How Effective are Food for Education Programs?

A Critical Assessment of the Evidence from Developing Countries

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Foreword

Food for education (FFE) programs, including meals served in school and take-home rations conditional on school attendance, have recently received renewed attention as a policy instrument for achieving the Millennium Development Goals of universal primary education and the reduction of hunger in developing countries. FFE programs attract children to school by providing nutritious meals in exchange for school participation. If children are undernourished, the programs may also boost learning and cognitive development by improving attention spans and nutrition. The attraction of these programs is their potential to improve both school participation and learning and cognitive outcomes by increasing the consumption of nutritious food by undernourished children. However, FFE programs also have their critics. They are often more expensive than other programs that provide school inputs to increase school participation, and the nutrition benefits are small compared to those from nutrition programs targeting younger children. As a result, governments and donors are in the midst of a debate about the future of FFE programs.

This food policy review presents a rationale for FFE programs and undertakes a critical review of the causal evidence on the impact of FFE programs on education participation and attainment, learning, cognitive development, and nutrition.

Results from the most careful studies show that in-school meals programs improve primary school attendance of enrolled students where initial attendance was low. Potential impacts on school participation by children not previously enrolled in school are not well known. There is mixed evidence that school meals can improve performance on math and literacy tests, and they may improve cognitive development, depending on the type of food provided, the size of the food rations, and program duration. Several well-designed controlled trials have shown that school meals have a positive impact on nutrition outcomes, including anthropometry and iron status, though these results have received less support from field trials in more typical settings. There are few studies of scaled-up take-home ration programs, but one study from Bangladesh shows a significant impact on school participation. In general, FFE programs have larger impacts in areas with low school participation and on children with greater initial malnutrition. The impacts of the programs may also be higher when combined with complementary programs to improve schools or child health.

Despite a large literature on the impact of FFE programs, the authors found that many studies suffer from methodological shortcomings that limit the quality of their contributions. They argue for more carefully designed field trials to bolster the evidence. New research should directly compare alternative FFE programs and other programs with similar objectives to identify the program components that are most effective. Within FFE programs, more information is needed on how impacts on school attainment, learning, and cognitive development could be improved through more effective targeting, changes to the size and composition of food transfers, or provision of complementary schooling and health inputs. Side-by-side comparisons to other popular programs, such as conditional cash transfers and deworming, should also focus on the relative cost-effectiveness of achieving the broad set of education and nutrition objectives.

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Summary

The economic motivations for investing in the education and nutritional status of primary-school-aged children are well established. Moreover, investments in both of these forms of human capital are likely to benefit from substantial complementarities. However, in developing countries, poor and credit-constrained households routinely invest less in education and nutrition than is privately or socially optimal. Food for education (FFE) programs, including meals served in school and take-home rations conditional on school attendance, attempt to improve these investments by subsidizing the cost of school participation through providing food that could improve nutrition and learning. This study examines the economic motivation for the use of FFE programs to increase investments in education and nutrition. The study then presents a critical review of the empirical evidence of the impact of FFE programs on education and nutrition outcomes for primary-school-aged children in developing countries. The main contribution of this study is to judge and summarize the strength of the evidence based on the extent to which existing studies have identified a causal effect of an FFE program, as opposed to finding an association between the program and key outcomes that may have been affected by other contextual factors.

The economic rationale for FFE programs is to offer free food conditional on school attendance to increase the net benefits of schooling enough to change some households' decisions about their children's school participation. Although school-aged children are past the critical window of opportunity during early childhood for the greatest gains from good nutrition, increasing food and nutrient consumption among school-aged children with low baseline food energy or micronutrient intake can improve weight, reduce susceptibility to infection, and increase cognitive function in the short run. Because school meals are usually fortified, a child's micronutrient intake can improve even if her total calorie consumption does not. These

short-run gains may improve a child's educational attainment and academic achievement, which can improve future welfare.

For logistical and political reasons, school meal programs are commonly provided to all children in a targeted school. This practice raises the cost of achieving program objectives, such as increased attendance rates, because it provides transfers to many children who would have attended school anyway. Take-home rations programs are less subject to this criticism, because they are more easily targeted to groups, such as poor or female children, who are in greater need or who may be more likely to change their human capital investment decisions as a result of the program.

Even when provided at school, food transfers can be diverted to other household members by taking food away from the beneficiary child at other meals. This practice could diminish the size of the transfer received by the beneficiary child, resulting in only a small net gain in the child's daily consumption. However, empirical evidence suggests that a substantial share of the food provided through in-school meal programs is not redistributed away from the beneficiary child.

The critical review examines the empirical literature on the impacts of FFE programs on education and nutrition outcomes. The education outcomes considered include school participation measured by enrollment and attendance, age at entry, drop-out status, learning achievement, and cognitive development. The nutrition outcomes reviewed include food energy consumption, anthropometry, and micronutrient status. The review focuses on the empirical literature with the strongest methodology for identifying causal impacts. This literature includes experimental studies, such as randomized controlled trials; experimental field trials; studies using quasi-experimental methods, such as natural or administrative experiments; and nonexperimental studies using careful evaluation designs. Although the literature on the impacts of FFE programs is vast, high-quality studies with evaluation designs that provide causal impact estimates are relatively few. The nutrition literature offers many more experimental studies on nutrition outcomes than is yet available in the economics literature on education outcomes, yet many of the nutrition studies are controlled trials in which many components of the intervention typically affected by behavior, such as amount of food available at a meal, are closely managed. The external validity of these studies for programs implemented in the field is often difficult to ascertain. The number of experimental field studies for any outcome is few, but growing. From the existing literature, it is possible to draw conclusions about the likely impact of FFE programs on some outcomes, whereas for other outcomes, the literature is inconclusive.

The empirical evidence suggests that in-school feeding has a positive impact on school participation in areas where initial indicators of school participation are low. In-school meal programs have been shown to have small impacts on school attendance rates for children already enrolled in school. However, there is no causal evidence for an impact on net primary-school attendance rates for all school-aged children in the service area of a school because of limitations in study design. The only study we found with attendance data for a representative sample of primary-school–aged children, including those enrolled in school at baseline and those not enrolled, found a strong association between participation in a school meal program and school attendance, but estimated impacts cannot be reliably attributed to causal effects of the program. For similar reasons, there is also scant evidence on the effects of school meals on primary-school enrollment rates.

Two empirical studies find that school meal programs cause a significant increase in learning achievement, as measured by improvements in test scores. However, in each study, scores were significantly higher for school meal recipients on only one of three tests taken. The impact of in-school meals on learning appears to operate both through improvements in school attendance and through better learning efficiency while in school, though no study has separately identified the relative contribution of these effects.

FFE programs may also have an impact on cognitive development, though the size and nature of the effect vary greatly by program, micronutrient content of the food, and the measure of cognitive development used. Empirical evidence on the effects of school meals on cognitive function is mixed and depends on the tests used, the content of the meals, and the initial nutritional status of the children. Most of the studies are conducted in a laboratory setting and look at the short-term impact of feeding on cognitive function. The aspects of cognitive ability tested differ by study, making it difficult to compare results. Nonetheless, there is evidence that school meals rich in animal-source foods improved cognitive function in Kenyan children. Another study demonstrates an effect of school breakfasts on cognitive function. Given the controlled setting that formed the basis for these experiments, it would be useful now to expand the external validity of the evidence through field experiments.

On other outcomes, the evidence of the impact of in-school feeding on primary-school drop-out rates is inconclusive. We also found no study that examines the impact of school meals on age at school entry, probably because of the expense of collecting data on a representative sample of children around this age. Also, there is little conclusive evidence on the impact of take-home rations on education outcomes.

For nutrition outcomes, most of the evidence comes from randomized trials in the nutrition literature. For food-energy (calorie) consumption, the evidence shows that in-school feeding programs show greater potential to improve children's total daily energy consumption when children's baseline consumption is well below their age- or weight-recommended consumption level. Differences in empirical strategy may account for differences in findings across studies, as randomized experiments found a lower impact than did quasi-experimental studies.

The diversity of program components and target populations in anthropometric studies, as well as the complexity of biological growth mechanisms, make it difficult to assess the effectiveness of FFE on anthropometric indicators. Overall, several studies showed gains in body size (for example, height, weight, body mass index) or composition (for example, mean upper-arm circumference) due to participation in FFE programs, with weight or body mass index appearing to respond most often. Improvements were typically small, though the effects of increased consumption may have been mitigated by increased activity levels in some cases. The micronutrient content of foods provided may contribute to gains in height (iron fortification) and mean upper-arm circumference (providing meat-based snacks). Deworming appears to have an interactive effect with FFE on height in one study.

Turning to micronutrient status, iron fortification of FFE meals appears to improve iron status in nearly all studies reviewed. Evidence for other micronutrients is more sparse. One study found that meat-based meals improve plasma vitamin B_{12} concentrations but found no impact on other micronutrients. Two studies reviewed the impact of FFE on vitamin A status: one found a positive effect on plasma vitamin A status, whereas the other found no impact. Finally, one study found that iodine fortification reduced the prevalence of iodine deficiencies. The presence of malaria or other infections may impede detection of these benefits, particularly with respect to iron status. Combining the treatment with deworming can improve the effectiveness of iron supplementation, particularly in children with low baseline iron stores.

Summarizing this evidence, FFE programs appear to have considerable impacts on primary-school participation, but the quality of this evidence is weak. Higher quality studies indicate some impacts on learning and cognitive development. There is evidence of effects on food consumption and micronutrient status, provided that initial consumption and nutrient deficiencies are identified and that programs are tailored to address these deficiencies. In many cases, the FFE programs appear to have little impact, because the levels of key outcome variables, such as school attendance or micronutrient status, are already high.

Despite this evidence, significant research gaps remain. A surprising gap in this literature is the lack of convincing evidence of these programs' effect on school enrollment and attendance for a representative sample of school-aged children from the school's service area. There is also no conclusive empirical evidence on the impact of FFE programs on age at entry and grade repetition, and little on drop-out rates. In general, the impacts of take-home ration programs are poorly understood. Also, few studies identify the differential impacts of FFE on children by age or gender. Finally, the impact of FFE programs on learning achievement has not been carefully analyzed by schooling inputs and class size.

Perhaps the greatest omission in current research on FFE programs is the absence of well-designed cost-effectiveness studies. The policy decision on whether to undertake an FFE program or an alternative education or nutrition intervention should be based on relative differences in cost-effectiveness. However, most studies that measure program impact do not collect the additional data needed to obtain a measure of cost-effectiveness. Such studies would identify the cost from various interventions of achieving a certain percentage increase in primary-school attendance, for example. The most convincing approach would be to conduct side-by-side randomized field experiments of alternative programs. To our knowledge, only one study has done so, comparing in-school meals to programs that provide teachers with school supplies or foster parent–teacher communication. However, even these comparisons are complicated by the scarcity of programs likely to have the same kind of combined impacts on both education and nutrition outcomes.

The most immediate policy implication of this review study is that more careful and systematic research is needed to find the most cost-effective combination of programs available. Without rigorous estimates of the impact of FFE programs on school participation, it is not possible to determine whether important secondary effects on learning achievement or cognitive development come primarily through school attendance or through joint effects of schooling and improved nutrition. It is these joint effects that are uniquely available through FFE programs. If the learning and cognitive benefits to school-aged children of simultaneous improvements in nutrition and schooling from FFE programs are small, cash-based programs may be more effective at increasing school participation. If there are no joint education and nutrition effects from FFE programs, it may be more cost-effective to replace these programs with specialized education and nutrition programs that are more narrowly targeted at specific objectives. More comprehensive and rigorous evaluation studies of FFE programs are needed to determine the full scope of the impacts of these programs and their relative cost-effectiveness.

Our interpretation of the empirical evidence reviewed here leads to several recommendations on the design and use of FFE programs. Effects tend to be larger where schooling participation is low or where there are significant nutritional deficiencies. This fact argues for doing an assessment of school needs in target areas before starting an FFE program. Such an evaluation would improve targeting and allow FFE program components, such as the nutrient composition and quantity of food, to be tailored to local needs. Also, program administrators should be willing to consider complementary programs to improve school quality. Learning effects cannot be achieved if the instruction is of little value. Poor school quality lowers the benefits of participation and discourages attendance. Though much more evidence is needed, results from field experiments in the Philippines suggest that the cost of alternative programs to improve school quality may be only a fraction of the per child cost of an FFE program. Coordinated programs that combine FFE with improvements in school quality may be much more effective.

Chapter 1

Introduction and Motivation

Food for education (FFE) programs, including meals served in school and take-home rations conditional on school attendance, are a common tool used to attract children to school and to reduce short-term hunger to help students concentrate and learn. FFE programs operated by the World Food Programme (WFP) reached 21.6 million children in 2005 (WFP 2004), and many governments operate publicly funded FFE programs. For example, Brazil's national school feeding program covers 36 million children aged 0–14 (WFP 2006). These programs are also advocated as important interventions for improving the human capital of school-aged children. The Millennium Development Goal (MDG) Task Force Report (Birdsall, Levine, and Ibrahin 2005) on achieving the Education MDG cites FFE programs as one important approach to attract children to school and improve learning. The *MDG Task Force Report on Hunger* (Sanchez et al. 2005) acknowledges FFE programs as an indirect nutrition intervention that can contribute to the reduction of malnutrition among school-aged children. Although some policymakers and donors push for expanding FFE programs, many question their cost-effectiveness. A recent report by the World Bank (2006) criticizes FFE programs because they are not targeted at the vital first two years of life and they divert resources away from less costly nutrition interventions.

To inform this ongoing debate, this study examines the rationale for FFE programs and conducts a critical review of the empirical evidence of their impacts on education and nutrition outcomes. Several recent studies have addressed the rationale for FFE programs or the empirical evidence of their impacts, including results of a recent experts seminar (WFP 2006), as well as Rogers and Coates (2002), Caldes and Ahmed (2004), and Kristjansson et al. (2007). The research behind this review has benefited greatly from these earlier studies. Although there is an extensive literature on the potential impacts of FFE programs, the strength of the evidence varies greatly, depending on study design and methods of analysis. The main contribution of this study is to provide a critique of the literature on FFE program impacts, judging the strength of the evidence based on the extent to which existing studies have identified a causal effect of an FFE program as opposed to finding an association between the program and key outcomes that may have been affected by other contextual factors.

Often the primary objective of FFE programs is to increase school participation; these programs have been a common tool in developing countries seeking to establish universal primary education. However, the use of food rather than cash as the form of transfer acknowledges that hunger plagues many poor students at school, which may discourage school attendance and also impede learning. Many developing-country governments and international organizations implementing FFE programs have recognized that, by fortifying the food with protein and key micronutrients, they may also be able to improve child nutritional status and reduce morbidity, and so have an additional positive effect on regular school attendance and learning.

