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Designs for Measuring How the School Breakfast Program Affects Learning

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Abstract

This report describes a study design permitting a scientifically defensible evaluation of the impact of the School Breakfast Program (SBP) on learning and cognitive development among children. Following presentation of a literature review and conceptual framework of the SBP-learning relationship, four alternative designs for measuring this relationship are proposed and assessed, including: (1) the design for an experimental evaluation of the impact of the universal-free SBP; (2) an experimental design involving random assignment of classrooms into the SBP among schools applying to participate in the program for the first time; (3) a nonexperimental design involving analysis of Early Childhood Longitudinal Study (ECLS) data; and (4) a nonexperimental design involving analysis of forthcoming National Health and Nutrition Examination Survey (NHANES) data. Among these alternatives, the ECLS-based design (with supplemental analysis of 1988-1994 NHANES III data) was selected as most promising, and the report presents further details concerning this design.

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Executive Summary

This report documents the development of alternative designs to study how participation in the School Breakfast Program (SBP) influences learning. The project, conducted by Mathematica Policy Research, Inc. (MPR) for the Economic Research Service of the U.S. Department of Agriculture (USDA), began with a review of the relevant literature, followed by the development of several alternative designs for studying the relationship between SBP participation and learning. The most feasible alternative was expanded into a detailed study design and analysis plan. The layout of this report mirrors the progression of the project work, from the literature review and description of the alternative designs to a detailed presentation of the most feasible one.

Justification for the Project

A thorough understanding of the nature of the relationship between SBP participation and cognitive performance would provide useful information for school administrators and policymakers. Administrators of schools that do not participate in the SBP could use the information to help decide whether to offer school breakfasts, and policymakers could use it to decide whether and how to promote school participation in the program. Although a separate evaluation of the universal-free school breakfast program (USBP) is underway, information about the effects of the regular SBP on learning might help policymakers determine the appropriate direction for SBP policy.

Previous research shows that eating breakfast improves performance on short-run cognitive tests (see Briefel et al., 1999, in Chapter II, for literature reviews). The research also shows that SBP participation both increases the intake of selected vitamins and minerals and improves school attendance (Devaney and Fraker 1989; and Murphy et al., 1998a and 1998b). Although these findings suggest that eating a school breakfast improves learning, studies that have estimated this relationship directly have failed to produce definitive results. No study has been able to conclude convincingly that eating a school breakfast leads to improvements in long-term or short-term cognition or academic achievement. The inconclusiveness of these studies reflects their limitations, such as the use of small samples, unreliable measures of SBP participation and of academic achievement, or the failure to control adequately for selection effects in the context of nonexperimental designs.

The designs proposed in this report build on the evidence in the literature. They assume an underlying conceptual model in which participation in the SBP directly influences dietary intake and school attendance—intermediate outcomes that ultimately may lead to improved learning. Eating which has the potential to improve their nutritional and health status. Improved nutritional and health status, in turn, are hypothesized to influence three key outcomes: (1) short-term cognition, (2) brain development and functioning, and (3) behavior in school. Health status may influence school attendance as well. All these factors may contribute to learning, the ultimate outcome of interest, which typically is measured by students' grades or performance on standardized academic or achievement tests.

Alternative Designs

MPR proposed four alternative designs that might be used to study the effect of SBP participation on learning: (1) an experimental evaluation of the USBP pilot project (the USBP design), (2) an experimental study of the effects of classroom implementation of the SBP on breakfast program applicant schools (the SBP applicant design), (3) a nonexperimental study of the effects of the SBP based on data from the Early Childhood Longitudinal Study, Kindergarten Cohort (the ECLS-K design), and (4) a nonexperimental study of the effects of the SBP based on data from the forthcoming National Health and Nutrition Examination Survey (the NHANES design).

USBP Design. The first alternative is based on design work that MPR has conducted under contract to USDA's Food and Nutrition Service. This design would use an experimental approach to examine the effects on student outcomes of USBP pilot programs funded by USDA and operating in elementary schools in six school districts. The outcomes would include dietary intake and academic achievement. Under this design, the elementary schools in each district would be matched to one another on the basis of their characteristics and then randomly assigned to either a treatment group that would offer the USBP or to a control group that would continue using the regular SBP. After implementation of the USBP in the treatment schools, samples of students would be drawn from each school. Survey data and administrative data would provide information on students, and outcomes of students in USBP schools would be compared with outcomes of students in control (regular SBP) schools to generate an estimate of the impact of USBP *availability*.

A major strength of this design is its experimental approach, which would generate unbiased estimates of the impact of USBP availability on student outcomes. In addition, USDA has funded this evaluation, which Abt Associates Inc., currently is conducting. The design's weaknesses include its expected high costs ($13.0–$13.3 million, including demonstration costs) and limited statistical power. It would not directly yield experimentally based (and unbiased) estimates of the impact of participating in the USBP or of eating a free school breakfast.

SBP Applicant Design. No previous study of a U.S. breakfast program other than the ongoing USBP study has used an experimental design to estimate the effects of participation on learning/academic achievement. Although a study of this type would be difficult to implement, it is important to fully explore the benefits and feasibility of using an experimental design. One potentially feasible experimental design is the SBP applicant design, which would involve random assignment of elementary school classrooms in schools applying to participate in the SBP for the first time. Rather than serve to permanently approve or deny SBP participation to the applicant schools' treatment and control classrooms, random assignment would be a mechanism to either enable classrooms to begin serving school breakfasts immediately (treatment status) or delay the SBP until a subsequent year (control status). During the first year, treatment classrooms would serve school breakfasts and control classrooms would not. The breakfasts would be served free to all students to encourage schools to participate in the demonstration, and to encourage students to eat school breakfasts. This design would yield experimental and unbiased estimates of the impact of the offer of free school breakfasts (as opposed to no school breakfasts) on student outcomes measured by survey and administrative data.

The SBP applicant design would have the benefits of random assignment and internal validity. However, because it probably would have to be implemented in a limited geographic area (or set of areas), it also would likely have only limited external validity. Also, there are certain aspects associated with the design, such as randomizing classrooms and serving breakfast in the classroom, which could cause problems when actually implemented. In addition, the design would be difficult to implement, and its cost would be high ($10.7–$11.3 million, which includes the costs of the demonstration, data collection, analysis, and report writing). Finally, given the similarity between the SBP applicant design and the USBP design, it is unclear what new information could be derived from this design.

ECLS-K-Based Design. The ECLS-K is a longitudinal dataset with information on a large, nationally representative sample of students from the kindergarten class of 1998. The dataset will ultimately include follow-up information collected through the students' fifth grade year (if they progress at a normal rate in school) and includes a wealth of information on their cognitive development, academic progress, and SBP participation status. This design alternative proposes using ECLS-K data in a nonexperimental examination of the effects of SBP participation on learning. Under this design, measured outcomes in SBP participants and in nonparticipants would be compared, after controlling to the extent possible for observed (and potentially unobserved) characteristics of the two groups.

The strengths of the ECLS-K design are that it would be nationally representative and would be based on large sample sizes of students and schools, giving the design substantial statistical power. The wealth of information in the ECLS-K dataset would allow the design to be based on a range of high-quality measures of student achievement and health status (height, weight, etc.) at various points in time. A final strength of the design is its relatively low estimated cost ($0.4–$0.5 million). A major limitation is that it would be nonexperimental, so that estimates of the impact of SBP participation on learning could be subject to selection bias. Although the design includes plans for addressing the selection bias issue, these methods are imperfect. Another weakness of the design is that it would not include estimates of dietary intake as an intermediate outcome.

NHANES-Based Design. The NHANES provides data on a nationally representative sample of students. The current NHANES data collection effort began in 1999 with a series of continuous, annual surveys. The NHANES-based design would use the current NHANES data to generate nonexperimental estimates of the impact of SBP participation on student outcomes. This design is similar in its basic approach to the ECLS-K design. However, it differs in that it focuses on dietary intake, nutritional status, and health status, all of which are intermediate outcomes that may ultimately be related to learning. Because the current NHANES dataset does not include the various measures of student achievement available in the ECLS-K, it would not be possible to use the NHANES-based design to measure the effect of SBP participation on student achievement unless a major supplemental data collection effort were undertaken. The data collection effort would be costly (contributing $2.9–$3.0 million in addition to the $1.0–$1.1- million analysis and reporting costs) and would involve administration of achievement and cognitive tests to students.

The major strength of the NHANES-based design is its ability to examine outcomes relating to dietary intake, nutritional status, and health status. Much like the ECLS-K design, this design would

produce nationally representative results. Its main weakness is its nonexperimental approach. Finally, because the sample size of the annual NHANES survey is much smaller than that of the ECLS-K, several years of NHANES data would have to be used to generate sufficient statistical power.

Assessing the Alternative Designs

A key factor in the choice of the most promising design is the fact that USDA has implemented the USBP design. The status of the USBP design does not preclude us from recommending that design as most promising one, and, by extension, from suggesting that no other design be considered. However, it does imply that any other design must be recommended in the context of the ongoing study of the USBP. We therefore rule out recommending the SBP applicant design, as it is similar to the USBP design and would yield little new information. Moreover, its implementation would be very difficult and costly. The two other designs are nonexperimental. We believe that the additional cost and number of years it would take to gather an adequate sample prevent us from assessing the NHANES-based design as being the most promising of the two.

We consider a modified version of the ECLS-K design as the most promising alternative. The ECLS-K design addresses at least two limitations of the USBP design, as it (1) would be nationally representative, and (2) would have large samples of students and schools, which would yield substantial statistical power. Furthermore, the rich information on students' achievement and cognitive functioning would enable us to address the key outcomes of interest in the study. The nonexperimental nature of the ECLS-K design is mitigated by the fact that the USBP design also examines the relationship between the breakfast program and learning, thereby providing a second methodological perspective on the issue. Another limitation of the ECLS-K design is its inability to obtain information on intermediate outcomes, such as nutritional status and health status. However, it is possible to correct this weakness by adding a supplemental analysis component to the design. In particular, analysis of the NHANES III dataset, which is similar to the current NHANES data collection effort but was collected during the period from 1988 through 1994 (and includes measures of students' cognition/achievement), would enable the design to examine the impact of SBP participation on both intermediate outcomes and the ultimate outcomes of interest. We refer to this combined set of analyses as the ECLS-K-NHANES III design.

The ECLS-K-NHANES III Design

SBP participation, defined as usually eating a school breakfast provided through a USDA-funded SBP, is the intervention to be examined by the ESLS-K-NHANES III design. The counterfactual aspect as defined in the design, is *not* usually eating a school breakfast. The design calls for estimating the differences in outcomes between students who participate in the SBP and those who do not participate. The study design, which is nonexperimental, would incorporate two analyses. The primary analysis would be based on ECLS-K data; the supplemental analysis would be based on data from NHANES III. The design would be primarily cross-sectional, with learning-related outcomes and SBP participation status measured at a given point in time (although with some longitudinal analysis of ECLS-K data). The design's nonexperimental nature would create the risk of selection bias, but the use of instrumental variables (IV) methods would address this problem.

ECLS-K Data and Measurement Issues. The ECLS-K is important because it is a nationally representative, longitudinal survey that is large enough to support the analysis of the relationship between SBP participation and learning in the overall sample and among key subgroups. The database contains information on three types of outcomes: (1) cognitive functioning, (2) events and processes associated with learning, and (3) other aspects of a child's growth. This variety of outcomes permits exploration of many of the possible effects of SBP participation. Although the information on school breakfast consumption contained in the dataset is not sufficiently precise for accurate measurement of the number of school breakfasts a child eats per week across the full school year, it could be used to create a binary or categorical variable that would measure usual SBP participation. Finally, the ECLS-K includes a rich set of variables that measure the background characteristics of students and schools. These variables are important because they could be used to control for factors that influence SBP participation as well as learning.

In addition to the existing ECLS-K data, the design calls for the use of supplemental data from the parent and administrator components of the ECLS-K survey. Suggested supplemental questions on the parent survey relate to children's breakfast habits; parents' attitudes about breakfast; and children's morning schedules, including their commute to school. Suggested supplemental questions on the school administrator survey would obtain information on the number of eligible and participating children in the SBP and National School Lunch Program (NSLP), by type of payment level (free, reduced-price, full-price); the schools' morning schedules; and the schools' reasons for not participating in the SBP. The supplemental data collection would provide useful descriptive information and could be used in the estimation of instrumental variable (IV) models to account for selection bias.

Analytic Approach. Because the proposed ECLS-K analysis would be nonexperimental, multivariate statistical methods would have to be used to infer the impact of school breakfast participation (or, alternatively, attendance at a school offering the SBP) on the educational outcomes of otherwise identical students. In the most basic set of models, ordinary least squares (OLS), fixed (school)-effects, or random-effects estimation is suggested. However, the strategy for estimating the impact of SBP participation on learning also should account for the possible selection of SBP participants into the sample, conditional on students attending a school that offers the SBP. In addition, it may be necessary to account for the selection of schools into the sample of schools offering the SBP. Selection of either type, if based on unobserved factors correlated with learning outcomes, would bias OLS estimates of the impact of SBP participation on learning.

