcomes.

The results are perhaps not surprising. Chile has attained universal primary school enrollment, and there is a little scope for treated schools to increase enrollments, unless accomplished by poaching students from other schools (He, 2009). And though more children suffered from caloric deficiencies during JUNAEB's early history, recent data suggest that rates of child wasting and stunting are low, and that rates of childhood obesity have reached levels of developed countries (Kain, Uauy, Lera, Taibo, & Albala, 2005; Kain, Lera, Rojas, & Uauy, 2007; Musgrove, 1993; Uauy & Kain, 2002). Also like developed countries, Chile is confronting a perceived stagnation in academic performance, but widespread child malnutrition cannot fully explain these patterns. Indeed, Chile has focused on reforms to the structure of educational markets – such as vouchers – and on direct investments in the quality of instruction (McEwan, Urquiola, & Vegas, 2008).

Still, the Chilean case is interesting because it provides a cautionary tale of what might occur in developing countries that expand popular feeding programs while maintaining a focus on addressing caloric deficiencies – even as rates of child malnutrition and stunting decline. The results suggest that continuing to provide supplemental calories is unlikely to affect education outcomes. On the other hand, limited evidence suggests that a renewed focus on the nutritional

[2] There are no full-sample enrollment effects in Uganda and Burkina Faso (Alderman, Gilligan, & Lehrer, 2008; Kazianga, de Walque, & Alderman, 2009). In Burkina Faso there are large enrollment effects in the subsample of girls and, in Kenya, substantial effects on school participation (Vermeersch & Kremer, 2005). Attendance effects are slightly more consistent. Besides the Kenyan effects on school participation, there are some positive attendance effects in the Jamaican, Peruvian, and Ugandan experiments, but negative effects in the Burkina Faso study (Jacoby et al., 1996; Powell et al., 1998).

[3] In full sample estimates, there are no statistically significant effects in Burkina Faso (Kazianga et al., 2009), Kenya (Vermeersch and Kremer, 2005), Jamaica (Powell et al., 1998), or Peru (Jacoby et al., 1996). Another Kenyan experiment finds modest but statistically significant effects of some treatments on some tests (Whaley et al., 2003), and a Uganda study finds effects on one test instrument among several (Alderman et al., 2008). Vermeersch and Kremer (2005) do find large effects on curriculum-based tests when teachers are more experienced, and Adelman, Alderman, Gilligan, and Lehrer (2008) and Adelman, Gilligan, and Lehrer (2008) find math and literacy effects among age-based subsamples.

[4] In 2004, JUNAEB was a founding member of the Latin American Feeding Network (www.larae.org), headquartered in Santiago. Chile's combinational auction system for awarding territorial meal contracts to private firms has received international accolades (Catalán et al., 2009; Epstein et al., 2002).

[5] Moderatively active girls and boys, ages 4–8, require 1400–1600 calories per day according to recent guidelines (U.S. Department of Agriculture & U.S. Department of Health and Human Services, 2010).

quality of meals could have such an effect on learning (Belot & James, 2011). More disconcertingly, simply providing more calories plausibly contributes to rising rates of childhood obesity. In the U.S., Schanzenbach (2009) finds that increases of as little as 40 calories per day in school lunches could increase obesity rates by 2 percentage points. There is no comparable estimate in Chile, but the mean increase of about 170 calories in the poorest schools provides cause for concern. The national debate in Chile has shifted to the causes and consequences of childhood obesity, but JUNAEB faces challenges in modifying an enormously popular social program.

The paper proceeds in Section 2 by reviewing empirical literature on the potential impact of school meals on education outcomes. Section 3 provides background on Chile's school system and child feeding programs. Section 4 provides a detailed description of the JUNAEB's school feeding program, and describes its assignment to schools and children. Sections 5 and 6 review the regression-discontinuity design and data, respectively. Section 7 presents the empirical findings, while Section 8 summarizes and concludes.

2. The potential impact of school feeding on education outcomes

There are three pathways by which school meals could affect student learning (for related overviews, see Alderman, Gilligan, & Lehrer, 2008; Bundy et al., 2009). First, in-school meals are a conditional transfer to children. Thus, school meals may induce families to enroll their children in school, to enroll their children sooner, or, conditional on enrollment, to encourage regular attendance. The availability of meals could also produce unexpected consequences, especially when not all schools provide free meals and when few rules govern school attendance zones and fees. For example, families may transfer between treated and untreated schools, and rising demand for treated schools could increase fees (He, 2009; Vermeersch & Kremer, 2005). Section 3 confirms that enrollment rates are high in Chile, suggesting that any increased enrollments due to school meals are likely the result of student sorting between schools. There is potentially greater scope for meals to increase regular attendance, conditional on enrollment.

Second, there is evidence that short-term attention and memory are adversely affected by skipping breakfast. Laboratory experiments suggest that even "empty calories" instead of a morning fast can improve short-term functioning among children and young adults (Pollitt, Cueto, & Jacoby, 1998). If energy sources contribute to more effective work during the school day, then they could produce sustained gains in test scores. In apparent recognition of this, Virginia schools increased the calories of school meals on high-stakes testing days, although this short-term response was more characteristic of a "gaming" response, rather than an investment in human capital (Fgilio & Winicki, 2005). Overall, this literature implies that the timing of meals during the school day is relevant to understanding the potential impact in Chile, a point to which I return in the next section.

Third, sustained exposure to nutritious food could prevent or ameliorate malnutrition. Child nutrition, in turn, could affect learning via increased attendance, cognitive development, or academic achievement (see Glewwe, 2005 for a review of the mostly non-experimental evidence). Nutritional gains could be blunted if supplemental calories or nutrients do not "stick" to children because households respond by reallocating across their members, but the best evidence suggests that at least half the calories and nutrients are actually received by children.[6] As described in the introduction, this paper's analyses focus on the effects of increased calories, rather than a qualitative change the nutritional content of meals. Given this, it is possible that higher-calorie meals contribute to higher rates of obesity and worse child health (Schanzenbach, 2009).

3. Schools and feeding programs in Chile

3.1. Background on Chilean schools

The Chilean school calendar is March to November, with approximately 180 days of instruction. Schooling is divided into three levels: pre-primary, 8 years of primary or "basic" education (ages 6–13); and 4 years of secondary education (ages 14–17). In 2000, the baseline year of this study, only primary education was mandatory. Table 1 confirms that net primary enrollment rates are generally over 98% of appropriately aged children.[7] Pre-primary and secondary enrollment rates are lower: 55% of 4–5 year olds are enrolled in some form of pre-primary, and 80% of 16–17 year olds are enrolled in secondary.

A 1980s reform decentralized control of national public schools to more than 300 municipal governments (McEwan et al., 2008). The revenues of municipal (public) schools and a new category of private subsidized schools were determined thereafter by a per-student subsidy multiplied by average attendance. Private subsidized schools are managed by a diverse group of for-profit and not-for-profit organizations, both religious and secular. A third, smaller category of private tuition schools receives no government subsidy, charges tuition, and does not participate in government programs.

3.2. Child feeding programs

The Chilean government operates two food distribution programs for children. First, the Ministry of Health administers the National Complementary Feeding Program (PNAC, or the *Programa Nacional de Alimentación Comple-*

[6] Jacoby (2002) finds that child caloric intake in the Philippines rises by essentially the same amount as feeding program calories, albeit less so among poorer families. In Guatemala, households reduced the caloric intake of children by about half the amount of a food supplement, but 80% of the supplement's protein was received by children (Islam and Hoddinott, 2009). Afridi (2010) finds that Indian families reduce children's caloric intake in the home, but that overall children's daily nutrient intake increases by 49–100% of the in-school transfers.

[7] The lower enrollment rate among 6–7 year-olds is an artifact of household survey's timing, since many surveyed 6 year-olds in November 2000 – the end of the school year – had not been eligible to enroll in the first grade at the beginning of the year (McEwan and Shapiro, 2008).

Table 1
Net school enrollment and program participation in 2000.

Age group	Percent of age group enrolled in school			Percent of age group participating in feeding program			
					Free school meal(s) in:		
	Pre-primary	Primary (grades 1–8)	Secondary	PNAC	Pre-primary	Primary (grades 1–8)	Secondary
0–1	2.7%	–	–	81.5%	1.4%	–	–
2–3	18.9%	0.1%	–	69.1%	9.5%	–	–
4–5	54.6%	1.4%	–	59.5%	22.6%	0.5%	–
6–7	21.7%	75.4%	–	–	5.7%	27.0%	–
8–9	–	99.4%	–	–	–	37.3%	–
10–11	–	99.3%	–	–	–	41.4%	–
12–13	–	98.1%	0.4%	–	–	39.5%	0.0%
14–15	–	44.5%	49.9%	–	–	18.8%	12.0%
16–17	–	5.1%	80.3%	–	–	2.6%	20.9%
18–19	–	0.8%	37.1%	–	–	0.4%	10.2%

Source: CASEN household survey, 2000, and author's calculations.
Note: Survey weights are applied. "Free meals" may include PAE or another intervention.

mentaria). PNAC provides take-home food rations to all children between 0 and 5 years old, as well as pregnant and nursing women (Kain & Uauy, 2001; Uauy & Kain, 2002). PNAC rations are only available to families obtaining regular check-ups at Ministry-operated health centers, leading to self-exclusion among higher-income or privately insured families. Even so, program participation is substantial: 82% of children ages 0–1 had obtained PNAC rations in the three months prior to a household survey, declining to 60% among the 4–5 year-old cohort (Table 1).