Despite these advantages, FFE programs are often criticized as an expensive method for producing the stated education and nutrition objectives. For specific education or nutrition outcomes, other, more cost-effective interventions may exist. Other criticisms include that school meal programs may divert class and teacher time away from learning. In addition, logistical and political considerations often make it difficult to effectively target the program to children who are in greatest need or who are most likely to change their behavior (and begin attending school, for example) as a result of the program. Consequently, many programs choose to supply meals to all students. Although this practice prevents claims of inequity, it raises the cost of achieving program objectives, such as increased attendance rates, because it provides transfers to many children who would have attended school anyway. Also, food transfers, even when provided at school, can be diverted at home by taking food away from the beneficiary child at other meals. This practice may be a rational household decision, but it decreases the potential impact of an FFE program on the target child's outcomes.

Developing a consensus on the desirability and cost-effectiveness of FFE programs in developing countries has been most difficult with regard to the nutrition objectives. The recent World Bank (2006) report on nutrition refocuses attention on the importance of nutrition interventions that reach children during pregnancy (fetal life) and the first 2 years of life. This period has been referred to as the window of opportunity, because it is a period of accelerated physical growth in which nutrition interventions are most needed and have the greatest impact on child survival, health, and development. Nutritional deficits during this period result in largely irreversible damage. In considering the nutritional impacts, the report criticizes FFE programs not only because they arrive too late in the life cycle, but also because they divert resources away from interventions targeting this most critical period of life for nutrition investments.

However, this critique fails to adequately account for the education objectives of school feeding and the potential joint benefits of feeding hungry children during school. Although the lifetime net nutritional benefits of nutrition investments before age 2 outweigh those made later, educating children is also critical to human capital formation and can have substantial returns later in life (Schultz 1988; Glewwe and Kremer 2006). There is substantial empirical evidence that the economic returns to education are high in developing countries (Psacharopoulos 1985, 1994; Duflo 2001). Education also changes behavior in ways that reduce fertility and improve the health and nutritional status of current and future generations (Strauss and Thomas 1995; Schultz 1997, 2002a,b).[1]

An assessment of the desirability of funding FFE programs should consider all potential effects of the programs across education, nutrition, and other objectives. Moreover, policy decisions should ultimately be made on the cost-effectiveness of FFE programs relative to alternative education and nutrition programs. Two shortcomings of the existing literature on FFE program impacts are that many studies focus on a limited set of education or nutrition outcomes and that a heavy emphasis is placed on identifying the benefits or impacts of FFE programs without much consideration of the costs. With this caveat in mind, the review examines the strength of the evidence on the impacts of FFE programs, to better inform the policy debate and identify important gaps to be addressed in future research.

The scope of this review includes the empirical literature in economics and nutrition on the impact of programs that provide food transfers conditional on school participation, such as subsidized school meals and take-home rations, on outcomes in primary school where these programs are most common. In addition, the review emphasizes that part of the empirical literature with the strongest methodology for identifying causal impacts. This literature includes experimental studies, including randomized controlled trials; experimental field trials, which have higher external validity; and studies using quasi-experimental methods, such as natural or administrative experiments that identify impacts by exploiting a quasi-random component of program eligibility. The review does not address several related outcomes, including class size and local agricultural production. We also exclude nutrition supplementation or fortification trials conducted in developing-country primary schools that do not include schooling improvements as outcomes.

[1]See Behrman (1999), Huffman (2001), and Glewwe (2002) for reviews of the impacts of education in developing countries.

This study is organized as follows. Chapter 2 describes the structure and scope of FFE programs. Chapter 3 describes the economic rationale for these programs and explores the mechanisms by which school feeding may affect economic and nutrition outcomes. Chapter 4 describes the methods used to select candidate studies for the review of the empirical literature. Chapter 5 reviews the empirical evidence on impacts of FFE programs on education outcomes. The education outcomes considered include school participation measured by enrollment and attendance, age at entry, drop-out status, learning achievement, and cognitive development. Chapter 6 reviews the empirical evidence for effects on nutrition outcomes, including food-energy consumption, anthropometry, and micronutrient status. We compare the relative impacts of FFE programs to those of alternative programs that provide schooling inputs and discuss the limited evidence on relative cost-effectiveness, where it exists, in Chapter 7. Chapter 8 concludes.

Chapter 2

The Structure and Scope of FFE Programs

FFE Modalities

FFE programs generally take two forms: in-school meals and take-home rations. The major objectives of both modalities are the same: to improve education outcomes and increase food consumption, and possibly nutritional status, of children. However, differences between these two modalities suggest that they may not be equally effective or may affect different aspects of education and nutrition. Among the differences between the programs are the likely timing of food consumption during the school day; who controls and distributes the food; the ability of recipient households to divert the food to other family members; and the quality of food stemming from differences in storage, sanitation, and preparation practices. The composition of the food provided is also often different. Take-home rations are more likely to be single, nonperishable food items, such as cereals or oil. Moreover, in the FFE modality, individual programs can be implemented very differently to achieve specific desired results.

In-School Feeding Programs

In-school feeding programs provide food to children while they are attending school. This food can take the form of breakfast, snack(s), and/or lunch. School meals vary in the quantity of food provided and in their nutritional content, and so their expected impacts also vary. In some cases, the food may be fortified, for example, with vitamin A or iron. School meals are often prepared on site, requiring kitchen facilities, cooking staff, eating and serving utensils, and a space at the school for consuming the meal, making these programs relatively costly to operate. Schools serving meals must set aside time to serve the food, which could disrupt learning, if time for meals would not otherwise be provided. Some programs also offer other health, nutrition, or education programs jointly with in-school feeding. These programs have included deworming, improving school quality and infrastructure, and providing health education. Unlike in the United States, for example, where school meals are targeted to selected students through exclusive breakfast before school or a targeted subsidy of lunch already available for sale, in developing countries, it is often infeasible or undesirable to target individual students for school meals. As a result, all students in program schools receive the food, substantially raising costs.

By providing food at school during the school day, in-school feeding has two advantages over take-home rations. First, it provides an incentive for school attendance directly to the child, rather than through the parents, as with take-home rations. Second, well-timed school meals alleviate short-term hunger, possibly improving students' ability to concentrate and learn (Caldes and Ahmed 2004). Although it is also possible that take-home rations can achieve this goal, this outcome is not explicit in the take-home rations design.

Take-Home Rations

Take-home rations are food rations given to the household conditional on a child's enrollment in school and a minimum level of attendance. Usually the ration is given monthly. A common requirement, though often weakly enforced, is that children attend at least 80–85 percent of school days to maintain eligibility for the program. Because the transfer is directed to the household and not the child, the welfare gains may be more dispersed.[1] The household can redirect the food ration to whomever it desires or sell it for other goods or cash. In this sense, the ration is comparable to an income transfer. Take-home ration programs place less emphasis on alleviating short-term hunger for children at school, focusing instead on improving food security at the household level (Pollitt 1995). It is often much less costly than in-school feeding and does not take time away from learning. In practice, take-home ration programs are often cheaper to operate, because they are more easily targeted, for example, toward poor households. Although it is often infeasible in developing countries to restrict in-school meals to specific children, either for logistical or political reasons, take-home rations are routinely provided to a select set of children. For example, the WFP sometimes targets take-home rations exclusively to girls, who often lag behind boys in school attendance. In some cases, these take-home rations are provided as a top-up transfer to girls: an additional incentive in areas where all primary-school children receive in-school meals (WFP 2005).

[1]As described below, this effect depends on whether the transfer is inframarginal with respect to the child's food consumption.

The Scope of Today's FFE Programs

It is difficult to know the full scope of FFE programs in developing countries, but a summary of the programs currently operated by the WFP, likely the world's largest provider of in-school meals and take-home rations outside of a single country, gives a good indication of the typology and popularity of FFE programs. According to Gelli (2006), WFP's FFE programs reached approximately 21.6 million children in 72 countries in 2005. In addition to in-school meals and take-home rations, WFP sometimes provides fortified biscuits for distribution at school. Nearly half of WFP FFE programs combine these modalities for linking food to school participation. In 24 percent of FFE schools, only fortified biscuits are provided, whereas 22 percent of program schools use only on-site meals and 6 percent use take-home rations exclusively. On average, in-school meals provide 876 kcal of food energy per child per day; biscuits provide 313 kcal of energy.[2]

The average cost of running FFE programs at WFP in 2005 was US$15.79 per child per year. The cost of on-site meals alone was slightly higher, whereas that of biscuits averaged US$9 per child. For take-home rations, the annual average cost was much higher (US$30) because of transport costs and differences in food bundles. In addition to the food, WFP also supports complementary activities to improve child health. For example, deworming is provided in 56 percent of WFP-assisted schools; micronutrient supplements are provided in 40 percent of schools; hand-washing facilities are provided in 51 percent of FFE beneficiary schools. In some cases, WFP partners with nongovernmental organizations (NGOs), the country government, or other United Nations agencies to provide complementary school facilities and services.

Many countries operate their own FFE programs without support from WFP. One such program is the national school-feeding program in Brazil, a universal school meal program covering public and religious schools operating since 1955 (WFP 2006). In 2006, the program reached 36 million children aged 0–14. Many other countries operate similar programs, but it is beyond the scope of this study to summarize their incidence and operation.

[2]Estimates of average food-energy intake from take-home rations were not available.

Chapter 3

Economic Rationale for FFE Programs

Conceptual Framework

Figure 3.1 shows the pathways through which FFE programs may affect participants' education and nutrition outcomes. FFE programs increase the benefits of school participation, which leads to increases in enrollment and attendance. These increases may improve learning and educational achievement, which may be bolstered by improved nutrition and associated gains in cognitive function.

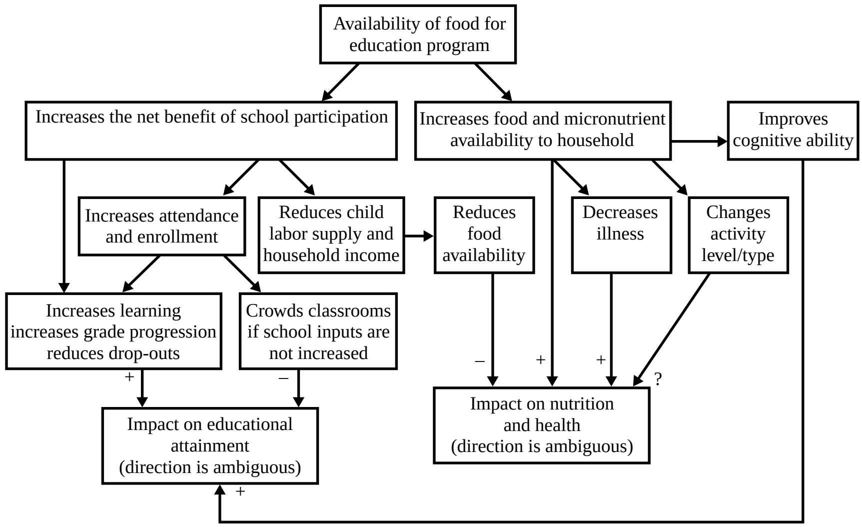
FFE programs improve nutrition and health by directly increasing household food availability, but the net effect on current nutritional status could be negative because of income loss resulting from increased school participation. If an FFE program is not accompanied by increased school capacity, classrooms may be crowded, leading to negative effects on learning. Thus, well-run programs that provide reasonably nutritious meals should have positive impacts on school participation, learning, and child dietary intake. However, the size of these effects depends on various programmatic and contextual factors. In some cases, they may be small or even negative.

The next two sections discuss these mechanisms in detail for both in-school feeding and take-home ration programs. The first section looks at the impacts of both programs on education outcomes, and the next one looks at the theoretical impacts on nutrition.

Theoretical Impacts on Education

The primary rationale for FFE programs is to promote households' investments in the human capital of their children, particularly investments in education but also

Figure 3.1 Potential benefits of food for education programs



in nutrition. Levels of education attainment remain extremely low in many developing countries, despite substantial evidence that both the private and social returns to education are high (Hanushek 1986; Schultz 1988). To understand how FFE programs redress this underinvestment, it is necessary to consider how households decide on schooling investments.

Parents decide how much to invest in the education of each of their children by comparing the potential future benefits of this education to current costs.[1] The current value of these benefits is affected by the household's discount rate—how much it values improvements in its current well-being over future improvements in well-being. The costs of education include both direct and indirect costs. Direct costs can include those of school fees, supplies, books, uniforms, and travel to school. Indirect costs include the opportunity cost of a child's time. These opportunity costs include the value to the household of the activities the child foregoes by attending school, such as caring for other family members, working on a family farm or business, or working outside the household to provide additional income. Households will not send their children to school if the costs of schooling exceed the expected benefits, or if the household is unable to finance the schooling investment, either directly or through credit.

[1]The future returns to education can include higher earnings in adulthood, improved marriage outcomes, and better health for current and future generations.

FFE programs lead to greater investment in education primarily by subsidizing these schooling costs. Moreover, the food provided in the program can also help the child learn more effectively, thereby increasing the returns to education in the future. Below we consider the mechanisms by which FFE programs impact various schooling outcomes, including school participation, age at entry, grade repetition, drop-out rates, classroom behavior, learning achievement, and cognitive development. We focus first on in-school meals and then consider how these impacts differ for take-home rations.

In-School Meals

The availability of subsidized in-school meals will increase school enrollment rates if the program changes the household's schooling decision for some children who would not have been enrolled in school otherwise. Changing the school enrollment decision to a positive one for these households requires that the net benefits of participating in the program exceed the gap between direct and opportunity costs of schooling and the expected benefits of schooling. The magnitude of the increase in enrollment rates depends in part on the size of the transfers relative to the size of this cost–benefit gap for these households. Because many children would have enrolled in school without the program or with a less-generous program, the inability to target in-school meals only to those children whose enrollment decision would be changed by the program raises the cost of the program per additional student enrolled.

In-school meals can also be effective at increasing school attendance rates because children receive the meal only on days when they attend. Because the opportunity cost of a child attending school can vary across school days—according to seasonal demand for agricultural labor, for example—the effectiveness of in-school meals at changing school attendance rates depends on the value of the meal relative to the difference between the cost and expected benefit of school attendance on a given day.