An IV procedure is a standard method of correcting for selection bias. IV models require the selection of instrumental, or identifying, variables that are uncorrelated with the outcome variable, but that are correlated with the endogenous explanatory variable—in this case, SBP participation status (and, perhaps, SBP availability at a school). Collecting supplemental data from parents and school administrators would increase the number of candidates for instrumental variables in the analysis. The key instrumental variable proposed for this design would be a variable constructed from information collected in both the regular ECLS-K data collection and through the supplemental data collection designed to measure the timing of children's arrival at school, the start of the school day, and the children's mode of transportation to school.

An alternative estimation strategy is based on propensity score modeling. Under this approach, probabilities of SBP participation by students in SBP schools would be estimated, making it possible to create subgroups of likely SBP participants in SBP schools and likely SBP participants in non-SBP schools. Such a *propensity score-based* strategy could help in estimating the impact of *offering* the SBP to students, as well as the impact of SBP *participation* on learning.

Each proposed estimation method has limitations. A simple comparison of reported differences in outcomes between SBP participants and SBP nonparticipants would not account for observed differences between the two groups of students. OLS, fixed (school)-effects, and random-effects estimates would account for observed differences between students and their schools, but not for the endogenous selection of students into the SBP (and, possibly, for selection into schools offering the SBP). IV estimates might help correct for sample selection bias, but the precision of the resulting impact estimates is likely to depend on the predictive power of the instrumental variables used in the analysis. Propensity score methods could account for any association between attendance at an SBP school and different outcomes for students with different probabilities of SBP participation. However, the ability of propensity score methods to identify the actual impact of SBP participation on learning outcomes depends on whether attendance at an SBP school is independent of unobserved factors influencing learning.

Supplemental NHANES III Analysis. Adding NHANES III data analysis related to SBP participation and learning to the ECLS-K data would enhance what could be inferred from the ECLS-K alone about the relationship between eating a school breakfast and learning. The NHANES III is a nationally representative database that contains information collected from 1988 to 1994 on school-aged children's school breakfast participation, dietary intakes, nutritional status, and health status (intermediate outcomes in our design), and on their performance on standardized cognitive tests. The survey also includes information on explanatory, or control, variables such as family characteristics; on school outcomes, such as attendance and grade level; and on behavioral outcomes.

The NHANES III data provide a unique opportunity to fully examine the relationships between SBP participation and such intermediate outcomes as nutrition and health status; this examination could not be performed with the ECLS-K dataset. In addition, NHANES III offers a cross-sectional examination of the relationship between school breakfast participation and two key indicators of learning—cognitive functioning and academic performance—in a national sample of participating and nonparticipating children. Although the ECLS-K and other national surveys also measure much of the same family and school data, NHANES III includes detailed information on diet, nutrition, and health pertaining to school-aged children.

The supplemental NHANES III analysis would provide additional evidence on the impact of SBP participation on learning and may corroborate (or may fail to corroborate) the primary findings from the ECLS-K. However, although the NHANES III includes a wealth of information pertinent to the research question of interest, the total sample size is smaller than that of the ECLS-K, and the dataset is not longitudinal. The cross-sectional nature of the NHANES III dataset (and the resulting NHANES III analysis) means that key outcomes are measured at a given point in time in the school year. This feature of the NHANES III dataset would limit the extent of an analysis of academic achievement, as it does not measure year-to-year change. It also would reduce the statistical power

of NHANES-based impact estimates of SBP participation on learning. Nevertheless, the additional evidence from the NHANES III on the research question still would be valuable because the issues of selection bias and statistical power are important to the ECLS-K-based design.

To estimate the effects of SBP participation on learning, regression analysis would be used to compare cognitive outcomes of participants and nonparticipants, after controlling for relevant, measurable factors. In comparing academic and learning outcomes in participants and nonparticipants, the design calls for models that account for such important factors as food insufficiency and poor nutritional status, as well as factors that relate to prenatal nutrition, such as low birth weight and exposure to cigarette smoke. Environmental exposures, such as to lead, revealed by elevated blood lead levels, should also be considered in interpreting the results of cognitive tests and academic performance. Thus, the set of controls available in the NHANES III data will be richer in some ways than those available in ECLS-K. Unlike the ECLS-K, however, the NHANES III dataset includes few school-level control variables (or indicators of which schools sample members attend). Thus, this analysis would not measure or control for some of the institutional factors associated with the schools that students attend.

Conclusion

A complete understanding of the relationship between eating a school breakfast and learning is very important to policymakers and school administrators who must make decisions about the direction of the SBP. The designs described in this report, particularly the ECLS-K-NHANES III design, could be used to perform analyses that would significantly add to the existing research on the impacts of SBP participation on outcomes related to learning. This design also has the advantage of maximizing the utility of existing, ongoing, national surveys of U.S. schoolchildren through supplemental ECLS-K data collection and analysis as well as analysis of NHANES III data. Finally, the ECLS-K-NHANES III design would complement the current USBP evaluation, which uses an experimental approach to study a similar set of issues.

I. Introduction

The School Breakfast Program (SBP) administered by the United States Department of Agriculture (USDA) provides breakfasts in school to more than 7 million students each school day. The SBP began as a pilot program in 1966 and was permanently authorized by Congress in 1975. It has grown steadily over time, and the number of participating schools has nearly doubled during the past 12 years. The SBP has always served mainly low-income children, with about 84 percent of current program participants receiving free or reduced-price meals. As the program has expanded, however, it has become available to increasingly large numbers of higher income children. Between 1997–1998 and 1998–1999, higher income children—that is, those not certified for free or reduced-price meals—accounted for two-thirds of the *growth* in SBP participation (Food Research and Action Center, 1999).

Many believe that eating breakfast helps children do better in school. In fact, research shows that eating breakfast improves performance on short-run cognitive tests (see Chapter II; and Briefel at al., 1999). Previous research also has found that SBP participation increases the intake of selected vitamins and minerals, and that it improves school attendance (Devaney and Fraker, 1989; and Murphy et al., 1998a and 1998b). These findings suggest that eating a school breakfast may have a positive influence on student learning; however, the results of studies that have directly estimated this relationship have been inconclusive.

Understanding the true nature of the relationship between SBP participation and cognitive performance is an issue of concern to policymakers, as shown in a Senate hearing on Child Nutrition Authorization (U.S. Senate, 1998). An understanding of this relationship would be useful for school administrators and policymakers for two reasons. First, although school participation in the program has been growing, with about 70 percent of schools that serve lunches currently serving breakfasts, many schools still do not offer the SBP.

A better understanding of the effects of the program would help school administrators in nonparticipating schools decide whether to offer breakfasts. This information would also help policymakers decide whether and how to promote school participation in the program. Second, policymakers currently are debating the merits of a universal-free school breakfast program (USBP). Although a separate evaluation of the USBP is underway, additional information about the effects of the regular SBP on student learning might help policymakers determine the appropriate direction for USBP policy.

This report is a compilation of the work completed for the Economic Research Service (ERS), USDA, by Mathematica Policy Research, Inc. (MPR) for the project Design an Evaluation of the Impact of the School Breakfast Program on Learning. The purpose of this report is to review the relevant literature on the connection between SBP participation and learning, describe four alternative designs for studying this relationship, and outline one of the four designs in detail. The design outlined in detail represents the design we believe to be the most feasible and the between SBP participation and learning, describe four alternative designs for studying this relationship, and outline one of the four designs in detail. The design outlined in detail represents

the design we believe to be the most feasible and the one best able to measure the true relationship between school breakfast participation and learning.

The remainder of the report is organized as follows. Chapter II reviews the existing literature on the relationship between nutrition and learning. It is a revised and updated version of the literature review memorandum submitted to ERS in March 2000. Chapter III describes and assesses the four alternative designs; it is based on the Alternative Designs Report submitted in December 2000. Finally, Chapter IV outlines the design selected as the best of the four alternatives; it is a revised version of the New Study Design Report submitted in January 2001. one best able to measure the true relationship between school breakfast participation and learning.

II. Literature Review

This chapter summarizes the relevant literature on the relationship between nutrition and learning and draws implications from the literature for the Design and Feasibility Study of the Impact of the School Breakfast Program on Learning. The literature suggests a relationship between eating breakfast, improved dietary status, and enhanced cognitive performance. Although the literature is suggestive of positive educational benefits, no study has been able to definitively conclude that eating a *school breakfast* results in improvements in long-term or short-term cognition or learning or in academic achievement. The inconclusive findings reflect limitations of the studies, such as the use of unreliable measures of breakfast participation and academic achievement, or the use of nonexperimental designs that do not adequately control for selection effects.

The first section of the chapter reviews three areas of previous research that are useful for the SBP Design Study: (1) the link between nutrition and child development; (2) the contribution of breakfast to children's behavioral and cognitive development; and (3) the relationship between SBP participation and children's dietary status, academic achievement, and other school-related outcomes. Another review of the literature on breakfast and learning provides summaries of key references (Briefel et al., 1999). The second section presents a conceptual model to describe how SBP participation would be expected to influence students' learning. In addition, it draws implications from both the previous research and the conceptual model for use in developing alternative designs to estimate the relationship between the SBP and learning.

A. The Literature

1. The link between nutrition and cognitive development

A large body of research documents the effects of inadequate nutrient intakes on children in developing countries, and a more limited group of studies focuses on the effects of dietary inadequacies on residents of developed countries, such as the United States. The literature includes experimental studies of the effects of nutritional deprivation on the cognitive functioning of animals, correlational studies linking hunger or undernutrition and the developmental outcomes of children, and experimental studies of the effects of nutritional supplementation programs on children. These studies provide solid evidence of a link between nutrition and cognitive development, although the extent to which undernutrition must be severe (rather than moderate or mild) to substantially affect development is unclear.

Animal studies provided some evidence of a link between nutritional deprivation and cognitive performance measures (Morley and Lucas, 1997). Similarly, a study of children in developing countries showed that permanent structural damage to the human brain can be attributed to the extremely detrimental effects of severe undernutrition in early childhood, a period in which the brain develops rapidly (Brown and Pollitt, 1996). Although this level of undernutrition is extremely rare in the United States (Pollitt, 1988), important aspects of cognitive development may occur after periods of rapid brain growth early in life, so that negative effects on cognitive development

potentially could result even from moderate or mild degrees of undernutrition. Both of these factors point to the potential importance of nutrition intervention throughout childhood and suggest that programs like the SBP could play a major role in the cognitive development of some children.

Much of the research on the relationship between nutrition and human development involves analysis of the effects of nutritional supplementation on cognitive or behavioral development (Gorman, 1995). Examining this research for evidence of the links between nutrition (or nutrition supplementation) and cognitive development helps us understand the degree to which an intervention such as the SBP has promise for promoting children's cognitive development. The remainder of this section discusses this area of research, focusing in particular on three different strands: (1) the effects of protein-energy supplementation interventions, (2) the effects of iron-repletion therapy on iron-anemic children, and (3) the relationship between hunger and cognitive/psychosocial outcomes. Throughout this review of the nutrition-cognitive development literature, it is important to distinguish between studies conducted in developed countries, such as the United States, and those conducted in developing nations, in which severe malnutrition is much more common.

a. The effects of protein-energy supplementation on cognitive and behavioral development

Most of these intervention studies were conducted in populations of mildly to moderately malnourished children in developing countries (for example, Colombia, Guatemala, Indonesia, Jamaica, and Taiwan). Gorman (1995) reviewed intervention studies that examined the effects of supplements provided during critical early periods of brain development (gestation and infancy); the evidence from these studies suggests that protein-energy supplementation is associated with improved motor and/or cognitive development. Two studies of this type examined the role of individual socioeconomic differences on the strength of the observed impacts; they yielded conflicting results (Gorman, 1995). In a nutrition supplementation trial in Guatemala, children living in families with low socioeconomic status (SES) were more likely than children in higher SES families to respond to protein-energy supplementation (Freeman et al., 1980). Waber et al. (1981) obtained the opposite result in Colombia, where children from families with better resources derived relatively greater benefit from supplementation. Rush et al. (1980) conducted a supplementation study in New York City similar to those two studies.

The researchers found that, with the exception of a visual habituation task, the supplementation of pregnant women had no effect on any of the behavioral measures examined in the women's children. Some researchers attribute this dearth of significant effects to the lack of evidence that the women who participated in the trial were nutritionally at risk (Pollitt, 1988). Unfortunately, few other studies of the effects of protein-energy supplementation during early periods of brain development have been conducted in developed nations, precluding investigators from definitively stating that such supplementation interventions lead to improved motor/cognitive development only in developing countries in which severe undernutrition exists.