Second, the National Board of School Assistance and Scholarships (JUNAEB), an independent unit of the Ministry of Education, operates the School Feeding Program or *Programa de Alimentación Escolar* (PAE).[8] In 1980, JUNAEB began contracting with private suppliers of schools meals, later implementing a combinational auction to award meal contracts to competing private firms (Epstein, Henríquez, Catalán, Weintraub, & Martínez, 2002). JUNAEB nutritionists establish minimum standards for meals, including (1) food calories and nutritional content; (2) food structure, including minimum or maximum frequencies of certain foods; (3) quality thresholds of ingredients; and (4) minimum acceptable operating conditions and food service infrastructure. Recipient schools are divided into territorial units, and firms can submit multiple bids for 1–8 territorial units, which are accepted or rejected in their entirety. A linear programming model ultimately identifies the least-cost combination of meal providers (Catalán et al., 2009).

The food calories of daily meal rations vary across schools (JUNAEB, 2000; Kain et al., 2002). In 2000, pre-primary schools were eligible to receive breakfast and lunch (or lunch and a snack) with a total of 700 kcal/day. Primary schools were eligible to receive one of three options: (1) breakfasts with 250 kcal/day; (2) breakfast/lunch or lunch/snack combinations with 700 kcal/day; and (3) breakfast/lunch or lunch/snack combinations with 1000 kcal/day. Finally, secondary schools were eligible for breakfasts of 350 kcal/day or lunches of 650 kcal/day. As Section 2 noted, the timing of school meals during the day is potentially important, and the meal combinations for options (2) and (3) imply different timing. At the time of this study, Chilean schools operated on a mix of half-day shifts (morning or afternoon) and full-day shifts, and the timing of meals was apparently determined by shifts. Further below I demonstrate that the proportion of schools using the full-day shift, among other school attributes, was similar across schools with lower- and higher-calorie rations. Thus, the effect of higher-calorie meal rations can be considered an average effect across schools with different shift schedules.

Table 1 reports the percentage of age cohorts that received free school meals in 2000, using household survey data. Overall, 23% of 4–5 year-olds receive free meals (or 41% of children attending pre-primary education). Thirty-seven percent of 8–9 year-olds receive free meals (38% of the cohort attending primary school), and 21% of 16–17 year-olds receive free meals (26% of children attending secondary school). This paper assesses PAE in primary schools.

4. Assignment of PAE to primary schools and students

4.1. Assignment to primary schools

Three criteria govern primary school participation in PAE. First, publicly funded schools are eligible, excluding the roughly 8% of students attending private tuition schools. Second, publicly funded schools must participate in an annual First Grade Survey used to calculate a vulnerability index (V), scaled from 0 (least vulnerable) to 100 (most vulnerable). The index is a weighted average of socioeconomic and anthropometric variables derived from survey data on first-graders, collected in the first two months of every school year (Kain et al., 2002). Some private subsidized schools never participate in the survey and exclude themselves from PAE eligibility. Table 2 examines the first two criteria using administrative data from 2000. Of 8727 primary schools in Chile, 80% offer some variant of PAE (see panel A). However, coverage is nearly universal among the population of municipal primary schools (97%), less so among private subsidized schools (63%), and almost nil among private tuition schools.

[8] JUNAEB is the Junta Nacional de Auxilio Escolar y Becas.

Table 2
School meals availability among primary schools and students in 2000.

	Municipal schools	Private subsidized schools	Private tuition schools	All schools
Panel A: School-level estimates from administrative data				
School receives PAE	96.8%	62.8%	0.5%	79.7%
Number of schools (percent of all schools)	5578 (63.9%)	2476 (28.4%)	673 (7.7%)	8727 (100%)
Panel B: Student-level estimates from administrative data				
Student receives PAE	45.8%	20.3%	0.1%	33.1%
Millions of students (percent of all students)	1.33 (56.5%)	0.84 (35.7%)	0.18 (7.8%)	2.36 (100%)
Panel C: Student-level estimates from a household sample				
Student receives free meal(s)	54.5%	23.5%	2.5%	39.3%
Millions of students (percent of all students)	1.36 (55.3%)	0.86 (36.6%)	0.19 (8.1%)	2.41 (100%)

Source: Panels A and B: JUNAEB master file, 2000; Ministry of Education enrollment file, 2000; and author's calculations. Panel C: CASEN household survey, 2000, and author's calculations.
Notes: Panels A and B: sample excludes schools devoted to special education. Panel C: sample includes all children, ages 5–18, that attend basic education (grades 1–8), excluding special education students; survey weights are applied.

The third criterion for school assignment is the value of V. JUNAEB assigns a single meal type to primary schools: no meals, 250 kcal/day, 700 kcal/day, or 1000 kcal/day. Schools receive higher-calorie options if V exceeds cutoff values. Although V is calculated annually, JUNAEB does not necessarily re-assign meal programs in each year, in order to maintain program continuity for schools, families, and providers. However, all newly opened schools are allocated PAE on the basis of the first available measure of V. In 2000, V was used to assign school participation for the schools years 2001–2005. Schools with $V \geq 68$ received 1000 kcal meals, and schools with $20 \leq V < 68$ received 700 kcal meals. Schools with $V < 20$ received 700 kcal meals if they received PAE in 2000 and participated in the Full School Day program in 2000; or they received PAE in 2000, did not participate in the Full School Day program in 2000, and the 1999 value of V exceeded 26.[9] Schools with $V < 20$ received 250 kcal meals if they received PAE in 2000, did not participate in the Full School Day program in 2000, and the 1999 value of V was less than 26. Finally, schools with $V < 20$ received no meal if they did not receive PAE in 2000.

Fig. 1 verifies that the assignment rules were mostly followed by JUNAEB. In all panels, the x-axis variable is the vulnerability index in 2000, V, and the sample includes 21,562 school-by-year observations pooled across treatment years in 2001–2005. The small circles indicate means of the y-axis variable within non-overlapping, one-point bins of V. In the top panels, the y-axis variable is the number of food calories (in hundreds) of school meal rations. The solid lines are fitted values from 4th order polynomials of V, estimated separately on either side of the assignment cutoff. Both top panels visually confirm that the food calories of rations increase sharply in the vicinity of each cutoff. The size of the discontinuity is much smaller at 20, consistent with assignment rules that "grandfathered" many schools into the 700 kcal/day ration, even with a V below 20.

The figure, and subsequent analyses, assume that meal rations contained the actual number of food calories recorded by JUNAEB. There is no way of verifying this, although strong incentives imply it. First, the for-profit firms that provide meals have no incentive to provide higher-calorie rations to schools with lower-calorie options. Second, the firms have incentives to under-provide, but parents and school officials can easily communicate discrepancies to JUNAEB, and firms are subject to a regular re-bidding process that penalizes bad performance.

4.2. Assignment to students within primary schools

Once a meal ration is assigned to schools, JUNAEB determines the proportion of students within schools that are allocated meals. The proportion is a continuous function of V, with higher proportions awarded to more vulnerable schools.[10] Before 2006, the distribution of meals to students within schools was at the discretion of local school administrators and unobserved by researchers (more recently, student eligibility is determined by a student-level assignment variable, an important topic for future research). The bottom panels of Fig. 1 verify that the proportion of students receiving meals within schools increases with V. As expected, it does not appear to increase sharply at either cutoff, although the proportion of students receiving rations is much higher (around 0.9) near the cutoff at 68. Viewed as a whole, Fig. 1 clarifies the school-level treatment that is being evaluated. The paper estimates the impact of providing meals with *different* caloric contents to *similar* proportions of students within schools.

[9] The Full School Day program, initiated in 1997, increased annual instructional by 232 h in grades 3–6 and 145 h in grades 7–8 (Cox, 2004; McEwan, 2008). It was eventually applied to all state-subsidized schools, but especially among municipal and rural schools in early years of operation.

[10] Rural schools and schools participating in the Full School Day program are also typically awarded a larger proportion of free meals, although I will confirm that neither variable changes sharply at the assignment cutoffs.