Furthermore, school attendance may be affected through improved nutritional status. Three aspects of nutrition may influence school attendance through in-school feeding. The first is the short-term impact of in-school feeding. In-school feeding alleviates a child's short-term hunger during the school day, by providing more nutrients to the child, providing the child with a meal when he or she would have not otherwise have had one, or replacing a meal that would have been received after school with one during school hours. A child who is not hungry during school hours is able to concentrate better and learn more (Grantham-McGregor, Chang, and Walker 1998). Such a child may benefit more in terms of learning from a day of school than would a hungry child, which may impact households' schooling choices. Additionally, the child may prefer to attend school when he or she is not hungry. The second nutrition-related influence of school feeding on school attendance is through the longer run benefits of program-induced improvements in nutrition. Sustained nutrition improvements through school feeding could improve a child's physiological capacity for learning, which has a direct effect on the benefits of schooling and an indirect effect by increasing the child's desire to attend school. Finally, in-school meals may improve attendance through nutrition by reducing morbidity. In many developing countries, morbidity is a leading cause of missed school days. Improved nutrition, especially adequate intake of micronutrients, can strengthen the immune system and reduce the incidence and severity of infectious diseases among children (Scrimshaw and SanGiovanni 1997). Therefore, if in-school meals improve children's nutritional status, they may reduce morbidity and decrease the number of school days missed from illness, thus increasing attendance.

Children may begin primary school at a different age than the country's suggested starting age for many reasons. These include lack of funds, lack of childcare, and a perception of limited benefits of attending school at the recommended age. Delayed schooling is very common in developing countries, with many children starting school much later than the recommended age (regarding Sub-Saharan Africa, see UNESCO Institute for Statistics 2001). In-school meals are most likely to affect age at entry through an income effect. That is, the food provided effectively increases household income and directly raises the benefits of attending school. This income effect will be large enough to cause some households to start their child in school at a younger age. Thus, average age at entry should decrease. Age at entry may also be influenced by neighborhood effects that result from the school feeding program. If, in a given neighborhood, households tend to send their children to school earlier with the introduction of school feeding, then there may be social pressure on those who have not yet enrolled their children to do the same.

The effect of in-school feeding on grade repetition, drop-out rates, learning achievement, and school performance are all interrelated. This effect works through two mechanisms: attendance and nutrition. The attendance channel can be described as follows. In-school feeding improves children's attendance, so they spend more hours learning in school. This attendance impact should allow them to learn more and, as a result, improve their school performance, decreasing their likelihood of repeating a grade or dropping out. This mechanism is dependent on the level of school quality, including teacher/student ratios; the availability of schooling inputs, such as textbooks and pencils; and teacher quality. If school meals increase enrollment rates and attendance, as expected, classrooms may become overcrowded, and teaching quality may decrease. Similarly, if school feeding represents a significant burden on the teachers' time, learning time may be reduced. Thus, unless additional financial and human resources are available, school feeding programs have the potential to worsen school performance and increase drop-out rates and grade repetition.

As with school participation, the effect of in-school feeding on other measures of school retention and performance may be enhanced through improved nutrition. This mechanism operates through two channels. The short-term impacts of providing children with a meal during the school day may alleviate hunger and help them to concentrate and learn better, thereby improving school performance and retention. The longer run effects are conditional on in-school meals improving the nutritional status of children and on nutritional status affecting the ability to learn. Furthermore, the impact of in-school feeding on education will vary, depending on the initial nutritional status of the child.

Pollitt (1995) discusses two biological mechanisms through which breakfast can affect cognition. By extension, these mechanisms are present in an in-school meal setting, be it breakfast, snack, or lunch. The first is the short-term metabolic and neurohormonal changes that are associated with the immediate supply of energy and nutrients to the brain. Brain function is sensitive to these changes. If an overnight fast is extended because a child does not eat breakfast, insulin and glucose levels gradually decline, resulting in a stress response that interferes with different aspects of cognitive function. If this stress occurs frequently, it is likely to have a cumulative effect. This sequence is the second biological mechanism discussed by Pollitt, which pertains to the long-term impacts of the sustained contributions of breakfast to a person's health status, which in turn affects cognitive development. To the extent that the in-school meal is, at least in part, an addition to the child's usual nutritional intake, then this second mechanism should also occur with other in-school meals. It should then improve the nutritional status of a child in the long run. In addition, when the school meal is nutrient fortified, it may prevent or reduce nutritional deficiencies that affect cognition, such as iron deficiency.

Take-Home Rations

Many of the mechanisms through which in-school meals can affect education outcomes also exist for take-home rations. This commonality is particularly true for impacts that derive primarily from the income effects of the transfer. However, differences in education effects between the two modalities arise for three reasons: (1) differences in how households redistribute food among their members under the two modalities, (2) constraints on the timing of meals with take-home rations, and (3) differences in the type of food provided. To consider the first two effects, assume that both programs would provide the same quantity and composition of food to the household over the course of a month.

The first effect represents a dilution of food transfers to the targeted child. With take-home rations, the entire household is targeted by the food transfers, as opposed to just the school-going child. When the rations are received at home rather than at school, it easier for the household to redistribute the food to other household members. In theory, if the quantity of food provided to a child through school meals is *inframarginal,* meaning that it is less than the child would consume that day in the absence of the program, the household could redistribute food at other meals to other household members and achieve the same daily allocation of food to all members as it would with take-home rations. If the school meal is *extramarginal*—if it is more than the child would consume otherwise—it is not even theoretically possible for the household to achieve the same intrahousehold allocation of food as with take-home rations. In practice, even inframarginal food transfers through in-school meals may be difficult for a household to redistribute through substitution at other meals, if social norms make it unacceptable to sharply reduce a child's consumption at other meals. This discussion assumes that the timing of food consumption during the day is inconsequential and that only total daily intake matters. If timing of consumption matters, the second effect becomes operative.

The second effect arises because of differences in the likely timing of food consumption under the two modalities. In-school meals benefit from timely provision of food to students during school hours, which can increase concentration and the ability to learn. These effects can only be replicated under a take-home rations program if children are able to carry a meal of equivalent quantity and quality with them to school or are able to consume a meal at home at the same time of day. The relative effectiveness of the two modalities depends on the optimal time of day to provide food to maximize the learning benefits. Also, it is harder to approximate the timing of some meals through take-home rations than others, particularly if these are "wet" rations, or those that must be prepared at the time they are consumed. If the school meal is provided at breakfast, the benefits of this meal are fairly easily replicated at home with breakfast before school under the take-home rations program. This parity holds unless the child has to travel a great distance to school, and so must eat breakfast at home using take-home rations well before she would receive the food at a school breakfast. If a school meal of wet rations is provided as a mid-morning snack or school lunch, achieving the same effects through take-home rations would require that the child go home for the meal, disrupting the school day. Alternatively, if the learning benefits of consuming breakfast outweigh those of lunch, and the school meal is suboptimally timed for later in the day, the meal could be better targeted at breakfast through take-home rations.

The third effect arises if the composition of the take-home ration differs from that of the in-school meals. In-school meals often include milk products or other nutrient-dense foods, whereas take-home rations primarily include cereals and oils, which may or may not be fortified. If foods provided through the program are more nutritious than foods eaten at home, then the impact of the program on the quality of the child's diet is dependent on the share of the child's daily consumption that comes from the program food. In this case, in-school meals would likely provide a better quality diet for the child than take-home rations. This difference in diet quality may lead to better educational performance. However, these differences in impacts arise from the application of take-home rations and in-school meals in practice and do not derive directly from differences in the two modalities of food delivery.

Theoretical Impacts on Nutrition

FFE programs have come under scrutiny recently for targeting children who are too old to reap lasting morbidity and anthropometric benefits of improved calorie and micronutrient consumption.[2] However, FFE programs can reduce short-term hunger and micronutrient deficiencies, which can increase cognitive function and resistance to intestinal and respiratory infections (Jamison et al. 1993, 2006).[3] FFE programs may also provide an important nutritional intervention during an often overlooked critical growth period. With delayed starts to schooling and repeated grades, many children in primary schools in developing countries have already reached adolescence. As adolescents can gain as much as 15 percent of adult height and 50 percent of adult weight, their energy requirements are very high during this period. Adolescent girls, in particular, have high nutrient and micronutrient demands. Although malnourished adolescent girls do catch up to well-nourished girls during puberty, their growth is delayed. This delay can mean that a malnourished girl is not finished growing at the time of her first pregnancy (Gillespie and Flores 2000), which can increase the risk of complications and of maternal and infant death.

FFE as an Intrahousehold Resource Allocation Issue

While providing a direct transfer to nutritionally vulnerable children would seem like a reasonable approach to offset intrahousehold nutritional inequalities, traditional intrahousehold resource allocation models (Samuelson 1956; Becker 1973) predict that households will treat these targeted transfers in the same way that they treat income transfers to the household as a whole. In these highly criticized models, household members pool their income, including transfers to children, and make consumption decisions according to a single household preference structure. As such, the models predict that regardless of which household member receives a transfer, household consumption will be affected in the same way. With respect to

[2]A counterexample to this claim is provided by a study from the Philippines (Adair 1999) that shows that children aged 2–12 who were previously stunted can experience catch-up growth.

[3]A considerable body of literature suggests that both educational attainment and cognitive ability improve adult productivity and earnings. For a review of literature linking educational attainment and wages, see Psacharopoulos (1994) and Psacharopoulos and Patrinos (2004); see, for example, Cawley, Heckman, and Vytlacil (2001) for evidence of cognitive ability's effect on wages.

school feeding, these models predict that when children receive food at school, they receive less food at home. Assuming that the school-feeding transfer is inframarginal (less than the child's usual consumption), the transfer is equivalent to a change in household income equal to the value of the transfer and is distributed accordingly. These models suggest the same outcome regardless of whether FFE programs are administered as in-school feeding programs or take-home rations.

More recent household models, which fall under the umbrella of "collective models" (see Chiappori 1988, 1992; Browning et al. 1994; Browning and Chiappori 1998), suggest that household income is treated differently depending on which household member receives the income. As such, these models predict that targeted transfers, including school-based feeding programs, can be more effective at improving specific household members' outcomes than transfers given to households as a whole. Household bargaining models (Manser and Brown 1980; McElroy and Horney 1981) are a form of collective model in which specific assumptions on the origins of the sharing rule are made.

Several recent empirical studies, although not providing direct support for these collective models, have shown that intrahousehold allocation, hence the transfer beneficiary, depends on which member brings income into the house and whether the income is conditional or unconditional (Thomas 1990; Duflo 2003; Quisumbing 2003; Quisumbing and Maluccio 2003; among others). In particular, these studies show that child anthropometric outcomes improve when mothers control more household income. For example, Duflo showed that girls living with a grandmother who received a government pension gained more weight than did girls living with grandfathers who received this pension. Quisumbing (2003) found that intrahousehold beneficiaries also change, depending on whether the transfer is conditional or unconditional.

As children are not typically involved in household resource-allocation decisions, it is unclear, theoretically, how providing children with a transfer will affect how the transfer is used. However, as care of children and allocation of food is typically under women's control, school feeding transfers are likely similar to providing a transfer to the mother, and are therefore more likely to be targeted toward improving children's health and nutrition (Rogers and Coates 2002). Additionally, Kooreman (2000) argued that child transfers generate a "labeling" effect in which child transfers actually change parental preferences in favor of child goods, increasing the percentage of the transfer benefiting the targeted child. Thus, school feeding may be an effective method of improving school-aged children's consumption.

Differences in Outcomes Based on FFE Modality

The resource allocation models described above predict that FFE programs that provide inframarginal transfers will have the same impact on consumption whether they are provided in school or as take-home rations. However, these models ignore the potential differences in effects of school meals and take-home rations on education outcomes mentioned earlier in the chapter. That is, outcomes may differ between modalities depending on the importance of the timing of food consumption and the potential for differences in the composition of food transfers in practice between the two types of programs. Overall, the timeliness of food consumption may be less important for nutritional status than for learning performance. Differences in micronutrient content of foods between the two modalities are likely to have more direct effects on nutritional status than on school participation, but they may have important effects on cognitive development.

Chapter 4

Method for Reviewing the Empirical Literature

Criteria for Selecting Studies for Review

The objective of the literature review is to summarize and critique the evidence from studies using rigorous methods to identify the impact and cost-effectiveness of programs for subsidized school meals or take-home rations in developing countries on outcomes in primary school, where these programs are most common. To identify candidate studies for the review, we used searchable electronic bibliographical databases with broad coverage of mostly peer-reviewed research in economics and nutrition, including ECONLIT and MEDLINE.[1] We also consulted with several colleagues who are familiar with this literature to identify relevant unpublished or very recent studies.

To be selected for the review, a study must: (1) address the impacts or cost-effectiveness of free or subsidized FFE programs (in-school meals or take-home rations conditional on school attendance), (2) use as primary subjects primary-school-aged children in developing countries, (3) focus on selected education outcomes (school participation measured by enrollment and attendance, age at entry, drop-out status, learning achievement, and cognitive development) and/or nutrition outcomes (food-energy consumption, anthropometry, and micronutrient intake or status), and (4) utilize statistical techniques to attempt to identify causal impacts of the programs (including, but not limited to, experimental designs, such as randomized controlled trials or field trials, and quasi-experimental designs using natural, administrative, or policy experiments that determine program access). Also,

[1]Keywords used include "school feeding" or "food for education" and "Asia or Africa," in combination with one of the outcome variables, such as "school attendance" or "anthropometry."

most, but not all, of the studies cited had already been subjected to peer review, for publication in a journal or elsewhere.

To keep the review focused and manageable, we excluded studies that estimated impacts of FFE programs on several related outcomes, including class size and improvements in local agricultural markets or agricultural production. We also avoided consideration of a number of complementary programs, including nutrition education or school gardens. The review also excludes studies of nutrition supplementation trials in which schools were used as a venue for the experiment, but the primary goal of the intervention did not include improvements in school participation or performance.

Assessing the Strength of the Evidence: Methods for Identifying Causal Impacts

The studies reviewed here seek to address the question, "What is the impact of this FFE program on education or nutrition outcomes?" In answering this question, researchers hope to measure the difference in the outcomes that can be attributed to the presence of the FFE program, or the causal impact. This measurement requires comparing outcomes for beneficiaries of a program to the counterfactual—what those outcomes would have been had these beneficiaries not participated in the program. All evaluation strategies are designed to find a method for constructing a proxy for these counterfactual outcomes from information on nonbeneficiaries. In this section, we introduce the intuition behind the need for these evaluation strategies. For a more detailed explanation, see the Appendix.