Studies on the effects of protein-energy supplementation generally have been well designed; they have used experimental or quasi-experimental methodologies that provide internally valid estimates. However, for three reasons, the applicability of these intervention studies to the current SBP design study is limited: (1) they were generally based on small sample sizes (well under 500 subjects in

most cases); (2) they involved the provision of a single food or nutrient supplement, rather than a wider range of foods such as are provided in a more general nutrition program like the SBP; and (3) they focused largely on undernourished children in developing countries.

b. The effects of iron-repletion therapy and iron deficiencies on cognitive and behavioral development

A large body of literature exists on the effects of iron-repletion therapy on cognitive and behavioral outcomes in iron-deficient and/or anemic children. In a review of the literature on the developmental impact of nutrition, Pollitt (1988) stated that the majority of studies conducted in developing countries and in the United States during the 1980s provide evidence that iron-deficiency anemia negatively affects cognitive functioning. Furthermore, experimental studies conducted in developed countries have found that, when compared with anemic children given a placebo, anemic children treated with iron-repletion therapy exhibit statistically significant improvements in motor and mental development scores (Pollitt, 1988).

An Indonesian study based on a clinical trial research design provided similar results, but the effects of the iron-repletion therapy generally were smaller than those in most U.S. studies. In particular, the motor and mental development scores of iron-treated anemic children remained significantly below those of iron-replete (nonanemic) children (Pollitt, 1988). The apparent inability of iron treatment to increase the motor development of iron-deficient children to the level of their non-anemic peers, despite its success in normalizing hemoglobin levels, also was found in a study conducted in Costa Rica (Lozoff et al., 1991). Pollitt (1988) suggested that differences between the impact of iron-repletion therapy in developed and developing countries may be attributable to other nutritional deficiencies in children from poor countries that may limit the ability of iron interventions to produce positive effects.

In a recent review of studies of the effect of iron deficiency on cognitive development, Grantham-McGregor and Ani (2001) note that most are non-experimental, longitudinal studies and conclude that making causal inferences about the iron deficiency-cognitive development relationship from longitudinal studies is difficult due to the possible confounding effect of poor socioeconomic status and environmental factors. They also report that studies conducted on children older than two years of age are somewhat more conclusive (in finding that iron deficiency interferes with cognitive development) than those for younger children, though they indicate that there is still a need for more rigorous, randomized controlled trials, particularly amongst children younger than two years of age (Grantham-McGregor and Ani, 2001).

One longitudinal, observational study conducted in the U.S. looked at a sample of over 3,700 10 year-old Dade County students who had participated in the WIC program before the age of 5 (Hurtado et al., 1999). The authors found that after controlling for confounding factors (birthweight, maternal education, sex, race/ethnicity, age at time of study, and age at entry to WIC), a child's risk of placement in special education classes increased by 1.28 for each decrement of iron level (Hb) at the time of entry to WIC. The study also found that students who were anemic at the time of entry to WIC were more likely to be disabled, black, low birthweight, and have mothers with less than a high school education. The findings from the Hurtado et al. (1999) study reinforce the fact that there is a connection between socioeconomic status and anemia.

Although the findings from these well-designed studies of the impact of iron repletion therapy on development are interesting, they have limited applicability to the current study for the same reasons that apply to the protein-energy supplementation studies. Similarly, many of the studies reviewed by Grantham-McGregor and Ani (2001) suffer from the same limitations, and also tend to have nonexperimental designs. However, the evidence of positive impacts of iron-repletion therapy suggests that school breakfasts containing levels of iron sufficient to help participating, iron-deficient children reach the RDA may help improve the cognitive and academic outcomes of those children.

c. The effects of hunger on cognition and psychosocial outcomes

Despite the high degree of interest in the impact of hunger on the cognitive and psychosocial outcomes of American children, few well-designed studies of this relationship have been conducted. One major research initiative examining this relationship is the Community Childhood Hunger Identification Project (CCHIP). CCHIP researchers have examined the link between hunger and cognitive and psychosocial outcomes in low-income children in Baltimore, Philadelphia, and Pittsburgh (Murphy et al., 1998a; and Kleinman et al., 1998).

Murphy et al. (1998a) found hunger status to be associated with children's behavior as measured by total scores on the Pediatric Symptom Checklist (PSC), which is used as a psychosocial screen, and on the Child Behavior Checklist, which identifies psychosocial symptoms. In particular, the authors found that, relative to other children, hungry children had significantly higher levels of hyperactivity and were more likely to be absent from school. Kleinman et al. (1998) reported that 25 percent of hungry children repeated a grade, compared with 19 percent of children at risk of hunger and with 12 percent of children who were not hungry. In addition, hungry children were three times more likely than at-risk children and seven times more likely than children who were not hungry to have PSC scores indicating dysfunction.

A number of limitations qualify the interpretation of the findings of these two correlational studies and are relevant for the SBP design study. First, the fact that parents were offered the choice to accept or decline an invitation for their children to participate in the studies raises the possibility of sample selection bias. Sample selection bias would make the findings less generalizable to the low-income population as a whole. Second, the studies' use of cross-sectional data limits the ability to prove a causal relationship between hunger and psychosocial outcomes. Third, parents provided subjective evaluations of the degree of hunger their children experienced. If other problems influenced the responses, the parents may have reported hunger in their children with error. Finally, the studies did not control for variables to account for other challenges low-income children face, such as violence or inadequate family support networks. The absence of control variables increases the likelihood that the differences between hungry and nonhungry children arose from factors other than hunger.

2. The link between breakfast and cognition

Another relevant group of studies comprises those that examined the effect of eating breakfast on cognitive outcomes. Although the literature includes studies of various groups (children, adolescents, young adults, the elderly), this review will discuss only the ones that focused on elementary school children, as this group is the one most relevant to the SBP design study. In general, research on the breakfast-cognition relationship in children shows that eating breakfast

(as opposed to fasting) is associated with improvements in some short-term cognitive functions, specifically, cognitive tasks involving memory (Pollitt and Mathews, 1998).The effects tend to be largest in nutritionally at-risk children on mornings that follow an overnight fast.

The most common methodology used in these studies was a randomized, blinded, crossover design in which researchers took the following steps: (1) randomly assigned children to a treatment group that received breakfast on the morning of the experiment after an overnight fast, or to a control group that received no breakfast on that morning, (2) blindly administered cognitive tests to the children several hours after breakfast, (3) switched treatment and control groups on a subsequent day, and (4) readministered cognitive tests to the children.

The estimated effects of breakfast eating on the cognitive development of middle-class, well-nourished children are mixed, with some studies finding a positive relationship (in some or all children) and others finding no relationship. Pollitt et al. (1981) found that the effects of eating breakfast differed significantly with the children's intelligence quotient (IQ); breakfast was positively associated with performance on a test of visual matching (the Matching Familiar Figures Test, or MFFT) among children with IQs below the group median but was not associated with the performance of children with IQs above the median. However, when Pollitt et al. (1983) replicated this study using a different sample, they found that eating breakfast was positively related to visual matching (as measured by the MFFT) among all children, regardless of IQ.

In addition, Conners and Blouin (1982/1983) found that breakfast was positively related to performance on a test of visual stimuli (the Continuous Performance Task) and on an arithmetic test among well-nourished 9- to 11-year-old children. Conversely, Cromer et al. (1990) found no significant differences between children who ate breakfast and those who did not in any of the cognitive measures examined. The results of these studies, although not definitive, suggest that breakfast eating leads to improvements in certain tasks of short-term cognition among well-nourished, middle-class youngsters.

The literature on studies conducted in *developing* countries provides one unambiguous finding—the effects of breakfast on cognition are moderated to some extent by nutritional status. Simeon and Grantham-McGregor (1989), Chandler et al. (1995), and Pollitt et al. (1998) each conducted a randomized, crossover study examining the effect of eating breakfast on the cognitive functions of low SES children with varying nutritional statuses. The results of the studies consistently showed that, compared with skipping breakfast, eating breakfast positively influenced the performance of undernourished children on a variety of cognitive tests (for example, the MFFT, verbal fluency tests, the Stimulus Discrimination Test, and the Sternberg Memory Search Test). In each study, neither eating breakfast nor skipping breakfast influenced the cognitive test performance of adequately nourished children.

Lopez et al. (1993) examined the same relationships as were examined in those three studies, but among children in Chile and without using a crossover design. In contrast to the findings from the three studies, this study found that breakfast eating was not significantly associated with cognitive performance among either adequately nourished children or undernourished children. A number of factors may explain these results.

First, the researchers did not conduct the study in a hospital or metabolic ward, so they had no control over the timing or composition of dinner the night before the experiment. Second, almost one-fourth of the subjects had eaten breakfast at home on the day of the experiment and consequently were assigned to the breakfast condition. This course of action indicates that true randomization did not occur (Pollitt and Mathews, 1998). Finally, many of the children were using a computer for the first time, and it is possible that high levels of motivation caused them to perform better than they normally would have (Lopez et al., 1993).

One study analyzed the effect of breakfast timing on the cognitive functions of students (Vaisman et al. 1996). In that study, students who ate breakfast at school, which was served half an hour before testing, had significantly higher test scores than did both those who ate breakfast at home two hours before school and those who did not eat breakfast. Another study, by Michaud et al. (1991), focused on the effects of breakfast size on short-term memory and concentration. In that study, energy intake at breakfast was not related to children's scores on a concentration test.

3. The effects of the School Breakfast Program

As described below, research has examined the effects of SBP participation on students' dietary intake and found that the program improved the intake of a number of different nutrients, particularly by low-income children. In this section we review studies that have examined the effects of breakfast program participation on school-related outcomes, such as student achievement, cognition, attendance, and psychosocial measures. They generally found that participation is linked with higher attendance, though the findings of these students about the effects of participation on students' cognition and academic achievement were inconclusive. Moreover, the studies suffer from various methodological limitations, so the question of how breakfast program participation influences children's learning in school remains unanswered.

a. Program effects on breakfast eating and dietary intake

A key research issue is whether the availability of the SBP in a school increases the likelihood that students will eat breakfast.[1] Early evidence suggested that the availability of the SBP does not affect whether students eat breakfast (Devaney and Fraker, 1989; and Gleason, 1995).[2] To define breakfast eating, however, both studies used either students' self-reports of breakfast eating or the intake of a relatively small minimum amount of food energy (50 calories). Devaney and Stuart (1998) replicated the two studies' findings, but, when they defined breakfast as consisting of a minimum of 10 percent of the Recommended Energy Allowance (REA), they found that the availability of the SBP increased the percentage of low-income children eating breakfast.

[1] This question arises not only by the presumption that breakfast eating will improve students' performance, but by research showing that breakfast eating positively affects 24-hour intake (Devaney and Fraker, 1989; Morgan et al., 1986; and Nicklas et al., 1998).

[2] The findings of Nicklas et al. (1993) are an exception. These researchers found that, after the SBP was introduced in Bogalusa, Louisiana, the percentage of 10-year-old students who skipped breakfast (that is, consumed no calories) declined.

Early research on the effects of SBP participation on students' dietary intake found mixed effects of the program. Using data on students in public schools during the 1980-1981 school year, Devaney and Fraker (1989) and Wellisch et al. (1983) found SBP participation to positively affect the intake of calcium and magnesium, and to negatively affect the intake of vitamin A, iron, and cholesterol. Devaney et al. (1987) used the same data and found that SBP participants were more likely than nonparticipants to drink milk, but less likely to consume eggs and ready-to-eat cereals, potentially explaining some of the effects of participation on the intake of vitamins and minerals.

Between the 1980-1981 school year and the 1991-1992 school year, when data from the first School Nutrition Dietary Assessment (SNDA-1) study were collected, the SBP not only grew but also had different effects on the dietary intake of students. Using SNDA-1 data, Gordon and McKinney (1995) found no difference in SBP participants' and nonparticipants' intakes of eggs or ready-to-eat cereals.

However, participants consumed more milk, cheese, meat, grains, and fruit juice than did nonparticipants. Devaney et al. (1993) and Gordon et al. (1995) found that SBP participation positively and significantly affected students' 24-hour intakes of food energy, protein, thiamin, calcium, phosphorus, and magnesium. They found no significant effect of participation on fat, saturated fat, sodium, or cholesterol intake. Using a small sample of preschool children, Worobey and Worobey (1999a) found that SBP participation led to a decreased intake of refined sugar. In a similar study by Worobey and Worobey (1999b), SBP participation led to a decrease in fat intake among a small sample of preschooler children.

The research on the dietary effects of SBP participation has been well designed and based on large, nationally representative samples.[3] However, the studies share two features that limit their applicability to the current design study. First, they estimated the effects of SBP participation prior to changes in federal regulations governing the school meal programs that may have influenced the composition and effects of the SBP.