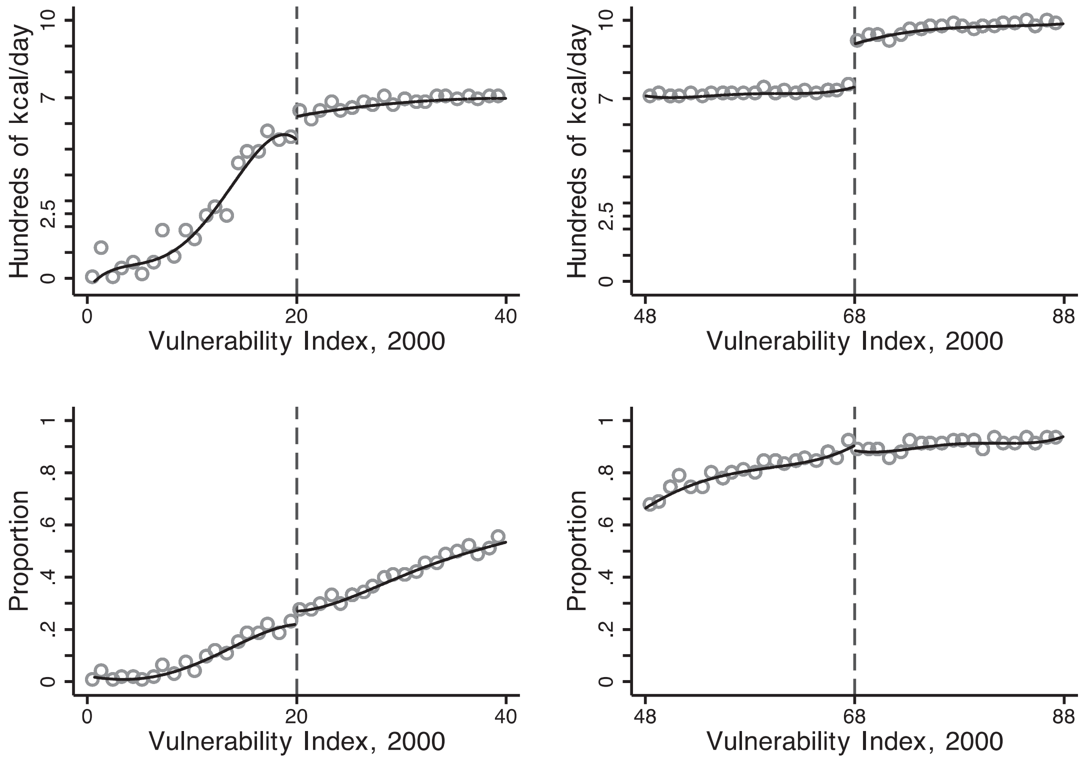

Fig. 1. Meal assignment (pooled school observations in 2001–2005) Note: "Hundreds of kcal/day" indicates the food calories of the daily meal ration provided to a school (0, 250, 700, or 1000) in the 2001–2005 school years. "Proportion" indicates the proportion of students within schools allocated the daily meal ration in the 2001–2005 school years. In this and subsequent figures, circles are means of the y-axis variable within non-overlapping 1 point bins of the 2000 vulnerability index. Lines are fitted values from 4th order polynomials of the 2000 vulnerability index, estimated on either side of the respective cutoff.

4.3. PAE coverage in 2000

Table 2 summarizes the overall percentage of students receiving free meals in 2000, the combined result of school and student assignment. In panel B, administrative data show that 33% of Chile's 2.36 million primary students were allocated a meal ration by JUNAEB, ranging from 46% of municipal students to 0.1% of private paid students. In panel C, weighted estimates from a household survey corroborate these findings. A somewhat larger 39% of students report receiving a free meal in school, ranging from 55% of municipal students to 3% of private tuition students. The household survey question does not state whether the free meal was directly subsidized by JUNAEB. Conservatively, about one-third of Chile's primary students received school meals via PAE in 2000. The evidence confirms that JUNAEB disproportionately targets these meals to students attending state-subsidized schools and to schools with higher values of the vulnerability index (V).

5. Empirical strategy

5.1. Identification and estimation

Impact evaluations of school feeding programs typically begin with the regression:

$$O = \alpha + \gamma T + X'\beta + \varepsilon \tag{1}$$

where O is a outcome measured at the school level, such as enrollment or average attendance; T is a measure of school treatment status; and X is a vector of control variables such as student poverty (school-level subscripts are omitted). If program assignment occurs on the basis of omitted variables that also influence O, then $cov(T, \varepsilon) \neq 0$, and the treatment effect $\hat{\gamma}$ does not have the desired causal interpretation.

In Chile, discontinuous assignment rules introduce exogenous variation in schools' T. Henceforth I define T as daily food calories (in hundreds) of a school meal ration. I formally test for discontinuous variation in T at each cutoff by estimating:

$$T = \delta_x D_x + f(V) + \varepsilon, \quad \text{where} \quad x - h \leq V < x + h. \tag{2}$$

V is the 2000 vulnerability index; $D_x = 1\{V \geq x\}$ indicates values of the assignment variable (V) that exceed cutoffs at either $x = 20$ or $x = 68$; and $f(V)$ is a continuous function of the assignment variable. The regression is estimated in a pooled sample of school-level observations between 2001 and 2005, further defined by the cutoff x and bandwidth h.

I report estimates from three variants of this regression. First, a global polynomial specification assumes that $f(V)$ is a piecewise 4th order polynomial, and applies a large bandwidth of $x = 20$.[11] Fig. 1 already illustrated fitted values from this specification. Second, I report global estimates that control for baseline school variables, mainly to improve precision. Third, a local linear specification assumes that

[11] Specifically, $f(V) = \beta_0 + \beta_1 V + \beta_2 V^2 + \beta_3 V^3 + \beta_4 V^4 + D_x[\beta_5(V - x) + \beta_6(V - x)^2 + \beta_7(V - x)^3 + \beta_8(V - x)^4]$.

$f(V)$ is a piecewise 1st order polynomial, and applies a smaller bandwidth of $x=3$. In choosing this bandwidth, I was guided by the cross-validation procedure described in Lee and Lemieux (2010), and applied to school-level dependent variables. The optimal bandwidths were similar across a range of variables. In robustness checks, I confirm that results are not sensitive to the order of polynomial or the bandwidth.

To formally test for differences in outcomes measured at the school level, including enrollment and average daily attendance, I estimate the reduced-form regression:

$$O = \lambda_x D_x + g(V) + \nu, \quad \text{where} \quad x - h \le V < x + h, \qquad (3)$$

and apply the same procedures just described. Note that (λ_x/δ_x) is the effect of a 100-calorie increase in meal rations among schools induced to participate by a small change in V (assuming that Eqs. (2) and (3) are estimated with the same sample and polynomial specification of V). This paper does not report these estimates because the reduced-form estimates from Eq. (3) are generally not statistically distinguishable from zero.

5.2. Internal validity

The causal interpretation of $\hat{\lambda}_x$ rests on the assumption that $cov(D_x, \nu) = 0$. Presuming that $g(V)$ is correctly specified, the condition might be violated if agents can *precisely* manipulate values of the assignment variable V, which is calculated using data from an annual First Grade Survey. For example, highly motivated schools could solicit enrollments from poor students or alter individual survey responses to qualify for higher-calorie meals. For several reasons this is unlikely.

First, schools were unaware in early 2000 of the impending program reassignment in 2001. Second, even armed with such knowledge, it is unlikely that schools could precisely manipulate their program status. Schools were unaware of the precise assignment cutoffs or the statistical procedure used to aggregate student-level data into the final measure of V, both of which change over time (Kain et al., 2002). Schools might raise or lower V through a concerted effort at manipulation of student survey data, but the precise value of V relative to an assignment cutoff is noisy. This implies that final assignment resembles the "local" randomized experiment described by Lee (2008) and Lee and Lemieux (2010). Third, JUNAEB itself had little incentive to manipulate V, since the index is publicly available in Chile and widely scrutinized by schools and researchers.

Nonetheless, I apply two checks of internal validity implied by the research design. First, I examine the smoothness of baseline school variables around the cutoffs, by estimating Eq. (3) with baseline covariates as dependent variables. Second, I inspect a histogram of V for evidence of bunching of schools on either side of assignment cutoffs. The absence of bunching does not rule out manipulation (since schools could sort in both directions), but its presence suggests ex ante manipulation of V that could introduce imbalance in observed or unobserved background variables around the cutoffs.

5.3. Student-level regressions

I also estimate Eq. (3) with data on student-level outcome variables, including first-grade enrollment age, grade repetition, and test scores (the data are described in Section 6). There is an additional source of bias in these regressions. Suppose that higher-calorie meals cause higher first-grade enrollments in treated schools, relative to untreated schools. Student sorting may also change the observed or unobserved attributes of entering students, which may be correlated with student outcomes such as grade repetition and test scores. To assess this issue, I use student-level background variables from multiple years of the First Grade Survey – applied at the *beginning* of each school year – to directly assess whether incoming first-grade students differ substantially in their socioeconomic status or baseline nutritional status across treated and untreated schools. I further use these baseline student variables as controls in regressions that estimate effects on fourth-grade test scores.