How should the comparison group for constructing counterfactual outcomes be formed? Average outcomes of all nonbeneficiaries may not make a very good counterfactual, because program beneficiaries may be systematically different from nonbeneficiaries even before the program. For example, most FFE programs target poor communities or areas where school enrollment is low. Also, the factors affecting household decisions to send children to school can vary. It may be that those who send their children to school in response to the FFE program have lower food consumption at home or have less need for the children to work on the farm. These distinguishing characteristics cause selection effects, leading some households and not others to participate in the FFE program. The most common form of selection effects arises from program targeting or household characteristics that affect self-selection from among eligible households. If these characteristics are not accounted for when constructing a comparison group, they lead to a form of bias in the impact estimates known as selection bias.

The most convincing way to form a comparison group for an impact evaluation is to randomly assign the program to a subset of communities among a group of similarly eligible communities. Households in communities not randomly selected for the program form an experimental comparison group or control group. Randomization of the program is an effective way to form a comparison group because, in a program provided by chance, we expect that beneficiaries and nonbenficiaries would have had similar outcomes in the absence of the program. Impact estimates from a randomized evaluation will have low bias, because outcomes cannot be correlated with access to the program. This approach can be justified in a program that is phased in over time because of budgetary limitations. Communities randomly assigned to the control group at first can be brought into the program in later years, when more funding is available.

Often, random assignment of the program for the purpose of an evaluation is not possible for logistical, ethical, or political reasons. In these cases, it may still be possible to conduct a rigorous evaluation by constructing a statistical comparison group from among nonbeneficiary households. There are many of these nonexperimental approaches to impact evaluation, including matching methods, regression discontinuity, or instrumental variables. The appropriate choice of technique depends on the design of the program, the method of targeting used, and the data available. Matching evaluation methods involve finding a subsample of nonbeneficiaries who are statistically similar to beneficiaries in terms of a large set of observable preprogram characteristics. Regression discontinuity constructs a comparison group of nonbeneficiaries who are most similar to beneficiaries near the threshold of program targeting criteria for a carefully and systematically targeted program. Instrumental variables techniques require proxy variables for access to the program that are uncorrelated with unobserved characteristics affecting program participation and the outcome variables. In most cases, an evaluation is stronger if outcomes can be measured before and after the program begins, so that impact estimates can be constructed as the difference in the average change in household or child outcomes between the treatment and comparison groups, or the difference-in-differences (DID) in outcomes. DID estimates remove bias caused by unobserved fixed characteristics that are systematically different between the treatment and comparison groups.

Our critical review of the literature presented in the following chapters considers the strength of the evaluation design and the methods used to reduce selection bias in the impact estimates. We emphasize the evidence from studies in the literature with the strongest evaluation designs.

Chapter 5

Empirical Evidence of the Impacts on Education

School Participation (Enrollment and Attendance)

In evaluations of FFE programs, school participation is the most common education outcome empirically investigated by economists. Improving school participation is usually the primary objective, and in-school meals programs are designed so that beneficiaries must attend school to receive the transfer. Take-home rations conditional on school attendance also link beneficiary status with school participation, though the impact of take-home rations on school attendance has received less scrutiny. Most of this literature supports the conclusion that school feeding has a positive and significant impact on school attendance and enrollment, though the strength of this result is qualified by methodological shortcomings. One significant shortcoming is that most studies only investigate the effect of school feeding on attendance for students who are already enrolled in school. Measuring attendance effects conditional on enrollment could vastly understate the full effect of these programs on school participation in countries where nearly universal enrollment has not yet been achieved. In randomized controlled trials reported in Jacoby, Cueto, and Pollitt (1996) and Powell et al. (1998), for example, school-based samples were used rather than random samples of school-aged children in the service area of the school. To our knowledge, the only studies that have looked at the impact of the introduction of school feeding on the enrollment and attendance of all children in the service area of a school are those by Ahmed (2004) and Vermeersch and Kremer (2004). These studies find impacts of FFE programs on school participation in their samples, but each study has some limitations. We begin by reviewing the controlled trials before turning to the larger field studies by Ahmed and Vermeersch and Kremer.

Powell et al. (1998) studied 814 children in second- through fifth-grade classrooms in rural primary schools in Jamaica. Children were randomly assigned to receive a breakfast (576–703 kcal and 27 g of protein) or a placebo (orange slice with 18 kcal) each day for one 8-month school year. This randomization occurred at the individual level, not the classroom level, so both treatment and control children were in a single classroom. This approach improves statistical power over randomizing at the school or classroom level by providing a larger number of treatment units. Attendance data taken from school registers showed a small improvement in attendance rates for children receiving breakfast over the control group. This effect was larger for undernourished children (a 3.1 percentage point difference in attendance rates) than for adequately nourished children (a 1.9 percentage point difference). Nonetheless, these impacts are small relative to the scope for potential impact, given that attendance rates in both groups were about 70 percent.

Jacoby, Cueto, and Pollitt (1996) found that a school breakfast program in Huaraz, Peru, improved attendance rates of fourth- and fifth-grade students. Ten schools were randomly assigned either to the treatment group, who participated in the program, or to the control group. The breakfast program started in April 1993, and the evaluation took place 30 days later. After those 30 days, the breakfast program was also implemented in the control schools. Therefore, there was little time for the program to have an effect. Jacoby, Cueto, and Pollitt (1996) found that there were no significant differences in attendance rates between the treatment and the control groups before the implementation of the program. During the program, attendance increased by 0.58 percentage points in the treatment schools and declined by 2.92 percentage points in the control schools. The difference in the change in attendance rates between the treatment and control schools was statistically significant.[1]

Ahmed (2004) evaluated the impact of a school feeding program implemented by the Government of Bangladesh and the WFP in food-insecure areas of Bangladesh. The evaluation took place in 2003, after most children in program schools had been receiving school feeding every school day they attended for more than 1 year. The school feeding program provided a mid-morning snack of fortified wheat biscuits to children in primary schools in these communities at the cost of US$18 per child per year.

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[1]Jacoby, Cueto, and Pollitt (1996) also show that attendance rates increased in both the treatment and initial control schools over the next 30 days, during which both groups received the program, but a statistically significant difference between the two groups remained. This second result is not very informative, however, because initial differences in attendance rates between the two groups were not controlled for. A more rigorous approach would have used a DID estimator, which measures impact as the difference in the change in attendance between the treatment and control groups.

With school feeding ongoing, measuring program impacts using an experimental evaluation design, such as a randomized field trial, was not possible. As an alternative, Ahmed estimated impacts by comparing outcomes in treatment communities running the program to outcomes in a set of comparison communities without the program that had similar observable characteristics. This empirical strategy should have reduced bias in the impact estimates by controlling for differences in observable community characteristics and for differences in *average* observable child and household characteristics at the community level. However, some bias may have remained if there were systematic differences in child outcomes between treatment and comparison groups that were correlated with the probability that the child received the program.[2]

Ahmed (2004) first compared raw outcomes in the treatment communities with those in comparison communities, with no controls for differences in child, household, or community characteristics. He found that 6 percent of all households with primary-school-aged children in rural program villages did not send their children to school, compared to 15 percent in control villages. He also noted that almost 50 percent of the primary-school-aged children in control communities in urban slums were not attending school compared to 41 percent in treatment communities. Ahmed also reported that school attendance from school registers increased in both program and control schools during this period, but that the increase was 1.1 percentage points higher in program schools.

To determine whether the observed gains in enrollment and attendance were in fact due to the introduction of school feeding, Ahmed (2004) performed an econometric analysis to isolate the effects of the program from other factors. To calculate the impact on enrollment, he regressed a dummy variable of whether a school-aged child in the sample was enrolled in primary school on a dummy variable that took on the value of 1 if the child lived in a program community and 0 otherwise. He also included child and household characteristics, as well as location fixed effects in the regression. The coefficient on the program dummy variable provided an estimate of the impact of the program on enrollment. Ahmed found that the school feeding program increased enrollment by 14.2 percent, and that this increase was statistically significant. Ahmed performed a similar regression of the number of days an enrolled child attended school in August 2003 on the same explanatory variables

[2]Child- and household-level matching methods may have further reduced bias in these impact estimates by constructing a statistical comparison group of children that more accurately represent the outcomes that children in the treatment group would have experienced in the absence of the program. See Ravallion (2001) for an accessible introduction to these methods. Also, see Zhao (2004) for a comparison of the statistical performance of matching estimators.

used in the enrollment estimation.[3] He found that the school feeding program increased attendance by 1.34 days per month, equivalent to 6 percent of total school days per month. This result was also statistically significant.

These results provide evidence of a fairly strong impact by the school feeding program on school participation. Nonetheless, these findings rely on the assumption that there are no unobservable characteristics of households living in program communities that affected both the household's access to the program and its decisions about school enrollment or attendance. Another possible caveat is that the data collection did not include any data for two of the three control areas before the implementation of the program in the treatment communities. Therefore, it is not possible to determine whether the enrollment differences were due to the program or whether the control communities had lower enrollment before the introduction of school feeding for reasons that were not controlled for in the regression. Finally, because the error term in the regression may be correlated across students within a community, standard errors of the impact estimates should have been modified to control for community clustering in the sample design. Without such controls for community clustering, standard errors may be underestimated, causing the significance level of the results to be overstated.

Ahmed and del Ninno (2002) investigated the impacts of a take-home rations program in Bangladesh on educational attainment, including school participation. The FFE program provided a free monthly ration of foodgrains conditional on children attending primary school. The cost of the program in 2000 was US$0.10 per beneficiary student per day. Although these per child costs are comparable to those in Bangladesh's in-school feeding program, the FFE program should have been cheaper to operate at the community level, because it was targeted to poor households. Children must have attended 85 percent of classes in a month to be eligible for that month's ration. School enrollment in FFE schools increased by 35 percent from the year before the program was put in place to 1 year after implementation. School enrollment in nonprogram schools showed an increase of only 2.5 percent during the same period. This significantly higher increase in program schools cannot be attributed to the take-home rations without further investigation. Ahmed and del Ninno did this by analyzing the impact of the program on enrollment at the household level while trying to control for the endogenous nature of program participation. Their method for controlling for endogeneity was to use instrumental variables, but the approach suffers from some limitations due to a weak set of available instruments.[4] They found that if a household received the sample mean ration

[3]Ahmed (2004) also included an additional control variable representing the number of days the child was sick in the last month.

[4]The instrumental variable used for child participation in the FFE program was whether the program was available in the union of the child's residence. Though they attempt to justify this instrument

for 5 months, then the probability of one of their children being enrolled in school increased by 7.9–8.4 percent, depending on the specification used, compared to receiving no ration.

Ahmed and del Ninno (2002) also examined the impact of the program on attendance. School attendance data were taken from school registers. The overall rate of attendance in program schools was 70 percent, compared to 58 percent in nonprogram schools. The authors were concerned that because a minimum level of school attendance is a condition for schools to receive the take-home rations, attendance may have been overstated in school registers at program schools. Therefore, survey enumerators took attendance on unannounced visits, and the attendance results were found to be similar to those in the attendance records. Again, based on their methodology, it is unclear to what extent this difference can be attributed to the impact of the school feeding program.

Vermeersch and Kremer (2004) used data from a randomized school feeding evaluation in Western Kenyan preschools between 2000 and 2002 to evaluate program impacts on school participation and achievement. This study had an extremely well-planned randomized field-study evaluation design. Unfortunately, participation in preschools in Kenya is not the norm; therefore, it is difficult to assess the relevance of these results for a primary school feeding program in a country with a policy of achieving universal primary education.

Twenty-five preschools were randomly selected from 50 to receive a fully subsidized in-school breakfast. Prior to the introduction of the breakfast program, the treatment and control preschools had very similar characteristics. The sample of children Vermeersch and Kremer (2004) were interested in was all children between the ages of 4 and 6 who lived within walking distance of a school in their sample, and they would like to have known which school parents would have chosen for their child in the absence of the program. This choice is unobservable, but Vermeersch and Kremer spent considerable effort trying to identify an estimate of this population. They assumed that parents would send their younger children to the preschool attached to the primary school attended by their older children. They were then able to identify a sample of children for each school who either attended the preschool or whose older sibling(s) attended the attached primary school. Unfortunately, they missed children who did not attend preschool and who did not have older siblings or who had older siblings who did not attend school. This study demonstrates the difficulty in estimating the true impact of a school feeding program on enrollment.

Vermeersch and Kremer (2004) were able to calculate an intent-to-treat estimator of the effects of the program on school participation. This approach estimates

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using methods applied in Ravallion and Wodon (1997), they cannot rule out persistent bias in their estimates from program placement effects.

the average impact of the program on school participation for all children assigned to the treatment group, regardless of whether they received the program, rather than estimating the impact of the program only on those who received it. Policymakers often prefer to know the latter. Vermeersch and Kremer's approach assigned children to the treatment status of the school they belonged to in the baseline, regardless of which preschool they actually attended. This issue is important in this setting, because it is very easy for children to transfer between preschools. There are many preschools within walking distance from a child's home. Furthermore, Vermeersch and Kremer noted that this estimator may be biased in their study because of imprecision in the data.

Using individual-level regressions that allowed for child- and school-level controls, Vermeersch and Kremer (2004) found that average school participation was 8.5 percentage points higher in the treatment group than in the control group, with children in the treatment group participating 35.9 percent of the time versus 27.4 percent for the children in the control group. This difference was statistically significant and varied depending on whether a child attended preschool at the baseline. If they did not, they had average participation rates of 16.4 percent in treatment schools and 14 percent in the comparison group. If a child was in school at the baseline, they had an average participation rate of 64.7 percent in the treatment group versus 50.7 percent in the control group. These numbers demonstrate that the effect was larger for children enrolled in preschool before the introduction of school feeding.

Finally, Vermeersch and Kremer (2004) investigated whether the higher participation rates in program preschools was due to an increase in the participation rates of children in treatment schools who were attending school at the baseline or whether it was due to an increased number of participants attending preschool at the same rate as in the control group. They addressed this question by regressing a dummy variable for whether a child attended preschool at least once on a dummy variable for whether the child was associated with a treatment school at the baseline, on a set of individual-level control variables and on school dummy variables. Vermeersch and Kremer found no significant effect of being enrolled on the treatment status of the preschool. Therefore, they concluded that the increases in participation rates seen in treatment schools were due to increases in the participation of children who were already attending preschool before the implementation of school feeding.

Age at Entry

To our knowledge, no existing study has investigated the impact of school feeding on age at entry into primary school, which is likely due to the difficulty in collecting such data. This issue is similar to that of estimating the impact of school feeding on the enrollment and attendance of all children in the service area of a school described in the first section of Chapter 4. The estimation of the impact of school feeding on age at entry requires data on students who have not yet started primary school prior to the introduction of the feeding program. It is often the case that samples are drawn from schools and not from the total population. In most circumstances, to draw a random sample requires a complete population list. It is easier to collect data on all students attending school than it is to generate a list of all school-aged children living in the service area of a school, especially in developing countries. As a result, this outcome has not received adequate attention. This gap in the literature is potentially serious, given that the economic implications of delayed schooling may be large.