Second, the studies were nonexperimental and therefore subject to selection bias, so unobserved differences between participants and nonparticipants may be driving their results. All the studies attempted to control for observable differences between the groups, and Devaney et al. (1993), Devaney and Fraker (1989), and Gordon et al. (1995) used econometric techniques to control for unobserved differences between them. However, these techniques have weaknesses, and the degree to which selection bias affected the studies' estimates is not known.

b. Program effects on school-related outcomes findings

Two clear findings relevant for the current study emerge from this literature, which is summarized in Table II.1. Breakfast program participation is positively related to students' attendance (Abell Foundation, 1998; Cook et al., 1996; Jacoby et al., 1996; Meyers et al., 1989;

[3]The two studies by Worobey and Worobey (1999a and 1999b) are exceptions, as they were based on extremely small and geographically limited samples of preschool children.

Murphy et al., 1998b and 1999; and Powell et al., 1998) and is negatively related to their tardiness (Abell Foundation, 1998; Cook et al., 1996; Meyers et al., 1989; and Murphy et al., 1998b and 1999).

The size of these effects is moderate; however, some of the studies either failed to conduct significance tests or had relatively small samples and did not find the effects to be statistically significant. Nevertheless, given that the findings are common to many different studies, are based on different samples, and use different methodologies, the findings that breakfast program participation leads to higher attendance and leads to less tardiness are credible (Briefel et al., 1999). The most recent major review of this literature, by Pollitt and Matthews (1998), reached the same conclusion.

Previous studies obtained differing estimated effects of SBP participation on academic achievement. Meyers et al. (1989) found the largest effects, with participation in the regular SBP estimated to lead to a significant increase of 10 percent of a standard deviation in a child's battery score on the Comprehensive Test of Basic Skills (CTBS). However, even this study failed to find statistically significant effects of SBP participation on the subtests that comprise the CTBS. Participation was estimated to have positive, but not significant, effects on language and math subtest scores and essentially no effect on the reading subtest score.

Table II.1—Summary of studies of the effect of SBP/USBP participation on school outcomes

Study	School outcomes examined	Design	Type of breakfast program	Sample	Findings (estimated effects of breakfast program)	Status
Abell Foundation (1998)	Attendance; tardiness; disciplinary incidents	Nonexperimental	U.S. USBP	3 treatment and 3 control schools in Baltimore, MD (school-level analysis)	Positive significant effect on attendance; negative significant effect on tardiness	Not published
Chandler et al. (1995)	Cognitive functioning	Experimental	Jamaican SBP	197 primary school students in 4 schools	Positive significant effect on verbal fluency; no significant effect on three other cognitive tests	Published
Cook et al. (1996)	Attendance; tardiness	Nonexperimental	U.S. USBP	USBP participants and nonparticipants in Central Falls, RI	Positive significant effect on attendance Negative significant effect on tardiness	Not published
Jacoby et al. (1996)	Attendance; achievement test scores; cognitive functioning	Experimental	Peruvian SBP	352 fourth and fifth graders in 10 Peruvian schools	Positive significant effect on attendance Positive significant effect on vocabulary and math tests among heavy children but insignificant effects overall Insignificant effects on cognitive functioning	Published
Meyers et al. (1989)	Attendance; tardiness; achievement test scores	Nonexperimental	U.S. SBP	1,023 third through sixth graders in 6 schools in Lawrence, MA	Positive significant effect on attendance Negative significant effect on tardiness Positive significant effect on CTBS test battery Positive insignificant effect on language and math test scores No effect on reading test scores	Published
Murphy et al. (1998a and 1998b)	Attendance; tardiness; math grades; psychosocial outcomes	Nonexperimental	U.S. USBP	133 students from 3 schools (1 in Philadelphia, PA; 2 in Baltimore)	Positive significant effect on attendance Negative significant effect on tardiness Positive significant effect on math grades Positive significant effect on psychosocial outcomes	Published
Murphy et al. (1999)	Attendance; tardiness; disciplinary measures; nurse visits; psychosocial outcomes	Nonexperimental	U.S. USBP	6 USBP and 6 regular SBP schools in Maryland (school-level analysis); 91 students (student-level analysis)	No significant effect on attendance or tardiness No significant effect on disciplinary measures Negative significant effect on nurse visits Positive significant effect on psychosocial outcomes	Not published

Study	School outcomes examined	Design	Type of breakfast program	Sample	Findings (estimated effects of breakfast program)	Status
Powell et al. (1983)	Attendance; achievement test scores; physical growth	Nonexperimental	Jamaican SBP	114 (12- and 13-year-old) students in 3 classes in a single Jamaican school	Positive significant effect on attendance Positive significant effect on arithmetic test scores No effect on reading and spelling test scores No effect on physical growth	Published
Powell et al. (1998)	Attendance; achievement test scores; physical growth	Experimental	Jamaican SBP	407 second through fifth graders in 16 rural Jamaican schools	Positive significant effect on attendance Insignificant overall effects on test scores Positive significant effect on arithmetic test scores in younger students	Published
Wahlstrom and Begalle (1999)	Achievement test scores; nurse visits	Nonexperimental	U.S. USBP	6 treatment and 4 control schools in Minnesota	Inconclusive results with respect to test scores Negative effect on nurse visits (significance not tested)	Published
Test of Basic Skills; SBP = n; USBP = universal-free l.	Cognitive functioning	Nonexperimental	U.S. SBP	12 preschool children in New Jersey	Positive significant effects on fine motor skills and visual perception/discrimination	Published

The other studies of the effects of breakfast program participation on academic achievement were based on foreign programs.[4] Powell et al. (1983) found that participation in a school breakfast program in Jamaica was positively and significantly related to arithmetic test scores but failed to find significant effects on spelling and reading test scores. In a later study by Powell et al. (1998), participation in the Jamaican program was not significantly related to test scores overall, although it was positively and significantly related to test scores in younger children.

Noriega et al. (2000) found that participation in a school breakfast program in Mexico had no significant effects on scores from a variety of attention, memory, and cognition tests. Finally, Jacoby et al. (1996) found that participation in a breakfast program in Peru was not significantly related to achievement overall; however, the researchers did find positive effects on vocabulary scores among a subset of heavier children whom they hypothesized were undernourished.

Three studies focused on the effects of breakfast program participation on short-term cognitive outcomes. Like the studies on academic achievement, these studies obtained mixed findings. Jacoby et al. (1996) found that participation in the Peruvian breakfast program was insignificantly related to students' performance on a coding test (the only short-term cognitive outcome they examined). Chandler et al. (1995) found that participation in the Jamaican breakfast program had positive and significant effects on a verbal fluency test, but that it was insignificantly related to performance on three other short-term cognitive tests.

The authors noted that their finding of a positive effect on verbal fluency was consistent with findings from another Jamaican study, by Simeon and Grantham-McGregor (1989), which examined the effects of eating breakfast versus no breakfast on cognitive outcomes. The authors also hypothesized that the effects of participation were limited to verbal fluency because verbal fluency was the only one of the four cognitive outcomes examined that involved "initiating and maintaining a mental process in the absence of any externally based organization," rather than relied on students' reactions to external stimuli (Chandler et al., 1995).

The only U.S. study to examine the effect of SBP participation on cognitive outcomes was a nonexperimental one based on a very small sample of preschool children in New Jersey (Worobey and Worobey, 1999b). In that study, a group of preschool students participating in a breakfast program very much like the SBP improved their test performance more than did a group of control students who continued to eat breakfast at home during the same period. The results were strongest for computer-based tasks related to visual perception, classification, and discrimination.

A few studies examined the effects of breakfast program participation on other outcomes. Murphy et al. (1999) and Wahlstrom and Begalle (1999) found that enrollment in a USBP school was

[4] Two other studies of universal-free school breakfast programs (USBPs) in the United States are relevant. Although Murphy et al. (1998b) did not examine test scores, they found that USBP participation was positively and significantly related to students' math grades in two large eastern cities. Wahlstrom and Begalle (1999) presented data on mean test scores from the periods preceding and subsequent to USBP implementation in several USBP schools and comparison schools in Minnesota. However, they did not conduct significance tests, and they made no claims about the implications of their data with respect to the effects of the USBP on academic achievement.

associated with decreases in the number of nurse visits and improvements in teachers' and parents' perceptions of the learning environment in school. (These relationships were not necessarily statistically significant.) Murphy et al. (1998b) and Murphy et al. (1999) found that USBP participation was significantly associated with children's psychosocial outcomes. The researchers argued that program participation led to lower levels of anxiety, hyperactivity, childhood depression, and psychosocial dysfunction.

Methodological issues

Because of limitations of the research, most of the conclusions from the literature on the effects of breakfast program participation on students' school-related outcomes remain inconclusive. These limitations are cited briefly in the following list and then are explored in greater depth in this section.

- ***Limited attention to any one outcome.*** A relatively small number of studies examined the effects of participation on any given outcome. (The exception was studies that focused on attendance/tardiness as an outcome).

- ***Differences in breakfast program interventions.*** Previous studies examined the effects of different types of breakfast programs that serve different populations of students. The comparability of these different methods of estimating effects is uncertain.

- ***Nonnational representativeness.*** The studies were not nationally representative and so have low external validity.

- ***Nonexperimental Designs.*** Most of the studies used nonexperimental designs subject to selection bias, and thus, may have low internal validity.

- ***Small sample sizes.*** Many of the studies analyzed relatively small samples of students and of schools.

- ***Significance tests ignoring the number of schools in the sample.*** The studies typically conducted significance tests that did not adequately account for the small sample size of schools.

Small number of studies devoted to any given outcome. Relative to the number of studies that examined the effects of eating breakfast on behavioral and cognitive development or the general link between nutrition and cognitive development, a relatively small number of studies have examined the effects of participation in school breakfast programs. Although most of the studies estimated the effects of participation on attendance and tardiness, relatively few of them estimated the effects of participation on any other outcome. For example, only four studies (Jacoby et al., 1996; Meyers et al., 1989; Powell et al., 1983; and Powell et al., 1998) examined the influence of participation on achievement test scores, and only three (Chandler et al., 1995; Jacoby et al., 1996; Worobey and Worobey, 1999) focused on short-term congnitive outcomes. Similarly, two studies (Murphy et al., 1998a, 1999) examined students' psychosocial outcomes, and two others focused on students' visits to the school nurse (Murphy et al., 1999; Wahlstrom

and Begalle, 1999). Given the other methodological limitations of these studies, their small number makes it difficult to draw definitive conclusions from them.

Differences in breakfast program interventions. The studies of breakfast programs estimated the effects of different types of breakfast programs serving very different types of populations. Chandler et al. (1995), Jacoby et al. (1996), Noriega et al. (2000), and Powell et al. (1983 and 1998) estimated the effects of foreign breakfast programs, which serve meals to relatively poor and undernourished children. Although the studies provide useful information, one cannot necessarily make inferences about the effects of participation in the SBP on the basis of the estimated effects of school breakfast programs in Jamaica or Peru.

The studies that examined U.S. breakfast programs also varied somewhat in the programs examined. Meyers et al. (1989) estimated the effects of participation in the regular SBP, whereas the remaining studies focused on some type of USBP. It is not clear whether the effects of participating in a USBP would be the same as the effects of participating in the regular SBP, but none of these studies investigated potential differences in this effect.

Nonnational representativeness. The studies of the effects of breakfast program participation analyzed samples drawn from limited populations. Meyers et al. (1989) examined the effects of SBP participation in a single city (Lawrence, MA) in schools where children tended to be poor. This study's findings may not translate to different parts of the United States or to wealthier populations of students. The USBP studies typically examined programs operating in a single city or in multiple sites within a single state. None of the studies examining the effects of breakfast program participation on school-related outcomes were based on a population that is nationally representative (or even representative of a single state).

Nonexperimental designs. Three of the five studies of foreign breakfast programs (by Chandler et al., 1995; Jacoby et al., 1996; and Powell et al., 1998) used an experimental design. Classrooms of students were randomly assigned to receive either a full school breakfast or a placebo breakfast typically consisting of an orange slice. Because of the experimental design, program participants and nonparticipants should not have differed systematically before the program started. Thus, any subsequent differences in outcomes between the two groups could be attributed to either the effects of the program or to random chance. These studies have high levels of internal validity.

In contrast, the studies of the U.S. breakfast program used a nonexperimental design (that is, individual students [or schools] decided whether they would or would not participate in the program). Thus, the researchers were unable to guarantee that program participants were similar to nonparticipants. The use of a nonexperimental design required the researchers to control for relevant preexisting differences between the two groups when measuring differences in outcomes between the groups. The studies controlled for preexisting differences in a variety of ways, although most used a pre-post design for both treatment group and control group members. Each of the studies is subject to the criticism that their findings were driven more by preexisting participant-nonparticipant differences than by effects of the breakfast program. The internal validity of these studies is lower than that of the experimental studies.