6. Data

6.1. School variables

Table 3 reports descriptive statistics on school-level variables in 2000, the baseline year. The same variables are available in treatment years between 2001 and 2005, except attendance which is available from 2003 to 2005. Unless otherwise noted, all regressions pool observations across available treatment years, with standard errors clustered at the school level.

Administrative files of the Ministry of Education report: (1) whether the school is private; (2) whether the school is rural; (3) whether the school operates a kindergarten or preschool (inferred from positive enrollments in any preschool grade); and (4) the school's administrative region (from 1 to 13). A separate Ministry of Education file indicates whether the school participated in Chile's "Full School Day" program in 2000. JUNAEB administrative files include (1) each school's V in 2000, (2) the meal program in which the school participated in each year, and (3) the proportion of students in each school that were allocated a meal ration in each year.

Ministry of Education files further report school enrollment in grade 1 and school enrollments in all primary grades (1–8). Finally, school-level average daily attendance (first available in 2003) is recorded in subsidy payment files, since publicly funded schools receive monthly payments based on average attendance. There are evident incentives for schools to inflate attendance. However, school reports are subject to audits, and schools face fines if attendance is inflated. Moreover, it is unlikely that incentives to over-report differ sharply in the vicinity of assignment cutoffs.

The first column reports means on the universe of 8727 primary schools in Chile, while additional columns report means on subsamples of schools with a valid V in 2000, and thus eligible for formula-based assignment. This paper focuses on the subsample of schools within 20 points of the assignment cutoff at 68. In comparison to other schools,

Table 3
Summary statistics for primary school variables in 2000.

	All primary schools in 2000	Primary schools with a vulnerability index (V) in 2000		
		All	$0 \leq V < 40$	$48 \leq V < 88$
Municipal (public) school	0.64	0.74	0.52	0.83
Private subsidized school	0.28	0.26	0.47	0.17
Private tuition school	0.08	0.00	0.00	0.00
Rural school	0.53	0.62	0.03	0.88
Full school day in 2000	0.42	0.49	0.06	0.69
On-site preschool in 2000	0.12	0.15	0.23	0.09
Vulnerability index, 2000	–	57.3	23.8	70.4
		(24.1)	(10.4)	(10.9)
D_{20}	–	0.90	0.63	–
D_{68}	–	0.42	–	0.60
Meal program				
0 kcal	0.20	0.07	0.23	0.01
250 kcal	0.01	0.01	0.03	0.00
700 kcal	0.33	0.38	0.72	0.22
1000 kcal	0.46	0.54	0.03	0.77
Proportion receiving meals within school	0.59	0.69	0.28	0.86
	(0.39)	(0.33)	(0.22)	(0.20)
Enrollment, grade 1	32.6	31.5	76.4	11.7
	(42.2)	(41.6)	(48.0)	(16.3)
Enrollment, grades 1–8	269.9	260.4	626.7	98.2
	(337.8)	(334.3)	(368.2)	(138.9)
Average daily attendance	–	285.9	700.3	103.7
		(377.5)	(427.4)	(155.0)
No. of schools	8727	7440	1972	4469

Notes: Standard deviations are in parentheses for non-dichotomous variables. The sample sizes for the proportion receiving meals are, respectively, 8725, 7438, 1971, and 4468. The sample sizes for average daily attendance are, respectively, 7118, 1924, and 4309. Average daily attendance is from 2003, the first year in which data are available.

note that these schools are substantially more likely to be public, located in rural areas, and with relatively small enrollments (an average of 12 first-graders, compared with a sample-wide average of 33).

6.2. *Student variables*

Table 4 describes student variables extracted from the micro data of JUNAEB's First Grade Survey in 2000, conducted in the first two months of the school year. The surveys are also available in treatment years from 2001 to 2004. Unless noted otherwise, the student-level regressions use a pooled sample of student observations from all available treatment years, clustering standard errors at the school level.

The survey data include (1) years of mother's schooling, (2) a female indicator, (3) height-for-age Z-scores, and (4) weight-for-height Z-scores.[12] Height-for-age is a measure of nutritional deprivation during the first several years of life, while weight-for-height is a better indicator of recent nutritional deprivation. Because the survey occurs early in the school year, one cannot expect either anthropometric measure, but especially the less malleable height-for-age, to be affected by 0–2 months of the meal program.

I then calculated each student's first-grade enrollment age as the days elapsed between birth and March 1 of their first year enrolled in grade 1, divided by 365.25 (also see McEwan & Shapiro, 2008). The survey reports each first-grade student's national identification number. I use this number, along with the 1999–2004 surveys, to infer the first academic year that each student enrolled in grade 1. Similarly, I infer that a student repeated first grade if the same student appeared in a subsequent round of the First Grade Survey.

Finally, the Ministry of Education's SIMCE (*Sistema de Medición de la Calidad de la Educación*) applies tests to the population of fourth-grade students in some years. I matched the 2002 cohort of first-graders to 2005 fourth-grade test scores using national identification numbers. Not all students are present in the 2005 data, primarily because of grade repetition. Once matched, I obtained four test score variables. First, I derived an indicator of whether 2002 first-graders participated in the 2005 fourth-grade assessment, a proxy of grade repetition by the fourth grade. Second, I derived an indicator of whether fourth-grade test-takers switched schools between grades 1 and 4. However, in subsequent analyses, I always assigned students to their original, first-grade school, since switching could have been an endogenous response to meal program provision. This preserves the internal validity of the intention-to-treat estimates, although it may attenuate those estimates.

[12] JUNAEB reports students' gender, day of birth, height, and weight. Using these variables, I calculated Z-scores with EpiInfo software and the CDC/WHO 1978 reference data, using official guidelines in WHO (1995) to code outliers as missing.

Table 4
Summary statistics for first-grade student variables in 2000.

	First-grade students in primary schools with a vulnerability index (V) in 2000		
	All	$0 \leq V < 40$	$48 \leq V < 88$
Years of mothers' schooling	8.56 (3.32) [238,450]	9.85 (2.79) [144,396]	6.29 (3.02) [63,315]
Height-for-age Z-score	−0.03 (1.08) [235,916]	0.12 (1.06) [142,649]	−0.29 (1.07) [62,823]
Weight-for-height Z-score	0.71 (1.39) [230,647]	0.78 (1.40) [141,399]	0.61 (1.36) [59,569]
Female student	0.49 [238,394]	0.50 [144,349]	0.48 [63,313]
First-grade enrollment age	6.38 (0.77) [224,308]	6.27 (0.39) [141,981]	6.64 (1.26) [53,669]
Repeated first grade	0.03 [238,518]	0.02 [144,433]	0.05 [63,336]

Notes: Standard deviations are in parentheses for non-dichotomous variables. Sample sizes are in brackets. See text for variable definitions.

Finally, I extracted the original fourth-grade language and mathematics scale scores.

7. Results

7.1. Program assignment

Table 5 empirically confirms that food calories in a school's meal ration increase sharply at each assignment cutoff, but especially at 68. In column 1, the coefficient on D_{20} indicates a 91 calorie increase, but it is only marginally significant. Controlling for background covariates (column 2) and applying a local linear specification (column 3) slightly reduce its size and, in the latter case, render it statistically insignificant. In contrast, the coefficients on D_{68} are larger (about 170 calories), highly significant, and stable across specifications.

At each cutoff, the coefficients from pooled observations conceal treatment slippage across school years. If one estimates regressions separately by treatment year, the largest coefficient on D_{20} reflects an increase of 103 kcal ($t = 2.33$) in 2001, while the smallest is 30 kcal ($t = 0.58$) in 2005. The slippage is less marked around the 68 cutoff, with magnitudes ranging from 143 kcal in 2005 to 193 kcal in 2001, all highly significant.

Columns 4–6 next examine whether sharp increases in the food calories of meals are accompanied by similarly sharp changes in the proportion of students within each

Table 5
Meal assignment (pooled school observations, 2001–2005).

	Dependent variable:					
	Hundreds of kcal/day in school meal ration			Proportion of students within school allocated meals		
	(1)	(2)	(3)	(4)	(5)	(6)
D_{20}	0.914***	0.796***	0.739	0.050***	0.039	0.031
	(0.467)	(0.438)	(0.459)	(0.029)	(0.028)	(0.031)
N (schools-by-year)	9697	9697	1526	9660	9660	975
Bandwidth	20	20	3	20	20	3
Order of polynomial	4	4	1	4	4	1
Controls	No	Yes	Yes	No	Yes	Yes
D_{68}	1.648*	1.677*	1.667*	−0.020	−0.017	−0.032***
	(0.172)	(0.159)	(0.155)	(0.020)	(0.019)	(0.018)
N (schools-by-year)	21,562	21,562	3437	21,202	21,202	3371
Bandwidth	20	20	3	20	20	3
Order of polynomial	4	4	1	4	4	1
Controls	No	Yes	Yes	No	Yes	Yes

Notes: Robust standard errors are in parentheses, adjusted for school-level clustering. Each cell reports the coefficient on D_x from a separate estimate of Eq. (2), controlling for a piecewise polynomial of V. Additional controls include dummy variables indicating years, regions, private schools, rural schools, schools participating in the Full School Day program in 2000, and schools with an on-site preschool in 2000. Samples are defined by cutoff and bandwidth (see text).