Drop-Out Rates

The evidence of the impact of school feeding on drop-out rates is inconclusive. Several studies have found a positive effect of school feeding programs, both in-school meals and take-home rations, on reducing the drop-out rate. Unfortunately, these studies suffer from statistical problems. Additionally, several studies have found no evidence of an impact of school feeding on drop-out rates, though these studies also have problems in the approach used to identify causal impacts.

Ahmed (2004) found that the in-school meals in Bangladesh described above reduced the probability of dropping out by 7.5 percent, based on an econometric specification similar to the ones used to calculate the impact of the program on enrollment and attendance in this study.

Ahmed and del Ninno (2002) found that the FFE program in Bangladesh that provided take-home rations conditional on attendance had an impact on drop-out rates. They argue that they are able to identify this effect because, within schools operating the program, only approximately 40 percent of students received the take-home rations. Therefore, they compared the drop-out rates of students who received the rations to those who did not within program schools and attribute the difference to the FFE program. From 1999 to 2000, about 6 percent of beneficiaries dropped out of school, compared to 15 percent of nonbeneficiaries in the program schools. Although this result does lend support to take-home rations having a positive impact on decreasing drop-out rates, the authors are unable to argue that this is a causal finding using the data available to them.

The government of the Philippines initiated the Dropout Intervention Program in 1990–1992, which included a school feeding program being randomly assigned to 10 schools in low-income areas of the country. Tan, Lane, and Lassibille (1999) have pre- and post-intervention data for these schools, as well as for 10 randomly selected control schools. Data were collected for all students in all grades in the sample schools. The school feeding program was implemented alone in five schools and with parent–teacher partnerships in five.[5] The authors computed the DID estimates of the impact of the school feeding program—the difference in the change in drop-out rate over time between the treatment groups and the control group at the school level. Though the estimates do suggest that the school feeding program, both alone and with parent–teacher partnerships, decreased drop-out rates, these results are not statistically significant. Furthermore, a regression model could not identify any impact of either program on the probability of a student dropping out.

Learning Achievement

Most empirical findings suggest that school feeding programs have a positive impact on learning achievement, as measured by increases in test scores. As with all school feeding outcomes investigated empirically, there are several econometric issues that raise questions about the validity of these results. Furthermore, the subject of the achievement test seems to matter. In general, school feeding does not seem to have the same impact on all subjects, even within a given study. Additionally, test scores improved as a result of school feeding for different subjects across studies.

Ahmed and del Ninno (2002) found that the FFE program in Bangladesh described in the first section in Chapter 4 had a statistically significant negative impact on the achievement test scores of fourth-grade students in program schools compared to those in nonprogram schools. Ahmed and del Ninno investigated whether this result is because of the increased class size that was seen in program schools. In this study, not all students in the program schools received the take-home rations. They found that nonbeneficiary students performed similarly in both program and nonprogram schools, and that the difference in test scores resulted from the lower scores of the beneficiary students. They claim that this result stems from the lower socioeconomic status of beneficiaries. Given the empirical approach, this result cannot be interpreted as causal, because the authors cannot control for other factors that may have lead to differences in learning achievement.

Ahmed (2004) evaluated the impact of the in-school meal program in Bangladesh on test scores using data on achievement test scores for 1,648 students in grade 5 attending primary school. Using an econometric specification to isolate the effects of the program, he found that students in program schools score 15.7 percent higher than did students in the control schools. This increase is statistically significant. He further decomposed this increase into the three subjects that make up the total score and found that the improvement was due mainly to an increase in the mathematics

[5]Parent–teacher partnerships involved an adjustment in the way parents interacted with teachers.

test score. He controlled for child, household, and school characteristics as well as location-specific fixed effects in the regressions. One issue with this specification is that he controls for the total number of students in the classroom in the regressions and finds that it has a significant negative impact on the English test scores, though the size of the impact is quite small. It is unclear whether one should control for class size when estimating the impacts of FFE programs, because the introduction of the program is likely to have an impact on class size if the program increases enrollment and attendance without an increase in the number of teachers or classes. Ahmed (2004) found that the average number of students per teacher was higher in program schools compared to control schools: on average 72 versus 65.

Tan, Lane, and Lassibille (1999) evaluated the impact of the school feeding program in the Philippines, described above, on the school performance of first-grade students. Because of data issues, the authors only had data on first-grade students' achievements pre- and post-intervention. The impacts of the school feeding program were not significant at the school level. At the student level, the authors estimated the child's academic performance as a function of their previous academic achievements, child characteristics, family background, the learning environment, community characteristics, and a dummy variable representing the program. One issue with this specification is that one of the explanatory variables is the lagged dependent variable. Therefore, it may be correlated with the error term. The authors corrected for this by instrumenting for the lagged dependent variable using the lagged values of scores on other tests, but the validity of this approach is questionable.[6] The authors found that school feeding, either alone or with parent–teacher partnerships, had a positive and statistically significant effect on English test scores. Furthermore, school feeding coupled with parent–teacher partnerships had a positive and significant impact on mathematics test scores. One difficulty with this study is that each program, school feeding alone and with parent–teacher partnerships, was only implemented at five schools. Therefore, it is hard to rule out the possibility that individual school characteristics biased the results.

Vermeersch and Kremer (2004) administered two attainment tests, one oral and one written, 2 years after the introduction of the in-school meals program described in the first section in this chapter. They found that the school meals increased test scores in schools where the teacher was experienced. This result was found by regressing the test score on both a treatment variable as well as a treatment variable interacted with the teacher's experience. These regressions also included

[6]There is an additional econometric issue with this specification. A selection bias exists because the weakest students are those who are most likely to drop out. Therefore, this analysis is performed on a censored sample. The authors used two different methods to correct for this effect as well as reporting the results without correcting for the selection bias. The results do appear to depend on which correction method is used.

additional controls. Vermeersch and Kremer found an increase in test scores of 0.38 standard deviation in schools with a teacher experience level that is 1 standard deviation higher than average. The treatment impact alone was not significantly different from 0. The authors note that the school meals program increased class size and displaced teaching time, which may explain why children without well-trained teachers did not improve their test scores.

Cognitive Development

Many studies have investigated the impact of nutrition on cognitive development. Several different aspects of nutrition have been explored. Many of these are relevant to the school feeding literature. Among them are the effect of breakfast on the cognitive performance of preschool-aged and primary-school-aged children, the impact of animal-source foods on the cognitive performance of primary-school children, and the impact of micronutrient-fortified foods on cognitive development. Additionally, several measures of cognitive development have been considered, including tests of verbal comprehension and arithmetic tests. Finally, effects are often greatest for children with low nutritional status.

Whaley et al. (2003) investigated the impact of animal-source foods (meat and milk) on the cognitive development of rural Kenyan primary-school children using a randomized school feeding intervention. The outcomes measured were changes in cognitive test scores during the intervention, which began in 1998. Twelve schools with a total of 555 Standard 1 children were randomized into one of the four feeding groups: meat; milk; energy; or the control group, which did not receive any feeding. For those schools that received an intervention, the program was a mid-morning snack that continued for 21 months. The measurement of food intake of the children from the mid-morning snack and the scoring of the cognitive development tests were carried out extremely thoroughly. Cognitive tests included the Raven's Colored Progressive Matrices, which were used to assess abstract and performance perceptual abilities, a verbal meaning test, and an arithmetic test. In addition, some household data was collected, which included maternal literacy and socioeconomic status variables. Results showed that children who received the meat supplement showed significantly greater gains on the Raven's Progressive Matrices than did all other groups. Children in the milk and energy groups did not outperform children in the control group. The four groups did not differ significantly on their performances on the verbal meaning test. Children in the energy and meat groups statistically outperformed children in the control group on the arithmetic test, and children in the energy group outperformed those in the milk group. The authors conclude that these results show that diet quality and quantity are predictors of arithmetic performance. This careful study demonstrates that animal-source foods as well as energy have a positive effect on primary-school children's results on cognitive tests that measure arithmetic and perceptual abilities. One shortcoming of this study is that there were only three schools per treatment, and so it is possible that the results may be in part due to school quality differences among the groups.

Van Stuijvenberg et al. (1999) studied the effects of iron-, iodine-, and β-carotene-fortified biscuits on the cognitive function of primary-school children in grades 1–5 in rural South Africa using a randomized experimental design. Iron and iodine deficiencies are known to affect the mental development and learning ability of children. Iron deficiency can also increase susceptibility to infections, which can affect school attendance and performance. One hundred fifteen children aged 6–11 were randomly assigned to the treatment group that received fortified biscuits and a vitamin C–fortified drink daily during the first 2 hours of the school day. The control group comprised 113 children who received nonfortified biscuits and an unfortified drink at the same time as the children in the treatment group. The randomization was done within classes; children in each class were randomly assigned to either the treatment or the control group. This method is useful if there are differences in cognitive abilities among classes. Cognitive function was assessed at the baseline and again after 12 months of receiving the biscuits for 135 children in the study who were in grades 2–4. The cognitive tests were designed to measure a range of mental and fine-motor skills, including verbal learning, visual memory, arousal, attention, retrieval, eye–hand perception, and coordination. The tests were designed to record speed of processing and capacity of working memory. In addition to receiving the biscuits, all children in both the control and the treatment groups were dewormed. The results showed a significant impact of the treatment on the scores of the digit-span forward task, a measure of short-term memory and attention. In addition, children in the treatment group missed significantly fewer school days compared to controls because of respiratory- and diarrhea-related illnesses. The study found no treatment effect on the other eight cognitive function tests. Finally, greater treatment effects on the digit-span forward task were found in the children with low micronutrient status, low serum ferritin, and low hemoglobin concentrations and those with goiter at the baseline. The authors believe that the lack of significant results on many of the cognitive function tests may result from the confounding effect of short-term hunger. Both the fortified and unfortified biscuits had equivalent amounts of macronutrients and thus reduced short-term hunger for both the control and treatment groups. If short-term hunger affects results on cognitive function tests, its alleviation in both groups may explain the lack of significant results.

Pollit surveyed the literature on the effects of breakfast on cognition. He concluded that a morning and overnight fast has adverse effects on cognition for at-risk individuals, particularly for speed of information retrieval from working memory.

This literature includes experiments performed in the United States (Pollit, Leibel, and Greenfield 1981; Conners and Blouin 1982/83; Pollit et al. 1982/83), which showed that 9- to 11-year-old, well-nourished, middle-class children committed fewer errors on problem solving, vigilance, and arithmetic tasks when they were given breakfast. In one of these studies (Pollit et al. 1982/83), the subjects with the largest changes in glucose levels between the days when they received breakfast and those when they did not were significantly more likely to have committed more errors on the day they received no breakfast.

A study on low-income children in rural Jamaica (Simeon and Grantham-McGregor 1989) found that breakfast had no effect on cognitive performance in children whose height and weight were normal for their age, whereas nutritionally at-risk children improved their performance when they consumed breakfast. This study was conducted in a laboratory setting where thirty 9- to 10.5-year-old children were admitted overnight to a research ward on two occasions 1 week apart.[7] On one occasion, the child was given breakfast; on the other visit, they received only tea in the morning, and their outcomes on these two visits were compared. Half of the children were given the breakfast on the first visit, while the others were given the breakfast on the second visit. This order was randomly determined. The children were given cognitive performance tests at 11:00 in the morning on each visit. The malnourished children, those who were stunted or previously malnourished, had lower scores in fluency and coding when they fasted, whereas the adequately nourished group actually had higher scores in arithmetic when they missed breakfast. Additionally, wasted children did poorer on the digit-span backward tests when they fasted, children who were wasted and previously malnourished scored lower on problem solving when they fasted, and adequately nourished children had lower scores on digit-span forward on the days they missed breakfast. The authors conclude that missing breakfast had more of an effect on cognitive functions of poorly nourished children.

Vermeersch and Kremer (2004) administered an oral cognitive test in the randomized preschool meals setting in Western Kenya described in the first section of this chapter. They found no impact of the school meals on cognitive test scores. As described in the previous section, the authors did find an impact of the school meals on attainment test scores in schools with experienced teachers. They claim that the finding of no effect on cognitive test scores is an indication that the program did not improve nutritional status, or that it did not improve it sufficiently to affect cognition.

[7]The children were admitted two at a time.

Chapter 6

Empirical Evidence of the Impacts on Food Consumption and Nutrition

Assessing the impact of FFE programs on food consumption and nutrition can be complicated, given the economic and biological processes that can moderate or obscure discernable improvements. For example, if a household feeds a child receiving school meals less at home (if they "tax" recipients) as a result of the program, then the child's total food consumption level may change only slightly. If the child is not taxed, the increase in calories may coincide with an increase the child's activity level, and hence the number of calories burned in a day, thus improving the child's health without much change in the child's weight or height. Furthermore, weight gain without a corresponding increase in height may not be beneficial to children with moderate or high baseline weight. Finally, if the school feeding transfer differs in nutrient composition from the child's normal diet, even if the child does consume less at home, he or she may still have a significant improvement in micronutrient status.

Most FFE evaluations focused on nutrition analyze one of three outcomes: food-energy (calorie) consumption, anthropometry, or micronutrient consumption or status. Given the potential for the benefits of school meals to be redistributed away from the recipient during later meals at home, a number of studies have focused on the food-energy intake of the children as a final nutritional impact indictor. Such studies address only how food intakes change, providing only partial evidence of the programs' effect on nutritional status, though intakes are arguably the outcome that best captures the ramifications of the program for household behavior. Other researchers concerned with micronutrient deficiencies have measured intakes of micronutrients, such as iron or zinc, when nutritional status for these nutrients was too expensive to measure. However, to understand the ultimate impact of the program on nutritional status, it is necessary to consider effects on anthropometry or micronutrient status in blood or urine concentrations. Here we review the evidence on the impact of FFE programs on several measures of food consumption and nutritional status.