Small sample sizes. Finally, even if the studies had controlled properly for all relevant preexisting differences between participants and nonparticipants (or had used an experimental design), it would have been necessary to statistically test for whether the resulting differences in outcome measures between the two groups were due to the effects of the program or to chance. In general, the larger th*e samples in the studies, the smaller the likelihood that an estimated effect of a given size was due to chance.

If the results of these studies are to be generalizable beyond the specific sites (and the specific points in time) in which the programs were examined, both the size of the sample of schools being studied and the size of the sample of students being studied are relevant. However, most of the studies used relatively small samples. For example, Murphy et al. (1998b) analyzed a sample of 133 students in three schools, Powell et al. (1983) analyzed a sample of 115 students in a single school, Worobey and Worobey (1999b) analyzed fewer than 20 students in a single preschool, and the Abell Foundation (1998) analyzed schoolwide data from only three USBP schools and three non-USBP schools. Even in studies that had large samples of students, the students came from relatively few schools. For example, Meyers et al. (1989) analyzed a sample of more than 1,000 students, but these students came from only six schools within a single school district.

Significance tests that ignore the sample size of schools. In principle, tests of statistical significance enable researchers to determine the extent to which they can be confident that their estimates reflect the true effects of participation, rather than random chance. These significance tests take into account the sample sizes of students [numbers of students]. In practice, however, the significance tests used in the studies of school breakfast programs appear to have taken advantage of the assumption that the observations of students' outcomes and characteristics were statistically independent of one another.[5]

This assumption might be reasonable for a sample of students drawn from a single school (although the results of the analysis of the sample would not be generalizable beyond that school). However, most of the studies drew their samples from more than one school. Therefore, one would expect the outcomes among students attending the same school to be related to one another, due to schoolwide characteristics that affected all the school's students. For example, one school may have had a particularly strong curriculum, so that all the sample members drawn from that school may have had relatively positive outcome values. Given the relatively small samples of schools in these studies, properly taking into account this correlation across different sample members within the same school would likely have led to lower significance levels.[6]

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[5]The studies did not present sufficiently detailed descriptions of their methodologies to enable us determine whether this assumption was maintained throughout their analysis. However, the assumption is a common one in significance testing, and most of the studies did not mention having relaxed it. Furthermore, the studies achieved levels of statistical significance that would have been unlikely had they developed this assumption of independence.

[6]To give a sense of the size of the effect, consider the following example. Suppose that a study randomly assigned the students of the three schools to receive school breakfasts, with the students of three schools that did not receive school breakfasts serving as the control group. Suppose further that 20 students were sampled randomly from each of the 6 schools, for a total sample size of 120 (including 60 SBP participants and 60 nonparticipants). For an outcome measure with an overall variance of 1, an estimate of the

This limitation is particularly relevant for studies in which breakfast participation status was determined at the school (or classroom) level. For example, Powell et al. (1983) studied the effects of participation in a trial in which students of one classroom were given a school breakfast and those of the two other classrooms were not. Outcomes were then measured in all 115 students in the three classrooms, and the effects of participation were determined by comparing outcomes in students in the breakfast classroom with outcomes in students in the non-breakfast classrooms. However, certain factors that have large effects on these outcomes are likely to vary only at the classroom level.

The quality of the breakfast classroom teacher may have been much different than that of the other teachers, and the effect of this factor on students' attendance, cognition, and achievement could have been strong enough to overwhelm any effects of the breakfast. If there were many breakfast classrooms, the average quality of teachers in the breakfast and non-breakfast classrooms would likely have been similar. With a small number of classrooms (or schools), however, factors such as this one probably differ for participating and nonparticipating students. It is important to account for this fact when conducting significance tests and assessing results.

B. Implications of Research

1. Conceptual framework

The purpose of this design study is to develop and assess alternative ways of testing the hypothesis that participation in the School Breakfast Program leads to improved learning by children. The research summarized in the preceding section does not definitively establish the existence of this relationship, but some of the studies do support the belief that various intermediate relationships may underlie the overall SBP-learning relationship. For example, research suggests that the availability of the SBP in a school promotes breakfast eating among students at the school (Devaney and Stuart, 1998). Other studies show a link between breakfast eating and short-term cognition (Pollitt and Matthews, 1998). If short-term cognition leads to improved learning, then these two findings could be components of a relationship between the availability of the SBP in a school and students' learning and academic achievement in that school.

Although research suggests some reasons for believing that SBP participation may promote learning, it is useful to clarify these reasons, as fully specifying the potential linkages through which participation could conceivably influence learning is helpful for designing empirical studies testing the relationship between participation and learning. This section describes a conceptual framework that presents the pathways through which participation could influence learning.

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effect of participation on the outcome would have a standard error of 0.31, if the sample size of schools were adequately considered. (We assumed the school-level variance in the outcome measure accounted for 10 percent of the overall variance.) If one were to ignore the sample size of the schools (and the sample was assumed to be randomly drawn), the standard error of the estimate of the participation effect would be 0.17. If the estimated effect were 0.34, it would be statistically significant ($t = 2.0$) under the statistical test that ignored the sample size of schools, but would be far from significant ($t = 1.1$) under the statistical test that took into account that sample.

School breakfast participation can affect school performance directly or through intermediate factors, such as improved dietary intake, nutritional status, or health. The underlying rationale is as follows:

- Some children do not eat breakfast or do not eat a nutritious breakfast.

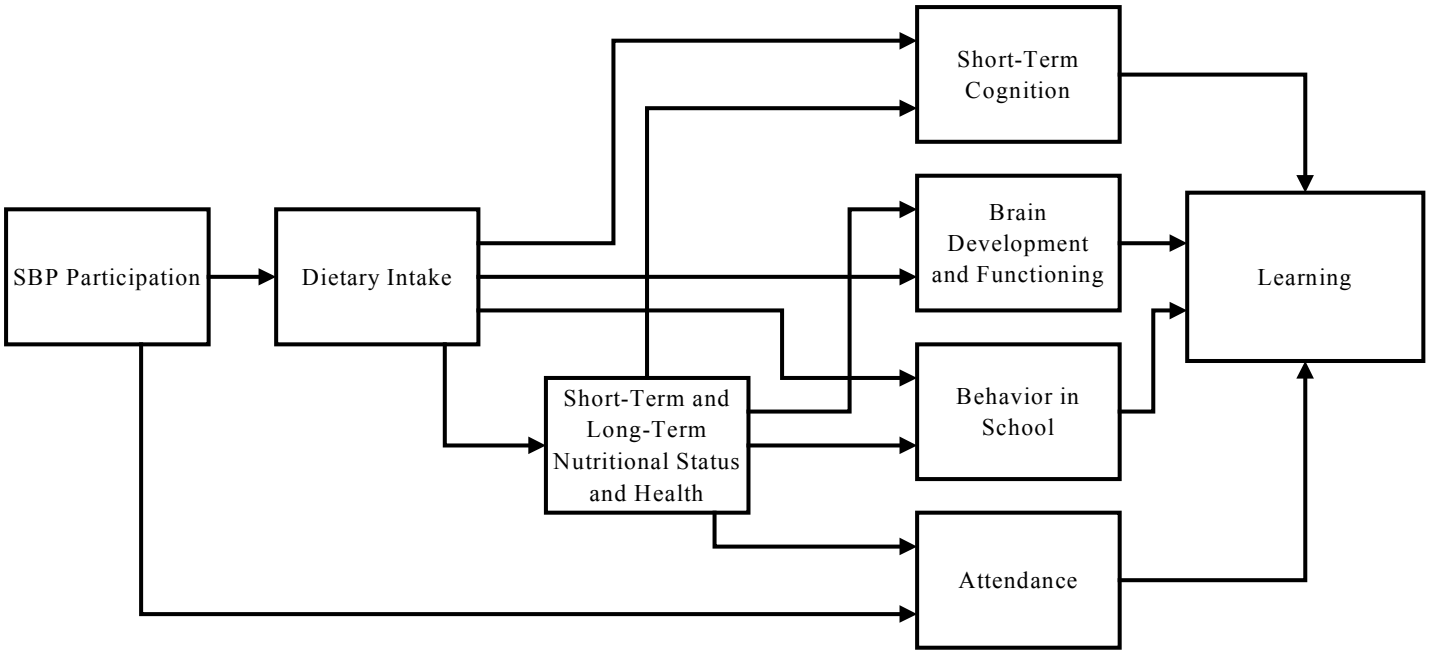
- Providing school breakfasts will increase the consumption of breakfasts, thereby increasing the number of students who eat a nutritious breakfast.

- Eating a nutritious breakfast leads to enhanced readiness to learn, improved cognitive and behavioral outcomes, improved dietary status, and, ultimately, higher academic achievement and school performance.

Figure II.1 summarizes the conceptual framework for analyzing the effects of SBP participation on learning. According to the framework, breakfast program participation directly influences dietary intake and school attendance, intermediate outcomes that may ultimately lead to improved learning.

By eating a school breakfast, students' dietary intakes (both at breakfast and over 24 hours) may be improved. This improved dietary intake, in turn, potentially leads to improvements in students' nutritional and health status. Together, improved dietary intake and health status are hypothesized to influence three key outcomes: (1) short-term cognition, (2) brain development and functioning, and (3) behavior in school. In addition, health status may influence students' school attendance.

Figure II.1—Conceptual framework for analyzing effects of SBP participation on learning



The conceptual framework serves two main purposes for this design study. First, it specifies the intermediate outcomes that SBP participation potentially influences. A good design should incorporate as many of these intermediate outcomes as is feasible. If the study were to fail to show a relationship between SBP participation and learning, for example, then knowing whether participation influences the intermediate outcomes would aid in the interpretation of the results and would suggest avenues for future research.

Second, it provides a basis for the specification of the model or models to be estimated. In particular, the control variables to be included in the model should include the factors that directly influence student learning or that influence any of the intermediate outcomes (which, in turn, may influence learning).

The framework also may help identify factors that modify the effect of SBP participation. For example, if one of the primary ways in which participation influences learning is through its effect on dietary intake, then one might expect the effect to be larger for children whose nutrient intakes at home are most likely to be inadequate (that is, low-income children).

If the study showed that participation leads to improvements in students' short-term cognition, but not to improvements in learning or academic achievement, future work would focus on examining competing explanations for the absence of an overall effect. For example, certain types of short-term cognition may not strongly influence students' learning. Alternatively, improvements in short-term cognition may influence learning only in the long term, suggesting a study with longer-term follow-up measures of students' SBP participation and learning. Identifying the key intermediate variables and incorporating them into the design would also be

important if the study were to show a positive and significant effect of participation on learning. Table II.2 describes the potential relationships between a variety of intermediate outcomes and learning. For example, such a study would be able to provide information about whether the overall effect occurred because students benefited more from their time in class (that is, their short-term cognition improved), because they were spending more time in class (that is, their attendance improved), or because of some other reason or combination of reasons.

Short-term cognition refers to the ability to recognize, absorb, and process information. In school, having highly developed short-term cognition enables students to benefit more from their classes (particularly during the late morning), which improves their base for future learning. Dietary intake potentially influences short-term cognition is two ways. First, researchers have hypothesized and empirical studies have found that higher breakfast calorie intake levels lead to higher glucose levels available to the brain and, therefore, to higher energy levels in the late morning (Benton and Parker, 1998). These higher energy levels improve students' late-morning cognitive functioning. Second, better dietary intake leads to improvements in health status, which prevents such factors as headaches, upset stomachs, or other symptoms from interfering with learning. Improved health status also decreases the number of days missed due to illness.

Regularly participating in a school breakfast program may also influence learning and academic achievement through a mechanism unrelated to dietary intake. Students may want to participate in the breakfast program because they (1) prefer the food at school to the food at home, (2) enjoy spending time with their friends during breakfast, or (3) appreciate the attention of breakfast program staff. For any of these reasons, SBP participation may lead to more regular school attendance and, therefore, to more time spent in class. Students who increase the time they spend in class (and who pay attention during that additional classroom time) have the potential to learn more.