* Indicates statistical significance at 1% level.
** Indicates statistical significance at 5% level.
*** Indicates statistical significance at 10% level.

school that are allocated a meal (the official program guidelines deny this possibility). Fig. 1 did not appear to indicate any appreciable breaks, although the proportion of students covered is much higher around 68 (nearly 90%) than around 20 (about one-fourth). Table 5 confirms that measured differences are generally small, though marginally significant in two specifications.

The rest of the paper focuses on the local experiment at 68, given the larger and more precise estimates of food calorie differences in school rations, as well as a greater proportion of students within each school eligible to receive meals (and, hence, higher expected calories among enrolled students). It constitutes the strongest test of whether higher-calorie meals affect student choices and learning. However, all the estimates subsequently reported were replicated at the lower cutoff, without notable differences in the results.

7.2. Internal validity

Fig. 2 and Table 6 assess whether school-level background variables vary discontinuously at 68. A visual examination of variable means within 1-point bins does not show any compelling evidence of discontinuous variation in either private status or rural location. Table 6 confirms this empirically for several school background variables, with small, statistically insignificant point estimates in global and local specifications.

Fig. 3 reports a histogram of V within the same 1-point bins. There is no clear evidence of bunching of observations to the right of left of either assignment threshold, which would be consistent with ex ante manipulation of V by either schools or JUNAEB. The data are noisy (partly owing to the relative undersmoothing of 1 point bins), but no more so around the cutoff than at other parts of the distribution of V.

Beyond assessing internal validity, Fig. 2 clarifies the subpopulation of Chilean schools to which results can be generalized. Around the cutoff at 68, schools are overwhelmingly rural and public. At 20, the opposite is true: schools are mainly urban and about half are private. There

Table 6
Smoothness of school baseline variables (school observations, 2000).

Dependent variable:	(1)	(2)
Rural school		
D_{68}	−0.013	−0.014
	(0.037)	(0.036)
Private school		
D_{68}	−0.040	−0.046
	(0.049)	(0.050)
On-site preschool in 2000		
D_{68}	−0.016	0.002
	(0.039)	(0.039)
Full School Day in 2000		
D_{68}	−0.015	−0.001
	(0.062)	(0.063)
N (schools)	4469	711
Bandwidth	20	3
Order of polynomial	4	1

Notes: Robust standard errors are in parentheses. Each cell reports the coefficient on D_{68} from a separate estimate of Eq. (3), controlling for a piecewise polynomial of V (the order is specified in final row). Samples are defined by bandwidth (see text).
[*] Indicates statistical significance at 1% level.
[**] Indicates statistical significance at 5% level.
[***] Indicates statistical significance at 10% level.

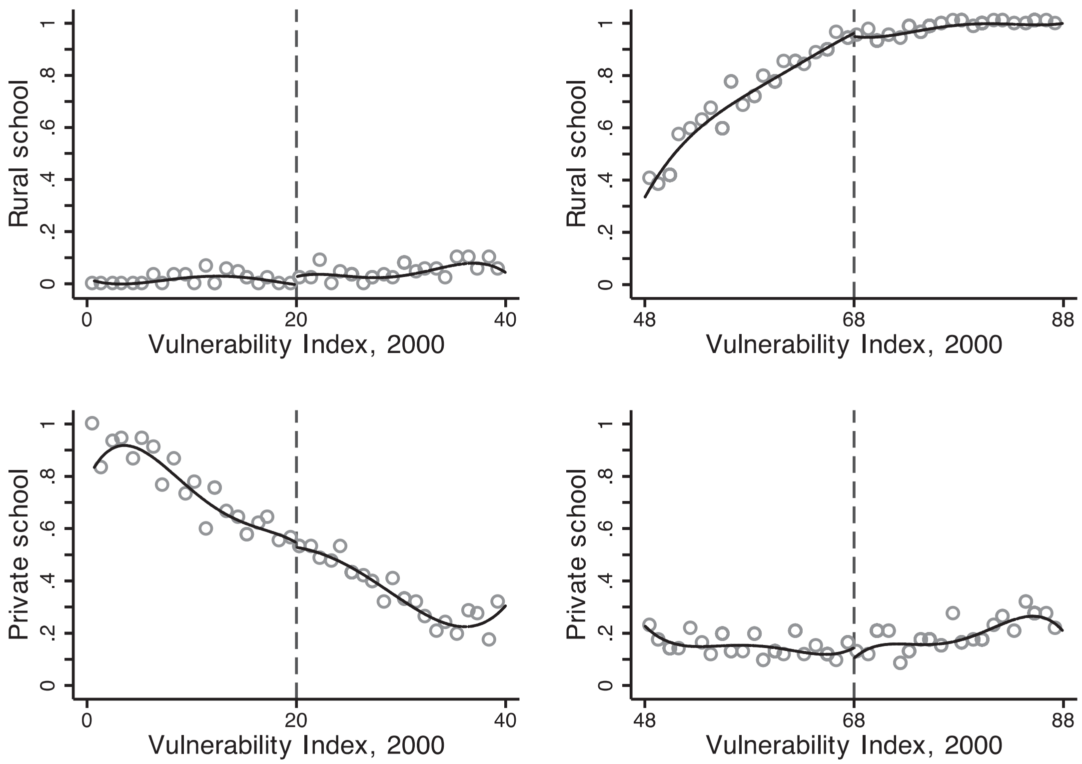

Fig. 2. Baseline school variables in 2000.

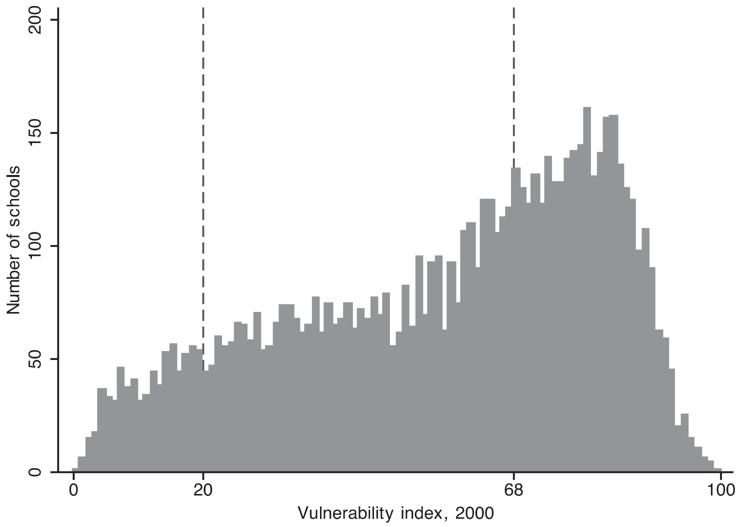

Fig. 3. Histogram of vulnerability index (V) in 2000.

are two implications. First, Chilean schools and students around 68 more closely resemble the target population of school feeding programs in poorer settings of Latin America.

Second, rural schools in Chile operate in less competitive local schooling markets (McEwan et al., 2008), with fewer opportunities for meal rations to induce "poaching" of students from schools with less generous meals (He, 2009). As a simple confirmation of this point, I obtained the latitude and longitude of 424 schools in Chile's sixth region, a mostly agricultural zone located to the south of Santiago (McEwan et al., 2008). Among schools near the assignment cutoff at 68, the closest school of any type is about 3 km away, on average, and the closest private school is about 15 km away, on average. Both distances are larger than comparable means – less than a kilometer – for the predominantly urban schools near the cutoff of 20.

7.3. School enrollment and attendance

Fig. 4 and Table 7 report discontinuity estimates for enrollment in grade 1, total primary enrollments in grades 1–8, and average daily attendance recorded at the school level. Fig. 4 reveals a negative gradient between enrollment measures and V, highlighting the pitfalls of naïve comparisons of school enrollments by meal ration. There is no clear evidence of sharp changes in the same measures around 68. In column 3, the estimated coefficients of −0.7, 4.2, and 5.0 are small relative to sample means of the three dependent variables (12, 98, and 104, respectively).[13] Still, a note of caution is warranted since estimates are relatively imprecise. Again looking at column 3, the right ends of each coefficient's 95% confidence intervals are 1.6, 17.5 and 21.7, respectively, which would constitute increases of between 13% and 21%. Thus, we cannot rule out modest positive (or negative) effects in these specifications. This is counter-balanced by a later robustness check that incorporates baseline data from 2000 (see below). It finds similarly small coefficients for enrollment, but with much smaller standard errors.