Change in Food-Energy (Calorie) Consumption

Increasing energy consumption has the potential to benefit children with inadequate baseline energy intake by leading to weight gain or increased energy levels. Household behavior can mitigate changes in consumption stemming from FFE if children are fed less at home because they are fed at school. For children who are underweight or who have low daily energy consumption at baseline, this substitution can undermine the nutritional goals of FFE programs. However, if recipient children already consume sufficient calories, substitution may allow households to direct resources formerly used to feed school-aged children toward other household needs, which could ultimately improve the welfare of the household as a whole and the school-aged child.[1]

The studies reviewed here measure daily calorie intake using a 24-hour dietary recall instrument. Parents, or children if they are older than 8, are asked detailed questions about each meal and snack that their child (or they) consumed during the previous day, including the portion size and ingredients used in preparing the meal. Total intake (in grams) of each food or ingredient is calculated, which in turn allows for the calculation of nutrient intake through the use of a food composition table. Food composition tables may be developed especially for use in the study area, or standard tables may be accessed when local tables are unavailable or inadequate (for example, the food composition table in the World Food Dietary Assessment System software developed by the Food and Agriculture Organization of the United Nations: Calloway et al. 1994). To gain accurate data about an individual's daily consumption, these data should be collected multiple times and averaged (Basiotis et al. 1987).[2]

Given the difficulty of collecting 24-hour dietary recall data, only a few school feeding evaluations analyze individual-level dietary intake.[3] While one study (Mur-

[1]Although it is possible that increasing calorie consumption among children who already consume sufficient calories may lead to obesity, none of the studies reviewed below addressed this concern.

[2]Although 24-hour recall data have benefits compared to methods described below, accurately assessing intake is difficult, as respondents must recall very detailed information from the previous day. Obtaining this information from children, particularly from those younger than 8, is particularly difficult. Additionally, unobservable psychological factors, including body image, embarrassment, or desire to please, may cause under- or over-reporting. Incorrect descriptions of foods or preparation and nonstandard units of measurement also make calculating micronutrient intake difficult. (See Gibson 2005 for a review of these potential errors.)

[3]Babu and Hallam (1989) and Dall'Aqua (1991) show some nonexperimental evidence that total house-

phy et al. 2003) showed clear evidence that children's at-home consumption fell as a result of participating in two types of school feeding programs, the majority of evidence in the literature suggests that school feeding is an effective tool for increasing caloric intake among school-aged children. In fact, three recent studies (Jacoby 2002; Ahmed 2004; Afridi 2005) show that children's daily intake increases by almost exactly the size of the transfer. However, these studies differ from the Murphy et al. (2003) study both in empirical methodology and subjects' baseline energy intake.[4] School feeding programs seem particularly effective at increasing participants' intake in communities where average daily food consumption is well below the recommended level for the child's age. Although consumption appears to increase more in worse-off communities, there is some evidence that larger and poorer households within communities redistribute more of the feeding transfer within the household, whereas better-off households let the intended recipients keep all of the calories transferred (Jacoby 2002; Ahmed 2004; Afridi 2005).

To assess whether parents feed their children less at home when children are given breakfast at school, Jacoby, Cueto, and Pollitt (1996; see the first section of Chapter 4) analyzed consumption of total calories (energy), protein, and iron during three distinct time periods: before school, during school breakfast time, and after breakfast and through night. The program provided fourth- and fifth-grade children with a breakfast of 600 kcal and 9.5 g of protein. Jacoby, Cueto, and Pollitt did not report baseline energy adequacy, but baseline energy intake (roughly 1,880 kcal) was close to the recommended level for this age group, and given that, on average, weight-for-height was adequate in this sample, baseline energy intake was likely sufficient. Using a randomized treatment design, they found that after 2 months, although children's daily energy consumption fell 7 percent in control villages,[5] energy consumption increased significantly in treatment villages by 15 percent, or almost 300 kcal (50 percent of the transfer). All of the increase came during the school breakfast period (7 a.m.–11 a.m.), while at-home consumption did not differ significantly from that of the control group. They found similar results with protein intake, which increased by 16 percent from the baseline of 48 g for beneficiaries (and was 29 percent higher than the control group's intake), and iron intake, which increased by 60 percent over the baseline of about 13 mg (73 percent over the control group's intake).

The control and treatment groups in this analysis showed only minor differences in observable characteristics, and comparability of the outcome variable dur-

hold consumption increases as a result of FFE programs. However, these studies do not analyze the distribution of calories to different household members.

[4]Baseline calorie adequacy was reported for weight in only one study (Murphy et al. 2003) and for age and gender in one other (Ahmed 2004). Other baseline adequacy calculations are based on age and gender recommendations from FAO (1985). Reported data were insufficient to calculate adequacy by weight in any study.

[5]This decline was statistically not significantly different from 0.

ing baseline, coupled with the short treatment period, suggests that these differences between the treatment and control groups did not drive the results. Interestingly, Jacoby, Cueto, and Pollitt (1996) presented only statistical comparisons of post-treatment outcomes for the treatment versus control groups. Had they employed a DID approach (comparing the change in consumption for the treatment group to change in consumption for the control group), they would have found that the program led to an increase in consumption of nearly 450 kcal.[6]

Murphy et al. (2003) used a clustered randomized trial design to evaluate a school feeding experiment in 12 Kenyan primary 1 classrooms (roughly 7- to 8-year-olds) on its impact on consumption. As described above (see discussion of Whaley et al. 2003), this study included three different treatments plus a control group (Table 6.1).

At baseline, consumption appeared adequate, given children's weight, though without data on children's activity level, true energy adequacy is unknown. However, baseline consumption of iron, vitamin A, vitamin B_{12}, and zinc were reportedly low at baseline for this sample. Changes in food consumption were analyzed independently for total consumption and consumption at home in the first 3 months of the program and then five more times within 2 years. Again, the analysis presents comparisons of means through *t*-tests and analysis of variance, relying on the randomized design to show causality.

Although parents were asked at the beginning of the experiment not to change their children's diets at home in response to school feeding, after 1–3 months, at-home consumption changed significantly from the baseline in all three treatments and in the control group. Children in the control and meat groups increased at-home consumption by 196 and 140 kcal, respectively; children in the energy and milk groups began consuming fewer kcal at home (–126 and –129 kcal, respectively). Total energy intake, therefore, only increased significantly (over baseline and control groups) for the meat group—a total increase of 381 kcal, or 147 percent of the intended transfer, from the baseline and 185 kcal compared to the control group. On average over all six visits, comparisons of treatment groups to the control group were similar to the results from the first visit. In particular, the meat group consumed, on average, 233 kcal more than the control group, which is almost 100 percent of the intended transfer.

Although energy consumption did not change for two of the treatment groups, consumption of several key nutrients did increase for the milk and meat groups in the first 3 months. Intake of protein, vitamin B_{12}, riboflavin, vitamin A, iron, and

[6]Another potential concern, however, is that standard errors that were presented did not appear to take the clustered design of the treatment into account and may, therefore, have been biased downward, thus overstating the significance of the results.

Table 6.1 Study design for Murphy et al. (2003) experiment in Kenyan primary schools

Treatment	Foods	Energy (kcal)	Protein (g)
Energy (n = 140)	Vegetarian stew	239	8.6
Milk (n = 137)	Vegetarian stew and 200 ml milk	262	11.0
Meat (n = 124)	Vegetarian stew and 60 g beef	259	16.6
Control (n = 123)	No foods		

zinc increased significantly for the meat group. Intake of vitamin B_{12}, riboflavin, vitamin A, iron, and calcium increased for the milk group. The energy group saw improvements in vitamin A but consumed less vitamin B_{12} and zinc overall compared to the baseline.

Randomized trials like those used in Jacoby, Cueto, and Pollitt (1996) and Murphy et al. (2003) have the strongest design for identifying causal effects. However, designing a randomized trial to evaluate school feeding programs is often infeasible, especially for government-run programs, as food programs are typically targeted to the poorest communities and rolled out in order of need. Three recent studies (Jacoby 2002; Ahmed 2004; Afridi 2005) have evaluated the dietary impact of three school feeding programs already in progress using a quasi-experimental approach. By comparing children's caloric intake on days that they attend school to their intake when they do not go to school, these studies were able to identify the effect of school meals at the group level, making plausible claims that the measured effects represent causal impacts of the programs. Using this approach, these studies concluded that children's total daily consumption increases by 75–100 percent of the school feeding transfer on days that they participate in the program. This gain could be quite beneficial, given the low baseline intake of children in these samples.[7] Under this approach, a DID estimation isolates the effect of the program subject to the assumption that the difference in in-school and out-of-school consumption is the same in treated and control villages.

Jacoby's (2002) paper first employed this approach using a dataset from the Philippines in which many of the children had access to school feeding programs at their schools. These programs provided roughly 300 kcal on average as a midmorning or mid-afternoon snack. Each child in the dataset was interviewed one time on a randomly selected day. Because some interviews were conducted the

[7] Calculating true caloric need requires data on activity levels as well as age and body size, but children in the Jacoby (2002), Ahmed (2004), and Afridi (2005) studies were clearly below the minimal intake needed for their age, at not more than 75 percent of daily requirements on average. However, it should be noted that if children are stunted, their energy requirements may be reduced.

day after a weekend or school holiday, some children in a school reported caloric intakes for a school holiday whereas others reported for a school day.[8] Although Jacoby (2002) finds that, on average, few to no calories are taxed at home, children living in poorer households have a smaller increase in daily intake as a result of the program, suggesting that when household resources are limited, resources are more likely pooled.

Ahmed (2004) varied Jacoby's (2002) design by collecting 24-hour dietary recall data for the same child on school days and nonschool days. The benefit of this approach is that Ahmed is able to estimate a child-level fixed-effects model, controlling for any unobservable differences in children that may drive consumption patterns. Also, because nonschool days can include holidays, when consumption may change irrespective of school feeding, Ahmed asked parents to keep their children at home on randomly assigned school days, which served as the nonschool reference days. This study found that consumption on school days actually increases by nearly the size of the transfer (97.4 percent), but finds no difference in the consumption change based on household income.

In Afridi's (2005) study of an Indian school feeding program, all interviews from a particular village were conducted on the same day. However, within the program's administrative unit (the gram panchayat [GP]), she had data on both school day and nonschool day consumption. Therefore, she estimated a GP-level fixed-effects model, finding that between 79 and 86 percent of the transfer stays with the child. She also repeated Ahmed's (2004) method in a quarter of the villages and found that energy consumption increased by 75 percent of the transfer. Like Jacoby, this study found that consumption increases by less for children in poorer households and also found smaller increases in consumption in larger households. Interestingly, Afridi (2005) found that consumption of micronutrients and protein does not increase and in some cases decreases significantly, which Afridi concludes may suggest that parents substitute away an equivalent amount or more of the nutrient-dense foods that the school meals provide.

Overall, these studies suggest that school feeding can increase energy intake among school-aged children, particularly when baseline energy intake is low. Jacoby (2002), Ahmed (2004), and Afridi (2005) compared programs that were similar in terms of transfer size (300–400 kcal; 7.5–8.5 g protein) and baseline energy intake, and all found that consumption increased by roughly the size of the transfer, indicating that parents impose at most a small tax on children's at-home consump-

[8] Jacoby's ability to compare school days and nonschool days for children in the same school makes it possible to reduce bias in the impact estimates by controlling for fixed unobservable characteristics of the schools that may have determined program placement. In addition, in-school analysis on these data should wipe out the differential income effect that variations in programs across sites introduce.

tion when they are fed at school. On the other hand, Jacoby, Cueto, and Pollitt (1996) concluded that consumption increases by only half the size of the 600-kcal transfer (though a preferred DID comparison of the change in consumption would have shown that consumption increases by more than two-thirds of the transfer). Murphy et al. (2003) found that consumption increases by more than the size of the transfer when children were served meat as a snack, whereas consumption does not increase significantly for children served milk or energy snacks of equivalent caloric value. In terms of nutrient and caloric content, the energy and milk snacks in the Murphy et al. (2003) study are similar to the snacks in the Jacoby (2002), Ahmed (2004), and Afridi (2005) studies (though the milk snack had slightly more protein), and yet the outcomes were very different.

The similarity in program components suggests that perhaps variations in initial conditions or in the populations of these school feeding areas, rather than program components, are driving the differences. Caloric intake without school feeding, as a percentage of needs, was lower in the Jacoby (2002), Ahmed (2004), and Afridi (2005) studies compared to that in both the Jacoby, Cueto, and Pollitt (1996) and Murphy et al. (2003) studies, so children in the Afridi, Ahmed, and Jacoby studies had a greater potential to benefit. Increased intakes detected in any of these studies may be due to children's hunger levels—they may choose to eat less at home after having a school meal—not substitution. Murphy et al. (2003) suggest that changes in hunger may be responsible for differences between children receiving meat and those receiving milk- or vegetable-based snacks. Children in the meat group, whose consumption did increase significantly during the study, also showed evidence of increased activity level, perhaps increasing their caloric demand.

These findings suggest that school feeding programs are most effective at increasing average intake in communities where intake is well below the recommended level. However, given the differences in empirical approach between the quasi-experimental studies, which found a larger increase in intake, and randomized trials, which found less consistent results, more information is needed to make firm policy conclusions about these programs' effectiveness. Moreover, Jacoby's (2002) and Afridi's (2005) findings that children in poorer or larger households saw lower increases in intake suggests that more analysis on the differential impacts of school feeding within a population is important.

Anthropometry

Anthropometric indicators provide useful summary measures of nutritional status based on measures of body size and composition, often relative to their distribution in a reference population. Anthropometric indicators measure achieved nutritional status, rather than nutrition inputs, are less subject to measurement error, and are less expensive to collect than intake data. However, changes in anthropometry reflect only net changes in health and nutrition and cannot identify the cause (for example, reduced morbidity, increased macronutrient consumption, increased intake of zinc) of these changes.

The most common measures of anthropometry for children used in economic literature are weight-for-height, weight-for-age, and height-for-age *z*-scores (WHZ, WAZ, and HAZ, respectively), which are calculated as the difference of the child's height or weight from the mean height or weight of a standard reference population, measured in units of standard deviation. Weight-for-height is more sensitive to current consumption and is typically used to identify short-term or current nutritional deficiencies. Height-for-age reflects a child's nutritional history and is therefore used to indicate past nutritional deficits. Weight-for-age reflects both current and past nutritional status.

Additional measures include body mass index (BMI), mean upper-arm circumference (MUAC), and mid–upper-arm muscle circumference (MUAMC). BMI, or weight over height squared, measures thinness/fatness and is used for adolescents and adults (and, increasingly, for school-aged children). Thinness and obesity cutoffs vary by age. MUAC and MUAMC are measures of body composition. MUAC is particularly useful in crisis settings, as it can quickly and easily be measured and can detect small changes in fat tissue and muscle mass—an indicator of protein-energy malnutrition. MUAMC is a refined measure used to estimate total body muscle mass and is less sensitive to brief changes in muscle mass that may occur during illness.