Table II.2—Implications of conceptual model with respect to intermediate outcomes

IntermediateOutcome	How Does SBP Participation Influence Outcome?	How Does Outcome Influence Learning?
Dietary intake	SBP participation has a direct positive effect on dietary intake.	Improvements in dietary intake lead to improvements in short-term cognition, brain development, and behavior in school (both directly and through the positive effect dietary intake has on health status); these outcomes in turn lead to improved learning. Improvements in dietary intake also positively influence school attendance through improvements in health status; greater attendance then leads to improved learning.
School attendance	SBP participation has a direct positive effect on dietary intake. Participation also positively influences attendance indirectly by improving dietary intake, which leads to	Improved school attendance has a direct positive effect on learning.

	improved health status, which leads to improved school attendance.	
Health status	SBP participation first leads to improvements in dietary intake, which lead in turn to improved health status.	Improved health status positively influences short-term cognition, brain development, behavior in school, and school attendance; improvements in these four outcomes then lead to improved learning.
Short-term cognition	SBP participation first leads to an improvement in dietary intake, which in the short term leads to an increase in blood glucose levels and in the long term leads to an improvement in health status. Both the increase in short-term blood glucose levels and in health status can positively influence short-term cognition.	Improved short-term cognition has a direct positive effect on learning.
Brain development	SBP participation leads first leads to an improvement in dietary intake, which leads to an improvement in health status. Both dietary intake and health status positively influence brain development.	Improved brain development has a direct positive effect on learning.
Behavior in school	SBP participation leads first leads to an improvement in dietary intake, which leads to an improvement in health status. Both dietary intake and health status positively influence behavior in school.	Improved behavior in school has a direct positive effect on learning

Initially, scientists viewed the main link between dietary intake and cognition as primarily the result of the effects of malnutrition on brain development. As described by Brown and Pollitt (1996), scientists had hypothesized that underfeeding in childhood led to "permanent, structural damage to the brain." Although the effects of malnutrition in early childhood on brain development no longer are believed to be necessarily permanent or the only means through which nutrition affects learning, they still are thought to be a potentially important means by which dietary intake influences children's intellectual development. Furthermore, other aspects of children's health status that also are affected by dietary intake may reduce or magnify this effect.

Obviously, the most important question to be addressed in the study being designed in this project is whether SBP participation leads to increased learning by children. However, the conceptual framework presented in Figure II.1 and the previous studies outlined in Table II.1 suggest several additional research issues and questions for this design study:

- Does SBP participation directly influence dietary intake and attendance—the primary outcomes through which it could affect learning? If there is an overall effect of participation on learning, does it arise primarily though the effect of participation on dietary intake or through the effect on attendance?

- Which of the factors that could directly influence learning (short-term cognition, behavior, and/or attendance) does SBP participation directly or indirectly influence? Are any of these intermediate factors more important than the others in explaining the relationship between participation and learning?

- What role does students' health status play in the model? Is health status an important means through which participation is linked with the other outcomes of interest?

- If no overall relationship between participation and learning is found, at which point does the conceptual model break down? Which link in the model is least likely to hold (or least likely to be statistically detected even if it does hold)?

Designing a study that can answer at least some of these questions will greatly increase the usefulness of the policy implications of the study results for policymakers.

2. Design alternatives

The research literature summarized in Section A has established four important research findings. First, a link between nutrition and cognitive development exists and has been shown to be strongest and most well established in severely undernourished children in developing countries. Second, eating breakfast leads to improvements in children's short-term cognition, especially in children who are generally undernourished. Third, children who usually eat school breakfasts are more likely than nonparticipants to eat a substantial breakfast and generally have greater intakes of selected nutrients. Fourth, breakfast program participation leads to greater attendance in school and to lower levels of tardiness. However, we do not know whether participation influences health status, short-term cognition, or learning and academic achievement, as limitations of studies that investigated these issues leave the findings inconclusive. Thus, it is desirable to design a study that will both reassess these questions and add to the evidence from some of the better-established research findings.

A study on the influence of SBP participation on such outcomes as learning, cognition, and health status should attempt to address the weaknesses of the previous research. It should also be designed in a way that takes into account the conceptual framework described in this chapter, and that addresses some of the issues arising from the framework. In particular, the study design should try to incorporate as many of the following features as possible:

- ***Examine multiple outcomes.*** The study should examine as many of the intermediate outcomes as possible, including short-term cognition, behavior and attendance in school, health status, and dietary intake. If the approach suggested by the conceptual framework were used, appropriately measuring many of these outcomes would be quite challenging and potentially costly. However, that approach would increase the value of the research to the literature as a whole.

- ***Examine the U.S. Breakfast Program.*** Because the SBP is the policy of interest, the study should examine this program either in its standard form or as part of the USBP that currently is being implemented in a number of sites across the country. If the study examines the USBP, it should include a sample of students attending schools that offer the regular SBP and should compare the estimated effects of participation in the USBP and regular SBP.

- ***Collect data from a nationally representative sample.*** Ideally, the study would be able to produce estimates that could be generalized to the United States as a whole (that is, the study

would have external validity). To produce these estimates, the study would have to use a nationally representative sample.

- ***Use an experimental design.*** To produce estimates that have the greatest internal validity, the study should use an experimental design. Under this design, either students or schools would be randomly assigned to a treatment group that participates in the breakfast program or to a control group that does not participate. The differences between the groups would be random, so any systematic differences arising after participation in the program could be attributed to the program.

- ***Use large samples of students and schools.*** To generate enough statistical power to detect relevant participant-nonparticipant differences in key outcomes, the study should collect data from a large number of students attending a relatively large number of schools. Although the appropriate number of students and schools has yet to be determined, the sample sizes used in many of the previous studies of the impact of breakfast programs were too small to achieve the aims of this design study.

- ***Conduct statistical tests that take into account the sample size of students and the sample size of schools.*** If the significance tests conducted as part of the study ignore the fact that the sample of students is collected from a limited number of schools, then they are likely to understate the variability of the estimates, and to overstate their statistical significance. The study should take into account this design feature so that valid estimates and conclusions are produced.

There are many tradeoffs among these study features and between the features and study costs. For example, a study using an experimental design would be challenging to implement nationally, so that it would be difficult to use a nationally representative sample. It also would not be possible to collect data on a nationally representative sample of USBP participants and nonparticipants, as that program is not available nationally.

There also are tradeoffs between the size of the samples analyzed, the number of outcomes measured, and the cost of the study. It may be feasible to collect data on a limited number of easily measured outcomes from a large sample of students, but it probably is not feasible to collect data from a large sample on a wide range of outcomes, including such difficult-to-measure outcomes as short-term cognition.

Given the design features the study is attempting to incorporate, the following four design alternatives:

1. ***USBP study.*** Under contract with the Food and Nutrition Service, Mathematica Policy Research, Inc. designed an evaluation of six USBP pilot programs. The design included a three-year study of elementary schools participating in the USBP and focused on the effects of the program on a variety of outcomes, including dietary intake and academic achievement. Abt Associates, Inc. currently is conducting this evaluation.

2. *SBP applicant design*. No study of a U.S. breakfast program has used an experimental design to estimate the effects of participation on learning/academic achievement. Such a study would be difficult to implement, but it is important to fully explore the design's benefits and feasibility. One experimental design involves random assignment at either the classroom or school level and includes only schools applying to participate in the SBP for the first time. Rather than approving or denying SBP participation to treatment and control schools or classrooms, random assignment could be a mechanism for either beginning the SBP in one school year or delaying it until a subsequent year for participating schools/classrooms. This design would have the benefits of random assignment and internal validity; however, it would likely have to be implemented in a limited geographic area (or set of areas) and thus would not necessarily have great external validity.

3. *ECLS-K-based study*. The Early Childhood Longitudinal Survey, Kindergarten Cohort, (ECLS-K) is a longitudinal dataset collecting information on a large, nationally representative sample of students from the kindergarten class of 1998. The dataset includes follow-up information collected through the students' fifth grade year (if they progress at a normal rate in school) and includes a wealth of information on the students' cognitive development and academic progress. (It also contains information on SBP participation.) A study using ECLS-K data to examine the effects of SBP participation on learning would incorporate many of the key features listed in this section. It would be nationally representative and would include large sample sizes. If supplementary data were collected on sample members' dietary intakes, it would be possible to examine a large number of outcomes. This design alternative would be unable to incorporate one feature—an experimental design.

4. ***NHANES-based study*.** The National Health and Nutrition Examination Survey (NHANES) provides data on a nationally representative sample of students. This design would be nonexperimental in nature and offers the benefit of being able to examine outcomes relating to dietary intake, nutritional status, and health status. However, the survey currently collects limited information on learning outcomes, school attendance, social and emotional development, or school characteristics. A study using NHANES data to examine the effects of SBP participation on learning would incorporate many of the key features listed in this section.

We develop each of these design options more fully in Chapter III. The conceptual framework and the literature provide the basis for a discussion of the advantages and disadvantages of each of the four options. The designs' structures were refined on the basis of comments from the Economic Research Service and from a panel of experts on design-related issues and on the SBP. (See Appendix A for a list of the experts as well as a summary of the May 2000 expert panel meeting.)

III. Alternative Designs

This chapter outlines four possible designs for obtaining rigorous estimates of the impact of participation in the School Breakfast Program (SBP) on key student outcomes related to learning:

1. The planned experimental evaluation by the U.S. Department of Agriculture (USDA) of the Universal-Free School Breakfast Program pilot projects (USBP design)

2. An experimental study of the effects of classroom implementation of the SBP among breakfast program applicant schools (SBP applicant design)

3. A nonexperimental study of the effects of the SBP based on data from the Early Childhood Longitudinal Study, Kindergarten Cohort (ECLS-K design)

4. A nonexperimental study of the effects of the SBP based on data from the forthcoming National Health and Nutrition Examination Survey (NHANES design)

First, the details of the methodological approach used by each of the four designs are described. Second, the strengths and weaknesses of the designs are assessed and summarized in terms of feasibility and methodological rigor. Third, a single design is selected as the most feasible of the four alternatives.

In Section A, we describe the key elements of the alternative designs. The subsequent four sections (Sections B through E) outline the four alternative design approaches by providing details about the key design elements of each one. Section F summarizes the strengths and weaknesses of each alternative and makes a case for the design we selected as the best of the four. (We describe the selected design in greater detail in the final chapter of this report.)

As described in Section F, we conclude that the ECLS-K design, supplemented with analysis based on the 1988-1994 NHANES III dataset, is the most feasible. We arrived at this conclusion after taking into consideration the fact that the USBP design has been planned and is being implemented. The strengths of the ECLS-K design include its large, nationally representative sample and longitudinal design with carefully developed measures of academic achievement. Although the ECLS-K provides no information on outcomes related to dietary intake or nutritional status, the supplemental analysis conducted with NHANES III data should help address this limitation.

A. Key Elements of the Alternative Designs

As we have discussed in Chapter II, the limitations of the existing studies of the effects of the SBP on learning reduce the usefulness of their results. It is therefore important that the alternative designs considered here address at least some of these limitations. In particular, the designs should do as many of the following as possible:

- Examine multiple outcomes
- Examine the U.S. School Breakfast Program

- Collect and/or use data from a nationally representative sample
- Use an experimental design
- Use large samples of students and schools

There are many tradeoffs both among these desired study features and between the features and study costs. For example, a study using an experimental design would be difficult to implement nationally, thereby making it difficult to use a nationally representative sample. Thus, it would not be possible to collect data on a nationally representative set of USBP participants and nonparticipants, because the USBP is not available nationally. There also are obvious tradeoffs between the size of the samples analyzed, the number of outcomes measured, and the cost of the study. Although it may be feasible to collect data on a limited number of easily measured outcomes from a large sample, it may not be feasible to collect data from a large sample on a wide range of outcomes that include difficult-to-measure ones like dietary intake and short-term cognition.

To define each of the alternative designs fully, we must describe design elements that address such issues as the research objective, the general approach to be used, and the details of how this approach will be implemented. The importance of the following elements will be considered in the discussion of the alternative designs:

1. ***Intervention and counterfactual.*** It is important to clearly define the *intervention* being examined as well as to explicitly state the *counterfactual*. Together, the intervention and counterfactual define the hypothesis that the study will test.

2. ***Basic design approach.*** It is necessary to determine whether the study will use an experimental or nonexperimental design. In an experimental design, the choice of the unit of random assignment is important. In a nonexperimental design, it is important to identify differences between the two groups other than the main outcome in order to limit selection bias. It is also necessary to select the population of interest and a corresponding sampling frame as well as to determine whether the study will use a cross-sectional or longitudinal design. [7]

3. ***Data collection.*** The design should specify whether the study is to be based on primary data or secondary data (or some combination of the two). Study designs involving primary data collection require many more decisions than do those using secondary data.

4. ***Measurement of key characteristics and outcomes.*** To measure SBP participation, the design first must specify whether the intention is to capture SBP participation on a single day or over a longer period. To measure students' learning, the design must specify what type of test will be used, and how the test will be implemented.

[7]The sample design should also specify planned sample sizes and levels of statistical precision. These issues are discussed in Sections B.6, C.6, D.6, and E.6.

5. ***Analysis plans.*** The major analysis issue to be considered by experimental designs is whether substantial *dilution* of the effect of SBP participation on learning is inherent in the design. The major analysis issue that must be confronted by nonexperimental design alternatives is selection bias.