Zero or very small effects on enrollment would be not be unexpected. Even in rural areas, primary enrollment is universal, especially in early grades. Further, there is less scope for inter-school sorting and "poaching" because rural schools are dispersed and less likely to be private. The findings on attendance are more surprising, especially since attendance effects are among the most robust findings of experiments in other countries. However, it is plausible that the meal itself provides the largest incentive to attend, rather than simply increasing its calories, as occurs near the cutoff at 68. Even if families are not indifferent to the higher caloric content of some meals, it is plausible that families cannot easily distinguish between the two types of meals rations, 700 kcal and 1000 kcal. It also bears emphasis that Chile's attendance-based finance rules provide financial incentives for administrators to encourage student attendance, independently of meal provision.

7.4. Student sorting on observed variables

Suppose that higher-calorie meals did have modest effects on new school enrollments. It seems unlikely that the mean attributes of newly enrolled first-graders would remain unchanged. Parents of malnourished children may be more likely to take advantage of higher-calorie meals; conversely, more educated parents may confront fewer economic or informational constraints to choosing a school with higher-calorie meals. To assess whether sorting occurs, I successively estimate Eq. (3) with four student variables from the First Grade Survey, collected in the first two months of the school year. Mother's schooling and gender are time-invariant, but two anthropometric measures of nutrition are less so. The height-for-age Z-score reflects malnutrition in early childhood, and is unlikely show evidence of "catch-up" in 1–2 months of PAE treatment in the first grade. Thus, it is a reasonable measure of nutritional status of incoming students. Weight-for-height Z-scores

[13] The available attendance variable includes average daily attendance in all grades offered at a school (not just 1–8). Hence, it is possible for the variable to exceed official enrollment in grades 1–8.

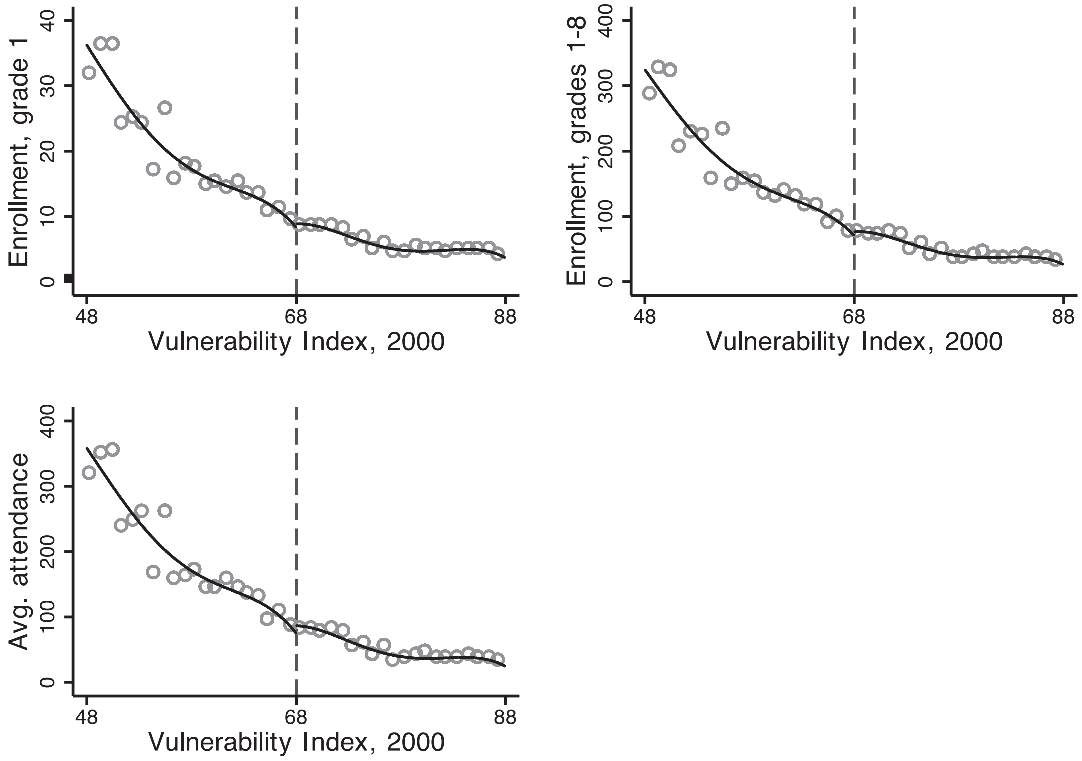

Fig. 4. School enrollment and attendance (pooled school observations in 2001–2005).

may respond more quickly to calories consumed, but it is still unlikely that less than 40 days of moderately increased caloric intake could produce notable gains.

Before proceeding, it is helpful to note that Chile's incidence of malnutrition is among the lowest in Latin America, and has been since at least 1980s (Musgrove, 1993). This is reflected in the nutritional indicators of children even before they are potentially exposed to PAE in primary schools. Using this paper's student sample from 2001, only 3.3% of incoming first-graders are stunted (height-for-age *Z*-score < −2) and 2.4% are wasted (weight-for-height *Z*-score < −2). On the other hand, 16.7% of the sample is obese, given weight-for-height *Z*-scores > 2. Indeed, the public health debate in Chile, as in the U.S., focuses on childhood obesity and its likely determinants (Kain et al., 2005, 2007; Uauy & Kain, 2002).

To visually assess whether there are discontinuities, I pooled first-grade observations between 2001 and 2004.

Table 7
Reduced-form regressions for enrollment and attendance (pooled school observations, 2001–2005).

Dependent variable	(1)	(2)	(3)
Enrollment in grade 1			
D_{68}	0.531	0.221	−0.736
	(1.719)	(1.324)	(1.200)
N (schools-by-year)	21,562	21,562	3437
Enrollment in grades 1–8			
D_{68}	6.080	3.408	−4.193
	(15.701)	(11.838)	(11.066)
N (schools-by-year)	21,562	21,562	3437
Average daily attendance			
D_{68}	11.723	9.584	−4.990
	(19.206)	(14.490)	(13.597)
N (schools-by-year)	12,714	12,714	2024
Bandwidth	20	20	3
Order of polynomial	4	4	1
Controls	No	Yes	Yes

Notes: Robust standard errors are in parentheses, adjusted for school-level clustering. Each cell reports the coefficient on D_{68} from a separate estimate of Eq. (3), controlling for a piecewise polynomial of *V*. School-level controls are in the note to Table 5. Samples are defined by bandwidth (see text). The sample for *average daily attendance* only includes pooled school observations from 2003 to 2005.
* Indicates statistical significance at 1% level.
** Indicates statistical significance at 5% level.
*** Indicates statistical significance at 10% level.

Table 8
Reduced-form regressions for student variables (pooled first-grade student observations, 2001–2004).

Dependent variable	(1)	(2)	(3)
Years of mothers' schooling			
D_{68}	0.028	0.135	0.028
	(0.295)	(0.244)	(0.232)
N (students-by-year)	192,050	192,050	26,646
Height-for-age Z-score			
D_{68}	0.082	0.071	0.052
	(0.062)	(0.059)	(0.057)
N (students-by-year)	190,193	190,193	26,379
Weight-for-height Z-score			
D_{68}	−0.041	−0.004	0.077
	(0.110)	(0.097)	(0.088)
N (students-by-year)	188,002	188,002	26,113
Female			
D_{68}	0.008	0.008	0.003
	(0.016)	(0.016)	(0.015)
N (students-by-year)	192,060	192,060	26,647
First-grade enrollment age			
D_{68}	0.027	0.038	0.039
	(0.024)	(0.024)	(0.022)
N (students-by-year)	191,233	191,233	26,522
Repeated first grade			
D_{68}	−0.008	−0.004	−0.010
	(0.013)	(0.013)	(0.012)
N (students-by-year)	146,762	146,762	20,265
Bandwidth	20	20	3
Order of polynomial	4	4	1
Controls	No	Yes	Yes

Notes: Robust standard errors are in parentheses, adjusted for school-level clustering. Cells report the coefficient on D_{68} from a separate estimate of Eq. (3), controlling for a piecewise polynomial of *V*. School-level controls are in the note to Table 5. Controls for *first-grade enrollment age* and *repeated first grade* regressions further include quadratics in both *Z*-scores, dummy variables indicating years of mother's schooling and female, and dummy variables indicating missing values of these variables. Samples are defined by bandwidth (see text). The sample for *repeated first grade* only includes observations from 2001 to 2003.
* Indicates statistical significance at 1% level.
** Indicates statistical significance at 5% level.
*** Indicates statistical significance at 10% level.

The figures – omitted for brevity – provided no evidence of discontinuous variation, although they confirmed that mean height-for-age *Z*-scores are not far below zero, even in predominantly rural and public schools, and that mean weight-for-height *Z*-scores are substantially above zero, consistent with high rates of childhood obesity cited above. The empirical specifications in Table 8 confirm that coefficient magnitudes are close to zero.