In the context of school feeding, changes in anthropometry can reflect two mechanisms. First, increased caloric intake can lead to weight gain and, in some circumstances, to height gain. Second, micronutrients in fortified school meals can help to contribute to growth and gains in muscle mass. Reducing zinc deficiencies, for example, can help to accelerate growth and improve appetite. Adequate stores of zinc, vitamin A, and iron reduce susceptibility to infection and hence improve growth.

Most studies assessing the effect of school feeding on anthropometry come from the nutrition literature and use randomized trials to evaluate the effects. One such randomized study was conducted among seventh-grade students selected because of low scholastic performance in a Jamaican school (see Simeon 1998 for a synopsis of results). Additionally, approximately half of these children were considered undernourished (weight-for-age below 80 percent of the reference standard using the Wellcome classification). Although the study found no impact of a 500-kcal school meal on weight-for-age, the time frame for this intervention was only 2 months, which may have been too short to detect weight changes, particularly in older children. Additionally, the study included only one treatment group of

44 students and two controls (totaling 77 students), providing very low statistical power to detect changes.

A larger Jamaican study of second- through fifth-grade classrooms was reported in Powell et al. (1998; cited earlier). This study was the randomized trial of the impacts of school breakfasts against a placebo (orange slices) with random assignment of children into treatment or control groups in the same classroom. At the end of the 8-month intervention, children in the treatment group showed small but significant improvements in height, weight, and BMI. Heights improved by approximately 0.25 cm over the control group, and weight increased by 0.4 kg over the control group. These gains were detected even among children who were adequately nourished at the baseline. The authors did not, however, report on whether this weight gain was excessive for any children.

Grillenberger et al. (2003), as part of the same Kenyan evaluation cited above (Murphy et al. 2003), also found small improvements in some anthropometric indicators after 23 months, even in the milk and energy treatment groups, which showed no increase in caloric intake. Children in all three treatment groups—energy, milk, and meat—gained 10 percent more in body weight than did children in the control group. However, there were no improvements in height or height-for-age for the energy, milk, or meat groups, though children in the milk group who were stunted at the baseline had small height gains. Additionally, children in the treatment and control groups actually saw a reduction in weight-for-height over the intervention period, likely because of a drought that affected the region during the intervention. As children in the energy and meat groups saw a smaller drop in WHZ, a DID estimate may have revealed significant increases in these groups relative to the milk and control groups.

Body composition outcomes varied more within treatment groups. Children in the meat and energy groups gained significantly more in MUAC compared to the control (the milk group had a small increase that was significant only at the 10 percent level). And, although all three treatments increased in MUAMC (by 90 percent for meat, 50 percent for milk, and 70 percent for energy) compared to the control, the meat group also increased by significantly more than the milk or energy groups.

It is surprising that all treated children gained roughly the same amount of weight, while caloric consumption increased significantly only for the meat group. However, anecdotal evidence suggests that the activity level of children in the meat group increased more than in the other groups. The authors conclude that this increase is likely due to the increased availability of iron and zinc. The zinc probably also contributed to the meat group's significant gain in muscle mass.

In a study from South Africa (Van Stuijvenberg et al. 1999), fortifying FFE meals with micronutrients had no impact on anthropometric outcomes. Primary-school children (aged 6–11) were randomly assigned (at the individual level) to receive either a biscuit fortified with iron, iodine, and β-carotene or an unfortified biscuit with the same number of calories and grams of protein. The prevalence of iodine and vitamin A deficiencies was high at baseline; however, wasting and stunting were not serious problems. After 12 months, there were no differences between the control and treatment groups in weight, height, WAZ, or HAZ, which was not surprising to the authors, given the baseline anthropometry and the goal of improving micronutrient status, rather than weight or height.

In a separate study of 6- to 8-year-olds in South Africa, 6 months of iron-fortified food coupled with deworming treatment significantly increased height, HAZ, and WHZ among children with low baseline iron stores compared to unfortified food with no deworming treatment and compared to either treatment alone (Kruger et al. 1996). Weight and weight-for-age also improved with this combined treatment in children with adequate baseline iron stores. The authors conclude that iron deficiency is likely a limiting factor in (height) growth in this population, but that iron supplementation only enhances the effect of deworming on growth. This finding lends support to the idea that school feeding is more effective when combined with deworming.

A limitation of these nutritional studies is that they cannot assess the effects of school feeding in a less controlled environment. Ahmed (2004) evaluated the effect of the Bangladesh school feeding program on children's BMI, finding an increase in BMI of 0.62 points due to the program, or 4.3 percent of original BMI. This gain would be equivalent to a healthy average-height 6-year-old gaining an additional 0.85 kg or a healthy average-height 12-year-old gaining 1.4 kg due to participating in the program. Ahmed regresses the BMI of sample children on a dummy for whether the child participated in the school feeding program and on household composition, economic, education, and health variables, as well as program and location variables. This vector of covariates controls for observable differences between participating and nonparticipating children. Nonetheless, unobservable differences in the comparison groups may drive some of the differences detected. Compared to other studies, which found less than a 0.5-kg treatment effect (if any), this effect appears large. The difference compared to other studies may arise from the severe undernourishment of Ahmed's population at the start of the program or to the treatment length, which is longer (1.5 years) than all but one study reviewed above. However, given the potential for bias arising from noncomparable treatment and control groups, the treatment effect may be overstated in the Ahmed (2004) study. The complications involved in Ahmed's approach highlight the need for randomized evaluations of actual government and NGO FFE programs to evaluate the impacts on anthropometry.

The evidence reviewed above suggests that FFE programs have the potential to increase children's body size and muscle mass through increased caloric intake or provision of micronutrients. Given the complex mechanisms determining the effect of energy and micronutrient intake on body size and composition, it is difficult to assess the effectiveness of FFE programs on these outcomes. Most studies show evidence of at least small increases in body size or composition, but the mechanism for this increase is unclear. Additionally, no study provided sufficient data on pre- and post-treatment activity levels to determine whether changes in energy use may have affected outcomes. Simply increasing access to calories over a sufficient period appears to increase body size (Powell et al. 1998; Grillenberger et al. 2003; Ahmed 2004; though not Simeon 1998). However, iron and/or zinc content, rather than energy content, appear to play a role in increasing height, HAZ, and WHZ in Kruger et al. (1996) and body mass in Grillenberger et al. (2003) in micronutrient-deficient children, but iron, iodine, and β-carotene appear to have no impact on anthropometric indicators in Van Stuijvenberg et al. (1999). Deworming also appears to have a significant interactive effect with FFE on growth in South Africa (Kruger et al. 1996), though no other studies disentangled the effect of deworming and FFE versus FFE alone.

Micronutrient Status

Providing foods rich in micronutrients that are not a regular part of children's diets may help to reduce micronutrient deficiencies. Lack of diet diversity and a high prevalence of infection in many developing countries can contribute to inadequate micronutrient status. However, inexpensive supplements and provision of food that is not typically part of a child's diet can improve micronutrient status and hence improve concentration and growth and reduce morbidity. Key micronutrients provided in school meals are iron, zinc, and vitamin A, which all improve resistance to infection and improve growth; iron supplementation has been linked to improved cognitive ability. Micronutrient intake can be measured using the 24-hour food recall approach, but infection and food interactions can prevent micronutrient absorption. Micronutrient status can be measured by a range of biochemical indicators requiring samples, usually of blood and/or urine.

At present, all studies of the impact of school feeding on micronutrient status come from the nutrition literature. All studies reviewed here employ a randomized trial design to identify causality. With the exception of the Kenyan animal-source foods study (Siekmann et al. 2003), all studies compare the impact of providing fortified meals to providing similar unfortified meals. Studies differ in the approach to randomizing the treatment: Walter et al. (1993), Kruger et al. (1996) and Siekmann et al. (2003) randomized at the school level, whereas Van Stuijvenberg et al. (1999) randomized at the individual level.

All of the micronutrient evaluations reviewed analyzed the effects of iron-rich school meals on iron status. And, despite variations in baseline iron status measures in the studies, all but one found a significant impact of school meals on iron status. In a study of older school-aged children in Chile, Walter et al. (1993) found that fortifying school snacks with bovine hemoglobin concentrate significantly increased hemoglobin concentration and decreased the prevalence of children with low iron stores. These increases were detected despite low levels of baseline anemia in the study region, but were more noticeable in children with higher iron demands (post-menarchial girls and pubertal boys).

Similarly, in two studies in South Africa (Kruger et al. 1996; Van Stuijvenberg et al. 1999; both described above), adding iron to school meals was associated with improved hemoglobin concentrations and serum ferritin concentration. In the two South African studies, some or all children were given deworming treatments in addition to iron supplements, which can improve iron status even without increased iron absorption. However, improvements were seen over and above the effect of deworming. In children with low-baseline iron stores, combining the deworming and iron treatments seems particularly beneficial compared to either treatment alone in reducing infection and improving hemoglobin concentration (Kruger et al. 1996).

Providing iron-rich meat snacks in the Kenyan study did not have an impact on any of the iron status measures analyzed (hemoglobin, plasma ferritin, or serum iron), despite low baseline hemoglobin concentrations for nearly 50 percent of children and low serum iron concentrations for 52 percent (the prevalence of low plasma ferritin concentration was low). Malaria was reported to have affected concentrations of all three measures, which the authors suggest may have influenced their findings. And although all children in this study were dewormed at baseline, there was no follow-up treatment during the 12-month intervention. Thus, children may have been reinfected during the course of the study, reducing the impact of iron fortification on these indicators.

Siekmann et al. (2003) also reported a high prevalence of vitamin A, vitamin B_{12}, zinc, and riboflavin deficiencies among subjects at baseline. These deficiencies were detected as low concentrations of plasma retinol, plasma vitamin B_{12}, serum zinc, and red-blood-cell riboflavin. Despite providing foods rich in these micronutrients, a treatment effect was detected only for vitamin B_{12} among children in the meat and milk groups (the energy school meal provided no vitamin B_{12}). The authors conclude that malaria may have mitigated the effects of the nutrient-rich food on micronutrient status.

The β-carotene fortification of school meals has been effective at reducing the prevalence of low serum retinol concentration in another context. Van Stuijvenberg et al. (1999) found that fortifying meals with β-carotene significantly reduces the prevalence of low serum retinol concentration after 6 months of treatment. The authors do not mention the presence of malaria as in the Siekmann et al. (2003) study, though they do report a significant drop in illness-related absences as a result of the program, suggesting that illness did not have the confounding effect that it may have had in the Siekmann study. Van Stuijvenberg et al. (1999) also observed a significant reduction in iodine deficiencies from iodine fortification. The prevalence of low urinary iodine concentrations fell substantially in the intervention groups after 6 months.

Despite the limited impact of school feeding programs on micronutrient status in the Kenyan study (Siekmann et al. 2003), fortified school meals appear to reduce the prevalence of micronutrient deficiencies among school children. Programs may be more effective at increasing micronutrient status among children with low baseline indicators (Kruger et al. 1996) or with higher micronutrient demands (Walter et al. 1993). Additionally, combining school feeding with treatments to reduce infection, such as deworming, may increase the effectiveness of FFE programs.

Chapter 7

Other Programs Providing Schooling Inputs

FFE programs have direct educational and nutritional goals: increasing school participation, improving test scores and cognitive development, and improving children's health. Other school-based programs may also help to achieve some or all of these goals by reducing the costs associated with sending children to school, increasing the benefits of attending school, or reducing children's health constraints. For example, cash-for-education programs, such as Mexico's PROGRESA program (now called Oportunidades), increase school participation by reducing the opportunity cost of sending a child to school rather than to work or by offsetting tuition, uniform, or supply costs. Programs aimed at reducing teacher absence or improving teacher quality can improve children's educational or cognitive outcomes. Here we briefly review several alternative programs, though direct comparison of impacts, and especially cost-effectiveness, to those of FFE programs is difficult in most cases.

Direct Expense Reduction

Tuition and other school-related expenses, such as uniforms or course material, pose a strong barrier to enrollment in low-income countries. National-level enrollment data in Kenya, Tanzania, and Uganda suggest that abolishing school fees drastically increased enrollment (see Glewwe and Kremer 2006). Kremer, Moulin, and Namunyu (2002) found that free uniforms decreased drop-out rates in Kenya, but providing textbooks did not appear to have an effect. In a randomized experiment also in Kenya, Kremer, Miguel, and Thornton (2007) analyzed the impact of offering scholarship competitions for girls on attendance and test scores. They found that girls who were eligible for the competition had increased attendance and test scores (before receiving the award) and that boys' test scores and teacher attendance also improved in schools offering the scholarships.

Cash-for-Education

Similarly, cash-for-education programs reduce the cost of sending children to school by providing additional household income and offsetting income losses if school replaces work for a child. The PROGRESA program in Mexico provides bi-monthly cash grants to mothers for every child aged between 7 and 18 who is enrolled and regularly attends school. Additionally, households are provided with basic health care services and nutritional supplements for pregnant women and young children (Skoufias 2005). This program was implemented in stages to allow for an experimental evaluation of its impacts. Randomly selected villages participated in the program earlier than others, which served as an experimental control. The program has successfully increased enrollment rates of both boys and girls at the primary and secondary levels (Schultz 2004), decreased drop-out rates and grade repetition, and is associated with earlier school entry (Behrman, Sengupta, and Todd 2005). The health interventions also appear to have reduced morbidity in preschoolers and adults (Gertler 2000) and to have reduced the prevalence of stunting in children younger than 3 (Behrman and Hoddinott 2000).

School-Level Inputs

Tan, Lane, and Lassibille (1999) evaluated the effect of four school interventions implemented randomly in 20 schools in the Philippines in 1990–92 on drop-out rates and student achievement. These interventions include the school feeding program introduced with and without parent–teacher partnerships discussed in Chapter 5. In addition, the authors estimate the impact of the provision of multilevel learning material, consisting of pedagogical material for teachers, which was implemented with and without parent–teacher partnerships. The interventions were randomly assigned to 20 schools, with another 10 schools acting as controls. The authors find that the only significant decline in drop-out rates was due to the multilevel learning materials, both with and without parent–teacher partnerships. They also find that learning materials, combined with parent–teacher partnerships, had a positive and statistically significant impact on students' scores on both English and Filipino tests. Furthermore, the authors note that both the multilevel learning material and the parent–teacher partnerships are much less costly than school feeding.[1] Tan,

[1]On average, school feeding cost P 946 (pesos) per beneficiary, whereas the multilevel learning materials cost an average of P 90 per student and the parent–teacher partnerships cost P 33 per student.

Lane, and Lassibille (1999) conclude that of the four programs implemented, the case for replication is the strongest for multilevel learning material combined with parent–teacher partnerships.