6. ***Statistical power.*** When evaluating a design, it is highly important to determine the degree to which the design has the statistical power to detect effects of a size likely to be produced by the intervention. The statistical power of a design approach typically is specified in terms of its minimum detectable difference (MDD), the smallest *true* intervention effect that would likely lead the evaluation to conclude statistically that the intervention had an effect.

7. ***Design costs***. The cost of carrying out a design is an important factor in assessing its promise. A design's cost can be influenced by each of the six design features described in this list. The cost also may include expenses related to establishing and operating a demonstration program (for designs based on a demonstration).

B. The USBP Design

The William F. Goodling Child Nutrition Act of 1998 (P.L. 105-336) authorized the Secretary of Agriculture, through the Food and Nutrition Service (FNS) of the USDA, to conduct a demonstration and evaluation that will rigorously assess the effects of the USBP on program participation and on a broad range of student outcomes, including academic achievement. Six school food authorities (SFAs) were selected to participate in the demonstration in spring 2000. FNS contracted with Mathematica Policy Research, Inc. (MPR) to develop a comprehensive study design for evaluating the USBP pilot programs. This section summarizes the design plan that MPR produced (Ponza et al. 1999). The plan proposed an experimental approach to randomly assign elementary schools within the six participating SFA's into a treatment group that will use the USBP and a control group that will use the regular SBP.[8]

1. Intervention and counterfactual

The primary intervention in the USBP design is having access to the USBP; the counterfactual is having access only to the regular SBP. In other words, students in the treatment group attend elementary schools over the course of a school year that offer the USBP, whereas control group students attend elementary schools that offer only the regular SBP. In practice, this intervention has two main implications for students. First, all treatment group students can obtain a free school breakfast, but only low-income control group students who have been certified for free meals can do so. Second, because breakfasts are served free to all students in USBP schools, cafeteria workers or other school personnel do not have to distinguish between certified and noncertified students, thus potentially leading to less stigma among SBP participants. Both implications are expected to lead to higher rates of participation in the USBP by treatment group students than in the regular SBP by control group students.

[8]FNS awarded the contract to conduct the USBP evaluation to Abt Associates Inc. The design as implemented may differ somewhat from the design described in this section.

In addition to these implications of attending a USBP school, the implementation of the USBP itself may lead to other changes in the breakfast program available to students at treatment group schools. For example, because the implementation of the USBP is expected to lead to greater program participation, schools may decide to alter the ways in which breakfasts are prepared or served. For instance, schools with USBPs sometimes have opted to serve breakfasts in classrooms as part of the school day, rather than in the cafeteria before the start of classes. A difference in breakfast delivery could influence student outcomes.

It is important to emphasize that the intervention in the USBP design is not participation in the USBP relative to a counterfactual of nonparticipation. Both treatment group students at USBP schools and control group students at regular SBP schools may or may not eat a school breakfast. Thus, the estimated impact arising from differences between treatment group and control group members cannot be used directly to determine the effects of breakfast program participation. Instead, the treatment-control difference simply provides an estimate of the impact of *attending* a school in which the USBP rather than the regular SBP is available. However, attending a USBP school rather than a regular SBP school (that is, being in the treatment group rather than in the control group) is expected to be positively correlated with eating a school breakfast. It is therefore possible to use treatment-control differences to obtain indirect estimates of the effects of participation.

For a direct estimate of the effects of breakfast program participation on learning, a secondary analysis will treat participation as the intervention. Within USBP schools, for example, this secondary intervention will be defined as usually eating a school breakfast, relative to the counterfactual of usually being a breakfast program nonparticipant. Breakfast program nonparticipants may or may not have eaten any breakfast, and these two possibilities will be distinguished. As described in the following section, the basic design approach for examining the effects of the secondary intervention differs from the experimental design used to examine the primary intervention—the effects of USBP availability.

2. Basic design approach

The USBP design uses an experimental approach to examine the effects of USBP availability on student outcomes. The unit of random assignment for the evaluation is the elementary school. Some or all of the elementary schools in each of the six participating SFA's are matched on the basis of their characteristics. One school in each matched pair is then randomly assigned to a treatment group that participates in the USBP or a control group that continues using the regular SBP. After the USBP is implemented in the treatment schools, samples of students are drawn from each school.

a. Random assignment

In this design, random assignment is conducted at the school level rather than the student level, primarily because the USBP typically is implemented at the school level (or at least at the classroom level). Implementing the USBP at the student level would make it possible to randomly assign students, but it would also result in the loss of two of the benefits of the USBP: (1) reduction of administrative burden on schools, and (2) reduction of stigma borne by students. In addition, attempting to randomly assign students into USBP and regular SBP groups would raise practical concerns that would make successful implementation unlikely.

One of the drawbacks of randomly assigning schools rather than students is that random differences between schools and their experiences over time will make statistical detection of program effects more difficult. This loss of precision arises because sample members in a particular group are influenced by shared events or circumstances. For example, if one treatment school has a particularly strong teaching staff, the student outcomes among all students in that school would be positively influenced relative to students in control schools. If students, rather than schools, were randomly assigned, some treatment group and some control group students would be affected by the teaching staff of that school.

To address the loss of precision from using schools as the unit of analysis, the USBP design suggests using as large a sample of schools as the funding constraints of the evaluation will allow. Random circumstances or events in treatment and control group schools will be more likely to cancel each other out if there are more schools in the sample. In addition, the design attempts to minimize random variation across treatment and control schools by matching schools prior to random assignment.

In particular, schools that have similar observable characteristics (for example, size, racial composition, average test scores, or income levels), and that local administrators consider to be similar in quality, will be matched. Each pair of matched schools is then randomly assigned, with one school becoming a treatment school and the other a control school.

The USBP design report discusses two alternative approaches that vary according to the number of schools sampled (Ponza et al., 1999). The preferred approach proposes that 144 schools be randomly assigned, 72 to a treatment group and 72 to a control group. The schools would need to be selected from the six SFAs participating in the demonstration.

b. Student sampling

The USBP design calls for two partially overlapping samples: (1) a cross-sectional sample of students in grades 1 through 6 as of the 2000-2001 school year; and (2) a longitudinal cohort of students in study schools, used to gather data on changes in student outcomes as measured by school records. The students in the cross-sectional sample are to be surveyed once, during spring 2001. That survey also will collect dietary recall data and information about the students' experiences in school and their attitudes toward school; the students also will be given cognition tests. In addition, their parents will be surveyed in spring 2001.

The second sample (the longitudinal cohort of students in study schools) includes students in grades 2 through 6 in school year 2000-2001. Most of these students will have been in grades 1 through 5 during the previous school year (the baseline year) and could be followed through the 3 years of the demonstration period; they will not be followed beyond grade 6, however. Clearly, some of the students (those in grade 1) in the cross-sectional sample will not be included in the longitudinal sample. In addition, for operational reasons, there will be longitudinal sample members who will not be in the cross-sectional sample. Otherwise, the two samples will overlap.

Administrative school records data will be the main form of information collected from students in the longitudinal sample. Ultimately, the aim is to collect school records data for each student from the baseline year and the 3 followup years. Of particular importance will be test scores. Longitudinal data on students' scores provide the best way to measure their achievement in a given school year. Test scores from a single point in time measure their level of achievement at that time, encompassing their progress over an entire lifetime. Longitudinal data enable researchers to construct variables indicating students' *gain* in test scores, which should be influenced primarily by experiences during that school year, including SBP participation, and by their natural aptitudes.

The USBP design also allows for collection of school-level data over the baseline year and the 3 followup years. This effort focuses on collecting data that measure such outcomes as attendance rates and mean test scores. Because the data cover both the preimplementation and the postimplementation periods, analysis can focus on how the change in mean outcomes between the baseline year and a particular followup year differ between treatment (USBP) schools and control (regular SBP) schools.

An important issue in the USBP design is the potential for dilution of the effects of the intervention. Dilution occurs when the intervention (relative to the counterfactual) influences only a small proportion of the target population. In the case of the USBP evaluation, the main expected effect of the availability of the USBP, relative to the availability of the regular SBP, is limited to students who would not have eaten a school breakfast under the regular SBP but who do so under the USBP.

These "new participants" are the only group whose behavior changes as a result of the availability of the USBP.[9] However, all students attending treatment and control schools are to be sampled, not just new participants. Thus, the measured size of the effect of the intervention (that is, the difference in mean outcomes between treatment and control students) depends on the effect of breakfast program participation on new participants and on the percentage of the sample consisting of new participants.

A design option attempts to deal with the possible effect of dilution by maximizing the proportion of the sample made up of new participants. This approach involves conducting a "preimplementation" survey during the summer before the USBP is implemented in the control schools. Students in both treatment schools and control schools would be asked whether they had usually participated in the SBP during the previous school year and whether they would participate in the program if breakfasts were free to all students of the school.

Those who report that they did not participate during the previous year but would do so if breakfasts were free to all students are defined as "likely new participants." A key feature of this design option is that, at the time of the survey, students do not know whether their school has

[9] "New participants" refers to students in both treatment schools and control schools, even though students in control schools do not have the opportunity to participate in the USBP. For that group, being a new participant implies that they do not eat a school breakfast at their regular SBP school but would eat a school breakfast if they were attending a USBP school.

been selected as a treatment (USBP) school; thus, the question about future participation is purely hypothetical for both groups of students. After the likely new participants are identified, they can be oversampled, thereby lessening the problem of dilution.

3. Data collection

The USBP design calls for a wide range of data collection activities, including surveys of students, their parents, and their teachers; the collection of school administrative data, including achievement test data; and the administration of cognitive tests. This variety of data sources will ensure that the evaluation has information on a large number of student outcomes; however, it will require an extensive data collection effort. The key aspects of this effort are described here and are organized according to the source of the data.

a. Data collection from students and their parents

Much of the analysis to be conducted by the USBP evaluation will be based on data collected from students and their parents. These groups will provide data in three forms: (1) 24-hour dietary recalls, (2) student and parent surveys, and (3) cognitive tests.

Dietary intake. The USBP design will use a 24-hour dietary recall instrument to collect dietary-intake information from the students. The 24-hour dietary recall methodology provides information on all the foods eaten during a 24-hour period and will be conducted at the end of the first followup year. The design proposes conducting one 24-hour dietary recall for all sample members (at the end of the first followup period) and a second recall for a second representative subsample to allow estimation of the distribution of usual intake. Ponza et al. (1999) recommended using computerized interviewing methods and using the protocol from the most recent Continuing Survey of Food Intakes by Individuals to elicit complete information on food intake and combat the problem of missing data.

In practice, the design calls for an in-person dietary recall interview to be conducted with the child sample member alone, immediately after the breakfast period at school. The child's intake during the rest of the day would be collected during an interview with the child and his or her parent on the following day. The presence of the parent at the second interview will help the child remember the foods consumed at home during the intake day.

Student and parent surveys. The USBP design suggests conducting in-person interviews with children and their parents to obtain demographic and socioeconomic information and to fully assess breakfast program participation. These interviews would be conducted in conjunction with the dietary recalls, with students interviewed in school on the intake day and parents interviewed after the completion of the 24-hour recall. In the basic USBP design, student and parent interviews are scheduled for the end of the first followup year.

A design option is for the evaluation to include a preimplementation survey of sample members' parents. This survey would be conducted in the summer before the first followup year (and prior to the implementation of the USBP in the treatment schools). The main pieces of information collected would be sample members' participation status during the previous school year and their likelihood of participating in the following school year, if breakfast were free to all. If

implemented, the preimplementation survey would last only 10 to 15 minutes and would be administered to a subsample of approximately 150 parents in each school.

Cognitive tests. The design includes administering a short-term test of cognition—the Wechsler Memory Scale. This scale measures students' short-term memory, has performed well in research settings, and is straightforward to administer. It typically takes about 10 to 15 minutes and would be given after breakfast and before lunch on the day that students' dietary intakes are measured.

The USBP design also proposes a second short-term test—the Revised Children's Manifest Anxiety Scale. This test would measure the children's emotional state. It would take 10 to 15 minutes and would be administered at about the same time as the Wechsler Memory Scale.

b. School records data

Schools' administrative records data could provide a wide range of student- and school-level information useful for the USBP evaluation, including rates of breakfast program participation, measures of student behavior, achievement test scores, visits to the school nurse, and attendance and tardiness. The USBP design calls for schools to complete an administrative data form for the school as a whole and for individual students in the sample. This method would ensure that, to the extent possible, data from different districts would be in comparable form.

It is possible, however, that this method would impose a burden on participating schools to gather this information accurately and promptly. As a backup method, schools could be asked to provide electronic data files containing the requested information, which would then be manipulated by the evaluator to bring it into a reasonable form.