7.5. Enrollment age and first-grade repetition

The final rows of Table 8 assess whether there are discontinuities in first-graders' enrollment age or the probability of repeating grade 1. One might predict that higher-calorie meals are an inducement to enroll children on time. However, the majority of first-graders in Chile already enroll on time, similar to the U.S. or Argentina (McEwan & Shapiro, 2008). Indeed, there is a nearly flat relationship between the enrollment age of first-graders and *V*, with no evidence of a discontinuity (the figure is omitted for brevity).[14] Table 8 confirms that the coefficients are close to zero. The overall first-grade repetition rate is lower in Chile (about 3%) than in most Latin American countries, though it is just over 5% when $V = 68$. Table 8 reports a discontinuity of just −0.008 in the base specification. None of the coefficients are statistically distinguishable from zero.

7.6. Test score outcomes

Despite Chile's success over many decades in raising primary school enrollment and attendance, the performance of Chilean students on international learning assessments has been low and stagnant over time, even compared to lower-income neighbors (McEwan et al., 2008). There is considerable interest, therefore, in evaluating the impact of education programs on student achievement.

Fig. 5 and Table 9 presents results from the analysis of four test score outcomes among the 2002 cohort of first-graders. Of the 2002 cohort of first-graders, 78% took the fourth-grade tests in 2005. There are three reasons why the match rate is not 100%. First, SIMCE coverage is almost

[14] Evidence from other countries usually shows a positive gradient between poverty and enrollment age. As one explanation for this correlation, Glewwe and Jacoby (1995) posit that families of malnourished children – gauged by low height-for-age – may delay enrollment as a rational investment in their child's readiness for primary schooling.

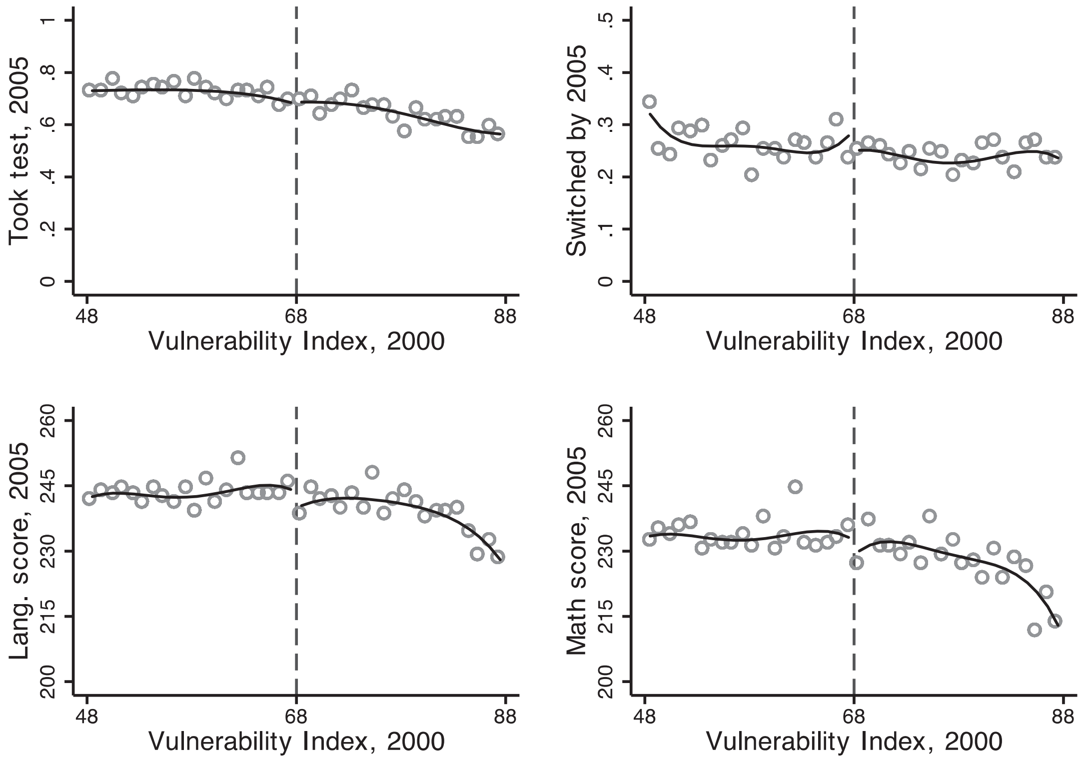

Fig. 5. Students' fourth-grade test score outcomes in 2005.

universal, but still may exclude some rural students in isolated schools. Second, there are matching errors, given misreported national identification numbers. In neither case should we expect that test-taking probabilities vary sharply around the assignment cutoffs. Third, students may not have taken the fourth-grade test on time because they repeated one or more grades.

The upper-left panel of Fig. 5 and the estimates in Table 9 show small and statistically insignificant effects on the probability of taking a fourth-grade test, a

Table 9
Reduced-form regressions for student test score variables (first-grade cohort, 2002).

Dependent variable:	(1)	(2)	(3)
Took a grade 4 test in 2005			
D_{68}	0.007	−0.003	0.030
	(0.037)	(0.036)	(0.035)
N (students)	49,341	49,341	6967
Switched schools by grade 4 in 2005			
D_{68}	−0.042	−0.046	−0.041
	(0.046)	(0.044)	(0.047)
N (students)	34,491	34,491	4812
Grade 4 language score in 2005			
D_{68}	−3.714	−3.734	−5.355
	(4.369)	(4.519)	(3.824)
N (students)	34,237	34,237	4785
Grade 4 math score in 2005			
D_{68}	−3.400	−3.049	−6.777
	(5.190)	(5.339)	(4.698)
N (students)	34,162	34,162	4767
Bandwidth	20	20	3
Order of polynomial	4	4	1
Controls	No	Yes	Yes

Notes: Robust standard errors are in parentheses, adjusted for school-level clustering. Each cell reports the coefficient on D_{68} from a separate estimate of Eq. (3), controlling for a piecewise polynomial of *V*. School-level controls are in the note to Table 5. Student-level controls include quadratics in both *Z*-scores, dummy variables indicating years of mother's schooling and female, and dummy variables indicating missing values of student-level variables. Samples are defined by bandwidth (see text). The means (standard deviations) of each dependent variable are 0.70, 0.26, 242 (50), and 232 (52), respectively.
* Indicates statistical significance at 1% level.
** Indicates statistical significance at 5% level.
*** Indicates statistical significance at 10% level.

Table 10
Robustness and heterogeneity.

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel A: School variables														
Hundreds of kcal/day in ration	1.797*	1.677*	1.744*	1.822*	1.667*	1.651*	−0.152	1.863*	–	–	–	–	–	–
	(0.183)	(0.159)	(0.127)	(0.126)	(0.155)	(0.259)	(0.186)	(0.104)						
Proportion within school allocated meal	−0.007	−0.017	−0.024	−0.006	−0.032***	−0.050***	−0.042***	−0.021**	–	–	–	–	–	–
	(0.021)	(0.019)	(0.016)	(0.015)	(0.018)	(0.030)	(0.022)	(0.010)						
Enrollment in grade 1	0.108	0.221	0.165	0.009	−0.736	1.071	−0.969	0.211	–	–	–	–	–	–
	(1.449)	(1.324)	(1.040)	(0.955)	(1.200)	(2.298)	(1.299)	(0.347)						
Enrollment in grades 1–8	1.590	3.408	3.339	1.856	−4.193	11.507	−5.816	1.142	–	–	–	–	–	–
	(12.93)	(11.84)	(9.33)	(8.70)	(11.07)	(20.41)	(11.36)	(1.97)						
Average daily attendance	7.870	9.584	6.424	5.296	−4.990	13.882	–	–	–	–	–	–	–	–
	(15.86)	(14.49)	(11.41)	(10.71)	(13.60)	(25.13)								
Panel B: Student variables														
Years of mother's schooling	0.152	0.135	0.045	0.047	0.028	0.106	−0.123	0.210	–	–	–	–	–	–
	(0.270)	(0.244)	(0.194)	(0.183)	(0.232)	(0.399)	(0.233)	(0.127)						
Height-for-age *Z*-score	0.053	0.071	0.061	0.094**	0.052	−0.003	−0.096	0.069	–	–	–	–	–	–
	(0.065)	(0.059)	(0.047)	(0.047)	(0.057)	(0.088)	(0.071)	(0.050)						
Weight-for-height *Z*-score	−0.021	−0.004	0.107	0.009	0.077	0.020	0.090	0.075	–	–	–	–	–	–
	(0.105)	(0.097)	(0.072)	(0.068)	(0.088)	(0.125)	(0.116)	(0.070)						
Female	0.011	0.008	0.008	0.008	0.003	−0.024	0.001	0.009	–	–	–	–	–	–
	(0.016)	(0.016)	(0.014)	(0.013)	(0.015)	(0.028)	(0.024)	(0.013)						
First-grade enrollment age	0.059**	0.038	0.025	0.028	0.039	0.085**	−0.073	−0.140	0.056**	0.018	0.072*	0.009	0.048	0.004
	(0.028)	(0.024)	(0.019)	(0.017)	(0.022)	(0.039)	(0.117)	(0.080)	(0.024)	(0.025)	(0.026)	(0.021)	(0.024)	(0.022)
Repeated first grade	−0.021	−0.004	−0.002	−0.010	−0.010	0.023	0.009	0.006	−0.016	−0.003	−0.007	−0.014	−0.014	0.004
	(0.017)	(0.013)	(0.010)	(0.010)	(0.012)	(0.022)	(0.021)	(0.012)	(0.014)	(0.013)	(0.014)	(0.015)	(0.013)	(0.015)
Took 4th grade test in 2005	0.005	−0.003	−0.007	−0.001	0.030	−0.036	–	–	0.016	0.041	0.021	0.046	0.050	−0.029
	(0.042)	(0.036)	(0.028)	(0.028)	(0.035)	(0.063)			(0.041)	(0.041)	(0.042)	(0.042)	(0.037)	(0.054)
Switched schools by 2005	−0.013	−0.046	−0.010	−0.011	−0.041	−0.091	–	–	−0.054	−0.032	−0.061	−0.025	−0.033	−0.077
	(0.051)	(0.044)	(0.036)	(0.037)	(0.047)	(0.071)			(0.052)	(0.056)	(0.050)	(0.061)	(0.047)	(0.079)
Grade 4 language score	−6.351	−3.734	−5.133	−3.276	−5.355	−14.30**	–	–	−5.180	−5.282	−6.767	−3.633	−6.869	0.041
	(5.003)	(4.519)	(3.164)	(3.095)	(3.824)	(7.228)			(4.599)	(5.069)	(4.488)	(5.452)	(4.011)	(6.088)
Grade 4 math score	−8.402	−3.049	−3.188	−2.721	−6.777	−18.948	–	–	−5.075	−8.692	−5.802	−8.582	−8.046	−0.862
	(6.157)	(5.339)	(3.818)	(3.704)	(4.698)	(9.675)			(5.811)	(5.782)	(5.376)	(6.070)	(5.065)	(6.548)
Bandwidth	20	20	20	5	3	1	3	3	3	3	3	3	3	3
Order of polynomial	5	4	3	1	1	1	1	1	1	1	1	1	1	1
Sample/method							2000 sample	"Diff-in-disc"	Male	Female	Low HFA	High HFA	Low m.s.	High m.s.