School-level inputs were also found to be effective in Nicaragua (Jamison et al. 1981). Children in classrooms randomly selected to receive radio-based mathematics instruction performed, on average, 1 standard deviation better than children who did not receive this instruction after 1 year of treatment. Children in classrooms that received mathematics workbooks also showed smaller but significant improvements in mathematics test scores. Banerjee et al. (2004) found similar results with computer-assisted mathematics instruction in India. On the other hand, providing flip charts to randomly selected classrooms was found to have no impact on test scores in Kenya (Glewwe et al. 2004).

Other Health and Nutrition Interventions

Basic health interventions can be a cost-effective way to improve the health of school-aged children and thereby reduce illness-related absences from school. Deworming medicine and nutritional supplements are inexpensive to purchase and administer and do not take time away from classroom instruction. Kruger et al. (1996; described above) found deworming treatment improved iron status and led to increased height and weight in children with low-baseline iron stores after 11 months. Because infections are a leading cause of anemia, this result is not surprising. Miguel and Kremer (2004) also found positive effects of deworming on school participation rates but not on test scores. Not only did deworming reduce absenteeism among treated children, but it also improved outcomes for untreated children living nearby, who were also less likely to contract the infections.

Supplementation without providing food is another approach to improving child nutritional outcomes and increasing school participation. Bobonis, Miguel, and Sharma (2004) show evidence of large weight gains and reduced absenteeism from iron supplementation and deworming in 4- to 6-year-olds treated in preschool but not in younger children. In older children, there is also evidence of improved outcomes related to supplementation. A supplementation study in Tanzania showed anthropometric improvements and improvements in iron and vitamin A status among primary-school children consuming fortified beverages (Ash et al. 2003; Latham et al. 2003). And in Cambodia, iron and folic acid supplementation was shown to decrease the prevalence of anemia among 5- to 11-year-olds, but was less effective among 12- to 15-year-olds (Longfils et al. 2005).[2]

[2]Several other studies also evaluate the effectiveness of supplements on micronutrient status among school-aged children. See, for example Latham et al. (2003) and Solon et al. (2003).

Comparing the cost-effectiveness of these interventions with FFE is not possible, because the contexts and scope of the projects vary substantially. However, the Tan, Lane, and Lassibille (1999) study and the studies in Kenya by Kremer, Moulin, and Namunyu (2002); Glewwe and Kremer (2006); and Kremer, Miguel, and Thornton (2007) provide some indications of cost-effectiveness, at least on school participation. For example, Tan, Lane, and Lassibille (1999) argue that learning material coupled with parent partnerships are highly cost-effective at increasing educational attainment compared to FFE. In Kenya, Glewwe and Kremer (2006) argue that school-based health programs, such as deworming, are highly cost-effective in increasing schooling attainment—more so than FFE.[3]

[3]Other interventions not explicitly related to schooling inputs may also improve education outcomes. Glewwe, Jacoby, and King (2001) show that nutritional interventions during early childhood may be highly cost-effective in improving primary-school attainment and academic achievement.

Chapter 8

Conclusion

The economic motivations for investing in the education and nutritional status of primary-school-aged children are well established. Moreover, investments in both of these forms of human capital are likely to benefit from substantial complementarities. However, in developing countries, poor and credit-constrained households routinely invest less in education and nutrition than is privately or socially optimal. FFE programs attempt to improve these investments by subsidizing the cost of school participation through providing food that could improve nutrition and learning.

This study reviewed the empirical literature for impacts of FFE programs on education and nutrition outcomes. Although this literature is vast, high-quality studies with evaluation designs that provide causal impact estimates are relatively few. The nutrition literature offers many more experimental studies on nutrition outcomes than are yet available in the economics literature on education outcomes, yet many of the nutrition studies are controlled trials in which many components of the intervention typically affected by behavior, such as amount of food available at a meal, are closely managed. The external validity of these studies for programs implemented in the field is often difficult to ascertain. The number of experimental field studies for any outcome is few but growing. From the existing literature, it is possible to draw conclusions about the likely impact of FFE programs on some outcomes, whereas for other outcomes the literature is inconclusive. Nearly all the evidence concerns school meals rather than take-home rations.

Among education outcomes, studies show small effects of in-school meal programs on primary-school attendance rates for children already enrolled in school. However, no study provides estimates of the causal impact of these programs on school participation for all school-aged children living in a school's service area. Similarly, there is scant evidence on the impacts of FFE programs on primary-school enrollment rates because of limitations in study design. There is no evidence that FFE programs influence age at primary school entry, primarily because of the cost and difficulty of collecting the data needed for this analysis. The evidence for effects on grade repetition and drop-out rates is inconclusive. For learning achievement, two studies show that school meals cause significant improvements in some test scores. The impact of in-school meals on learning appears to operate both through improvements in school attendance and through better learning efficiency while in school, though no study has separately identified the relative contribution of these effects. FFE programs may also influence cognitive development, though the size and nature of the impacts vary greatly by program, micronutrient content of the food, and the measure of cognitive development used.

Several carefully designed experimental nutrition studies demonstrate considerable effects of school meal programs on nutrition outcomes, including food-energy intake; measures of anthropometry, such as weight, BMI, and height; and micronutrient status. These results indicate that where school-aged children suffer from nutrient gaps, appropriately designed FFE interventions can be effective in closing these gaps and improving nutritional status. FFE programs targeting school-aged children will not reverse most of the previous nutritional insults experienced by these children during early childhood, and the consensus view among nutritionists is that returns to interventions against malnutrition alone are greatest at this early stage of life. Whether the combined impacts of FFE programs on education and nutrition outcomes of school-aged children are sufficient to justify their use in boosting human capital investment in developing countries is largely inhibited by the weakness of the evidence for effects on education and the lack of careful estimates of cost-effectiveness.

The policy decision for whether to undertake an FFE program or an alternative education or nutrition intervention should be based on the internal private and social rate of return on these interventions and their relative differences in cost-effectiveness. Based on the literature on returns to education and nutrition, there is substantial indirect evidence that the impacts of the programs summarized here could lead to large lifetime returns to investments in FFE programs, more than justifying these investments in terms of internal return. However, the key question is whether other interventions would yield even higher returns. On this matter, we have very little evidence. Other than Tan, Lane, and Lassibille (1999), very few studies conduct side-by-side experimental evaluations of alternative programs. Moreover, those studies that measure impact often neglect to collect the additional data needed to obtain a measure of cost-effectiveness. Also, few other programs are likely to have the same kind of combined effects on both education and nutrition outcomes, further complicating the comparisons.

The most immediate policy implication of this review study is that more careful and systematic research is needed to find the most cost-effective combination of programs available. Surprisingly few studies have undertaken the sampling and evaluation design needed to carefully measure the impacts of FFE programs on the school-participation decisions of a representative sample of school-aged children living in the service area of a school. Without knowing the size of this participation effect, it is not possible to determine whether important secondary effects on learning achievement or cognitive development come primarily through school attendance or through the joint effects of schooling and improved nutrition. It is these possible joint effects that are uniquely available through FFE programs. If the learning and cognitive benefits to school-aged children of simultaneous improvements in nutrition and schooling from FFE programs are small, cash-transfer programs linked to schooling, such as the conditional cash transfers now popular in Latin America and elsewhere, may be more effective at increasing school participation. If there are no joint education and nutrition effects from FFE programs, it may be more cost-effective to replace FFE programs with specialized education and nutrition programs that are more narrowly targeted at specific education or nutrition objectives. More comprehensive and rigorous evaluation studies of FFE programs are needed to determine the full scope of the impacts of these programs and their relative cost-effectiveness.

Our interpretation of the empirical evidence reviewed here leads to several recommendations on the design and use of FFE programs. Impacts tend to be larger where schooling participation is low or there are significant nutritional deficiencies. This circumstance argues for doing an assessment of school needs in target areas before starting an FFE program. Such an evaluation would improve targeting and allow FFE program components, such as the nutrient composition and quantity of food, to be tailored to local needs. Also, program administrators should be willing to consider complementary programs to improve school quality. Learning effects cannot be achieved if the instruction is of little value. Poor school quality lowers the benefits of participation and discourages attendance. Though much more evidence is needed, results from field experiments in the Philippines reported in Tan, Lane, and Lassibille (1999) suggests that the cost of alternative programs to improve school quality may be only a fraction of the per child cost of an FFE program. Coordinated programs that combine FFE with improvements in school quality may be much more effective.

Appendix

How an Impact Evaluation Measures Causal Effects of the Program

An impact evaluation is designed to measure changes in household welfare that can be attributed to the program being studied. This measure involves constructing a comparison group that accurately represents what the outcomes of beneficiaries would have been if they had not participated in the program. To be able to claim that measured differences in outcomes between the beneficiary (or treatment) group and the comparison group are caused by the program, it is necessary to control for the effects of other programs, economic shocks, or changing economic trends.

Consider a basic treatment model in which we want to identify the average impact across households (indexed by i) of treatment d on outcome y after controlling for observable household and community characteristic X:

$$y_i = \alpha + \delta d_i + X_i\beta + \varepsilon_i. \quad \text{(A1)}$$

If d is a discrete treatment, estimating this model on a random sample of beneficiary and nonbeneficiary households yields an estimate of program impact:

$$\hat{\delta} = E[y_i \mid X_i, d_i = 1] - E[y_i \mid X_i, d_i = 0]. \quad \text{(A2)}$$

That is, $\hat{\delta}$ estimates the average difference in outcome y between beneficiaries and nonbeneficiaries, conditional on X. However, this difference may capture the effects of other trends besides the program, so it is not possible to conclude that the observed differences $\hat{\delta}$ are caused by the program. The presence of other factors that could be contributing to the observed difference in outcomes can be modeled as an omitted variable captured in the residual ε. If program participation d is correlated with this omitted variable, δ will be a biased estimate of program impact. This problem is referred to as selection bias. The most common sources of selection bias are targeting (or "program placement") bias and self-selection by beneficiaries concerning the decision to participate.

To see how selection bias hinders estimation of causal program impacts, assume treated and untreated individuals have potential outcomes in two states, with and without the program. Let y_1 be the outcome in the treated state and y_0 be the outcome in the untreated state. Let d indicate treatment group status. The average impact of the treatment on the treated (ATT) is

$$\delta^{ATT} = E[y_1 \mid d = 1] - E[y_0 \mid d = 1], \tag{A3}$$

where $E[y_0 \mid d = 1]$ is the unobserved counterfactual outcome if beneficiaries had not received the program. Adding and subtracting $E[y_0 \mid d = 0]$ from equation (A3),

$$\delta^{ATT} = \underbrace{\{E[y_1 \mid d = 1] - E[y_0 \mid d = 0]\}}_{\text{Observed}} - \underbrace{\{E[y_0 \mid d = 1] - E[y_0 \mid d = 0]\}}_{\text{Selection bias}}. \tag{A4}$$

A causal impact estimate is equal to the observed difference in outcomes between beneficiaries and nonbeneficiaries (equivalent to equation (A2)) minus the selection bias. Designing an impact evaluation requires developing a strategy to identify a measure of the counterfactual outcome $E[y_0 \mid d = 1]$.

The preferred method is to design a field experiment by random assignment of the program among comparably eligible communities or households. Those that are randomly selected out of the program form a control group, whereas those selected for the program are the treatment group. Heckman, Ichimura, and Todd (1997) show how random assignment of a program eliminates bias and identifies causal impacts. The intuition is that, because assignment of the program is randomly determined and is not correlated with the outcome variables, differences in outcomes over time between randomly selected treatment and control groups must be a result of the program.

For many large-scale social programs, randomly selecting communities to receive the program is politically infeasible or ethically questionable, unless the program can only be phased in over time because of budgetary constraints. When randomization is not possible, it is necessary to construct a statistical comparison group of communities and households that are sufficiently similar to the treatment group before the program that they serve as a good indication of what the counterfactual outcomes would have been for the treatment group. The design of this comparison group provides the identification strategy that makes it possible to claim that the observed differences in outcomes between the treatment and comparison groups are causal—a direct result of participation in the program.

When random assignment of the program is not possible, an evaluation has to rely on alternative methods to identify program impacts. In some instances, administrative errors or unexpected events interrupt access to the program for a subgroup of eligible households, keeping them from receiving the benefits for some period of time. If this process is quasi-random or is in no way correlated with household characteristics that determine program participation or the target outcomes, then this subgroup would make a viable comparison group. Such approaches are referred to as quasi-experimental impact estimators.

The availability of a quasirandomly selected comparison group for an evaluation is uncommon, so other nonexperimental statistical techniques have been developed to construct a comparison group. The most common methods used include matching methods (including propensity score matching and covariate matching), regression discontinuity design, encouragement design, or instrumental variables.[1] To develop the intuition of how these methods work, we briefly introduce the method called propensity score matching (PSM).[2] Like other matching methods, PSM constructs a statistical comparison group by matching nonbeneficiaries to beneficiaries based on preprogram observable household and community characteristics that are correlated with the probability of receiving the program and with the outcome variables of interest. PSM uses a sample of program beneficiaries and nonbeneficiaries and estimates a model that predicts the probability of each household participating in the program—the propensity score—as a function of these observable characteristics. For each beneficiary, nonbeneficiaries with similar propensity scores represent a viable comparison group, because both groups of households would likely have similar future outcomes in the absence of the program. The impact estimate is constructed as the average difference between the outcome for each beneficiary and a weighted average of outcomes of nonbeneficiaries with similar propensity scores.

In general, impact estimates can be improved by measuring outcomes for treatment and comparison groups before and after the program begins. This makes it possible to construct difference-in-differences (DID) estimates of program impact, defined as the average change in the outcome in the treatment group T minus the average change in the outcome in the comparison group C, defined by:

$$\delta_{\mathrm{DID}}{}^{\mathrm{ATT}} = (y_1{}^T - y_0{}^T) - (y_1{}^C - y_0{}^C). \qquad \text{(A5)}$$

[1]See Blundell and Costa Dias (2000) for a review of nonexperimental evaluation methods.
[2]See Heckman, Ichimura, and Todd (1997), Heckman et al. (1998), and Smith and Todd (2001, 2005) for a presentation of PSM.

The main strength of DID estimates of treatment effects is that they remove the effect of any unobserved variables that represent persistent (time-invariant) differences between the treatment and comparison groups. This elimination helps to control for the fixed component of various contextual differences between treatment and comparison groups, including depth of markets, agroclimatic conditions, and any persistent differences in infrastructure development. As a result, DID estimates can lead to a substantial reduction in selection bias of estimated program impacts.

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