Notes: Robust standard errors are in parentheses, adjusted for school-level clustering. Each cell reports the coefficient on D_{68} from a separate estimate of Eq. (3), controlling for a piecewise polynomial of *V*. Regressions include school and student controls listed in the notes to Tables 5 and 9.

* Indicates statistical significance at 1% level.
** Indicates statistical significance at 5% level.
*** Indicates statistical significance at 10% level.

grade repetition proxy. I next examine whether, among test-takers, there is a discontinuity in the probability of switching schools between first and fourth grades, perhaps in response to different meal rations. Overall, about 26% of students who took the test switched schools between first- and fourth-graders (although student observations are always matched to their first-grade schools). There is no evidence in the figure or table that switching probabilities are affected by exogenous exposure to higher-calorie meals.

Finally, in the analysis of language and mathematics scores in fourth grade, I assign students to their original first-grade schools. In Fig. 5, the test-score discontinuities are slightly negative. In the final rows of Table 9, the point estimates are consistently negative, even after controlling in columns 2 and 3 for a wide range of baseline variables (see the table note). Given a standard deviation of 50 on both tests, the negative coefficients are not more than 10% of a standard deviation. The right ends of 95% confidence intervals, though positive, are only about 4–5% of a standard deviation, suggesting little scope for noteworthy effects on test scores.

7.7. Robustness and heterogeneity

Table 10 reports the results of several robustness checks, applied to all school and student dependent variables. Each cell reports the coefficient on D_{68} from a separate regression. Each regression uses the largest available sample of pooled school or student observations and includes controls for school and student variables (the samples and controls are described in earlier tables). First, columns 1–3 report regressions that control for 5th, 4th, and 3rd order polynomials of V, respectively, using the largest bandwidth. The results with higher- and lower-order polynomials are not substantially different from the paper's main results. Second, columns 4, 5, and 6 report local linear results in samples defined by bandwidths of 5, 3, and 1, respectively. Estimates from the smallest bandwidth yield larger enrollment effects, but these and other coefficients are substantially less precise. The coefficients on test scores are not positive in any specification.

Third, column 7 reports a falsification test for dependent variables measured in 2000, the pre-treatment baseline year in which V was measured. The first row confirms the anticipated result that there is no jump in the calories of meal rations at 68. Also as expected, there are no "effects" on 2000 values of dependent variables in the vicinity of 68.

Fourth, column 8 pursues an extension of the falsification check, by pooling observations from 2000, when available, with observations from available treatment years (as described in the corresponding table for each dependent variable). In addition to other controls, described in prior tables, the regressions include year fixed effects, the dummy variable D_{68}, and an interaction between D_{68} and a dummy indicating school years after 2000. The interaction term measures the "difference-in-discontinuities" between the pre-assignment year, and all post-assignment years. The results do not overturn the basic conclusions from the prior estimates. However, in the case of both enrollment outcomes, the estimates are considerably more precise.

Fifth, columns 9–14 report estimates for student-level outcomes within subsamples defined by gender, above-median and below-median height-for-age, and above-median and below-median years of mother's schooling. The results counter-intuitively suggest positive effects on enrollment age among boys and relatively shorter children, although the coefficients are still quite small relative to the sample standard deviation. All coefficients on test scores are still slightly negative and statistically insignificant.

8. Conclusions

School feeding programs are popular with parents, educators, and politicians, but there is insufficient evidence on whether they affect education outcomes, especially for mature, government-run programs in developed countries. Belot and James (2011) report evidence that improving the quality of schools meals in a London borough improved test scores and reduced absences. A small number of randomized experiments, primarily conducted in very poor African settings, find some effects on student attendance, fewer on enrollments, and anywhere from zero to small effects on measures of cognitive ability and achievement outcomes.

This paper applies a regression-discontinuity design to evaluate Chile's large-scale plan that awards meal rations with varying caloric content to schools, depending on their measured "vulnerability." The paper focuses on a subset of public and rural schools on the margin of receiving 1000 kcal/day rations instead of 700 kcal. I verify that the food calories of rations do, in fact, rise sharply in the vicinity of the assignment threshold, by about 170 food calories. I then compare school and student outcomes on a wide variety of measures, with global polynomial and local linear specifications. The outcomes include school enrollment and attendance, students' first-grade enrollment age and grade repetition, and fourth-grade test score outcomes including national mathematics and language tests. Overall, the results show no effects of higher-calorie meals on education outcomes.

To explain the absence of effects, especially on learning, it would be useful to know the program's actual impact on nutrients and calories consumed and, eventually, on nutrition and health. Given data constraints, I have no evidence on whether added school meal calories are reallocated from children within households. However, the best recent evidence from other countries suggests that at least half of meal calories and nutrients end up "sticking" to child recipients (Afridi, 2010; Islam & Hoddinott, 2009; Jacoby, 2002).

Presuming that children consume additional calories and nutrients in treated schools, it is still plausible that learning does not increase. Baseline levels of primary enrollment are already high in Chile; most first-graders enroll on time and early grade repetition is relatively low. In this setting, another mechanism for learning increases over early primary grades is through improved nutrition. However, extreme forms of child malnutrition (such as stunting or wasting) have largely been eliminated among Chilean children, perhaps due to an extensive program of early childhood take-home rations or simply declining poverty over the last two decades (Uauy & Kain, 2002). Indeed, Chile now confronts rates of childhood obesity that exceed 15% among incoming first-grade students, much like the U.S. In this context, it is plausible that additional food calories exacerbate these challenges among a population of relatively poorer, but not necessarily malnourished, children. The latter hypothesis cannot be fully explored in this present paper.

While perhaps not surprising, the paper's results on education outcomes should be viewed in the larger context of Chilean development. JUNAEB has provided school feeding programs since the mid-1960s, when Chile's real per capita income was roughly one-third as large as it was during the period of the evaluation. It is possible, and consistent with emerging African results, that earlier versions of the program were more effective in raising education outcomes when Chile had higher poverty rates, higher rates of malnutrition, and lower school enrollments. Thus, the results provide a cautionary tale for countries with rapidly growing incomes. An early focus on caloric content of meals may have outlived its policy usefulness, especially as a means of drawing children into the school setting. To better focus on learning outcomes, and perhaps address the emerging challenges of childhood obesity, school feeding programs could instead focus on improving the nutritional content of meals (Belot & James, 2011; Schanzenbach, 2009).

Acknowledgements

I am grateful to Kristin Butcher, Adrienne Lucas, Maria Perez, Joseph Shapiro, and seminar participants at Brown, the Inter-American Development Bank, and the University of Connecticut for their comments. JUNAEB and the Ministry of Education of Chile generously provided data, and the Spencer Foundation provided financial support. These individuals and organizations are not responsible for the findings or interpretations of this paper.

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