The collection of school- and student-level administrative data would begin during the baseline year and would continue throughout the 3-year demonstration period. At the beginning of the evaluation period, the evaluator would seek parental consent to obtain sensitive information on individual sample members and would work with schools to provide both this information and school-level information.

c. Data collection from teachers

The USBP design includes surveys of the sampled students' teachers to collect additional information about the students and about the general school climate. The survey would also collect information about the teachers, their impressions of the school and its students, and their comments about individual sample members. In particular, teachers would be asked about each student's attendance and tardiness, classroom behavior, health, and academic performance. Finally, the teachers would be asked a series of questions as part of the Connors Teacher Rating Scale (CTRS) to assess sample members' classroom behavior and attention level in class.

To collect data about individual students, a student-teacher crosswalk must be developed as part of the initial sampling frame. The teachers would then be sent a general questionnaire and a set of individual questionnaires, one for each sample member in their class. This survey would be conducted at the end of the first followup year.

d. Other forms of data collection

The USBP design also proposes three additional forms of data collection: (1) direct observation of sample members' participation in the SBP/USBP, (2) data collected from SFA and school personnel on the characteristics of demonstration schools and the schools' food service operations, and (3) qualitative data collection based on site visits.

Direct observation of breakfast program participation. Because accurate measurement of sample members' SBP/USBP participation is critical to the USBP design, the design includes an option for observing this outcome directly. Field interviewers or school staff would observe students as they passed through the cafeteria line or ate in the classroom at breakfast, noting when students in the study sample selected a school breakfast. This observation would take place during the 5-day school week in which the 24-hour dietary recall interviews were conducted. School meals for sample students would be observed at the end of the first and second followup years.

Data collection from SFA and school personnel. The USBP design includes surveys of the SFA directors of the six districts and of school administrators and cafeteria managers at the treatment and control schools. These surveys would provide information on SFA operations and school outcomes, such as attendance and SBP/USBP participation rates. They would be conducted at the end of the first followup year.

Qualitative data collection. The USBP design also calls for the collection of qualitative data covering issues related to USBP implementation. In particular, the evaluator's staff would conduct site visits to demonstration schools to collect information on such issues as:

- How schools have implemented the USBP
- How much cross-school variation in program implementation exists, and why this variation arises
- What strategies schools have developed to deliver breakfast program services
- How the USBP and regular SBP differ in their attempts to promote nutrition, learning, and other key outcomes
- How the program costs of the USBP and regular SBP differ
- What the characteristics are of the meals offered by USBP and regular SBP schools

The qualitative data collection effort would include a meals-offered survey; examination of program documents and records; focus groups with school staff and students; and semistructured interviews with SFA administrators, cafeteria managers, and school administrators. This effort would be conducted through a combination of telephone interviews and site visits.

4. Measurement of key characteristics and outcomes

Any design of the relationship between the SBP and learning must be able to accurately measure relevant aspects of students' participation in the breakfast program, outcomes related to their learning, and other relevant characteristics. Because the approach taken toward measurement of these factors influences the details of both data collection and data analysis, the design should carefully consider these measurement issues.

a. School breakfast participation

At a conceptual level, students should be defined as SBP participants if they select a set of foods from the school that qualifies as a USDA-reimbursable breakfast. Because most schools do not keep detailed student-level records on participation based on this definition, the USBP design relies on three approaches to proxy for it.

The first and most direct approach for determining students' participation status is to ask them (and/or their parents) directly whether they ate a school breakfast on a given day or during a given period. The drawback of this approach is that parents may not know, and their children may claim to have eaten a school breakfast to satisfy their parents even if they did not actually do so.

A second approach defines participation based on the foods the students obtained for breakfast from school. For example, the School Nutrition Dietary Assessment (SNDA-1) study defined school breakfast participants as students who obtained at least two food items from their school that contributed to the USDA breakfast pattern requirement. The third approach involves direct observation of breakfast program participation, as described in the previous section.

Participation can be defined either in terms of the target day (that is, the day on which the students' dietary intake information is collected) or over a longer period, such as a week, month, or school year. The USBP design recommends collecting both types of participation measures, because each type is appropriate for different outcomes. For example, one would expect short-term cognitive functioning to be most strongly influenced by breakfast program participation on the day that cognitive functioning is measured, rather than by usual participation. Conversely, academic achievement would more likely be influenced by usual participation, rather than by whether a student ate a school breakfast on any given day.

b. Outcome measures related to student learning

Student learning can be measured directly, through multiple years of achievement test score data, or indirectly, through various outcomes that may in turn influence academic achievement. These direct and indirect outcomes are described here.

Academic achievement. The most direct means of measuring students' learning in school is to measure their academic achievement. In particular, the gain in their level of academic achievement from one year to the next can serve as a measure of the amount that students learned during a school year. The best current measures of students' academic achievement are their scores on standardized tests. If the administrative records of participating schools provide these

test scores (or if these tests can be administered by the evaluator), the USBP design recommends using gains in test scores as the primary measure of student learning.

The most appropriate type of test for measuring student learning is a norm-reference test (NRT), which is designed so that scores can be compared with the scores of a reference group of students—typically, students across the country as a whole. This type of norming allows for comparisons of test scores across students taking different standardized tests, provided that the comparisons are made carefully. Examples of NRT's include the Iowa Test of Basic Skills (ITBS) and the Comprehensive Test of Basic Skills (CTBS). Ideally, these tests would be administered in the spring, to correspond with the evaluation's other data collection efforts.
In comparing students' test scores over time (to generate a measure of the gain in scores), across grades, or across schools, the USBP design emphasizes certain types of measures. For example, in addition to using NRT scores, scores can be further standardized by converting percentile scores to normal-curve-equivalent (NCE) scores, which have better mathematical properties. If data from different schools contain information on student scores on different standardized tests, the norming populations for the different tests should be as similar as possible. In addition, the subject matter of the tests should be similar, with reading test scores being compared with reading test scores rather than with math test scores.

Cognitive functioning. There are many aspects of cognition, as well as different measures of any one aspect. The USBP design recommends the Wechsler Memory Scale, based on a review of the literature on the effects of breakfast eating on cognition (Pollitt 1995; Vaisman et al., 1996). This literature indicated that tests of verbal memory are sensitive to breakfast consumption. Although breakfast eating may also may influence other aspects of cognition, such as visual perception, verbal fluency, and time on task, the limited resources of the evaluation and the limited time over which information can be collected from students indicate use of the Wechsler Memory Scale.

Emotional functioning. A number of instruments are available to measure various aspects of children's emotional status. Given time constraints and expectations about the effects of breakfast program participation, the USBP design calls for using the relatively short and easy-to-administer Revised Children's Manifest Anxiety Scale (Reynolds and Richman, 1985).

Attendance and tardiness. Obtaining school-level attendance data should be easy, as schools are required to report some measure of average daily attendance to district and State education authorities. When using school-level attendance data, however, it will be necessary to ensure that comparisons of attendance rates between schools are made only if the measures of attendance are similarly constructed, with similar definitions. Obtaining parental consent is a key issue with respect to collecting student-level administrative data on attendance (and on other outcomes).

Information on attendance and tardiness also would be obtained from the student and parent surveys. These self-reported data may be less reliable than the records data, but it would be possible to collect a wider range of information during these surveys.

Classroom behavior and disciplinary incidents. The USBP design calls for measuring student behavior in three different ways. First, the teacher surveys would include a variety of questions related to sample members' behavior, including the CTRS. In particular, the teacher survey would include the subscale of the CTRS that measures student hyperactivity. Second, the student and parent surveys would include questions on behavior. For example, sample members would be asked (possibly by proxy) whether they have engaged in particular behaviors, such as fighting or talking back to teachers, or whether they have been disciplined (formally or informally) during the past school year. Third, school records may contain some information on students' disciplinary incidents.

5. Analysis plans

The USBP design proposes two major types of impact analysis to examine the relationship between the breakfast program and learning. The first involves estimation of the effects of USBP availability on student outcomes—*the availability analysis*. The second involves estimation of the effects of SBP/USBP participation on student outcomes—*the participation analysis*. The availability analysis is based on an experimental design, whereas the participation analysis uses a nonexperimental design. The primary student outcomes to be examined, as called for by the Child Nutrition Act of 1998, are breakfast program participation, academic achievement, attendance and tardiness, and dietary intake over the course of a day. Finally, either of the two types of analysis can be estimated using student-level data or school-level data.

Two types of analysis, each of which can be estimated using school-level data or student-level data, result in four types of models overall. The four types are described here.

a. School-level USBP availability model

To estimate the effects of USBP availability on student outcomes using school-level data, Ponza et al. (1999) propose the following model:

(1) $Y_j = Z_j \delta_1 + \alpha_1 \text{USBP}_j + \varepsilon_{1j}$,

where:

Y_j	=	mean outcome among students at school *j*
Z_j	=	vector of characteristics of school *j*
$USBP_j$	=	binary variable representing USBP status of school *j*.

In the model, an outcome, such as a school's mean test score, is regressed on an indicator of whether the school is a USBP school (treatment school) or a regular SBP school (control school) and a set of school characteristics that potentially influence the mean test score.[10] The estimate of the parameter α_1 represents the estimated effect of USBP availability on academic achievement. The experimental design, if properly implemented, ensures that this estimate is unbiased. The model

[10]In this case, the control variables would include the lagged value of the dependent variable (that is, the mean test score at the school in the previous year). By controlling for the previous mean score, the estimated effect of USBP availability would be its effect on the gain in the school's mean test score, rather than its estimated effect on the level of the mean score.

would be estimated using ordinary least squares (OLS), logit/probit, or tobit estimation techniques, depending on whether the dependent variable is continuous, binary, or truncated.[11]

b. Student-level USBP availability model

The USBP design calls for the estimation of a hierarchical linear model (HLM) of the effects of USBP availability on student outcomes using student-level data. A simplified version of this model is shown here:[12]

(2) $Y_{ij} = X_{ij}\,\beta_2 + Z_j\,\delta_2 + \alpha_2\,USBP_j + \gamma_{2j} + \varepsilon_{2ij},$

where:

Y_{ij} = value of outcome among student *i* at school *j*
X_{ij} = vector of characteristics of student *i*.

The other variables in the model are defined as before. This model explicitly addresses the potential nonindependence of the error term across observations by giving each observation a school-level and a student-level component of the error term. The key estimate from the model is the estimate of the parameter α_2, which represents the average effect of USBP availability on the outcome. Again, random assignment ensures that this estimate is unbiased. If the outcome measure is participation in the breakfast program, α_2 represents the direct effect of USBP availability. For other outcome measures such as dietary intake or test scores, however, α_2 represents primarily an indirect effect. The main way in which USBP availability is expected to influence key student outcomes is by first influencing program participation, which, in turn, influences the outcome of interest.

c. School-level participation model

If the USBP data collection effort yields multiple years of information on school-level participation rates, school characteristics, and outcome measures, the following school-level participation model may be estimated:

(3) $Y_{jt} = Z_{jt}\,\delta_3 + \alpha_3\,P_{jt} + \gamma_{3j} + \varepsilon_{3jt},,$

where:

Y_{jt} = mean outcome among students at school *j* in year *t*
Z_{jt} = vector of characteristics of school *j* in year *t*
P_{jt} = participation rate among students at school *j* in year *t*
j = fixed effect of school *j*.

The key parameter in the model is α_3, which represents the influence of the participation rate on

[11]Furthermore, the sample of schools is unlikely to be a simple random sample. To account for any possible design effects, the standard errors of the coefficients would be estimated using Taylor series approximation methods with a software package such as SUDAAN.

[12]It is possible to increase the complexity of the model by modeling the coefficients on the school-level variables in the model as dependent on other school-level variables and a random error.

the school-level outcomes of interest. The model implicitly assumes that the effect of the participation rate is the same in USBP and regular SBP schools, although this assumption could be relaxed by interacting the participation rate variable with the binary variable indicating USBP availability. For the model to be estimated with a reasonable degree of precision, the participation rate variable must have sufficient exogenous variation over time and across schools. Without sufficient variation, the estimate of the effect of participation will be imprecise, and the analysis will lose statistical power.

The model allows for the direct control of the unobserved-school fixed effect, using binary variables representing each school in the sample. Controlling for the fixed effect in this way would address one possible source of selection bias—unobserved fixed school-level differences that are related to breakfast program participation and that affect the outcomes of interest.

d. Student-level participation model

The USBP design also proposes an HLM model to estimate the effects of breakfast program participation using student-level data. A simplified version of this model is:

$$(4) \quad Y_{Ij} = X_{ij}\,\beta_4 + Z_j\,\delta_4 + \alpha_4 P_{ij} + \beta_4 P_{ij} * USBP_j + \gamma_4 + \varepsilon_{4ij},$$

where:

P_{ij} = variable indicating the participation status of student *i* at school *j*.

The other variables in the model are defined as before. This model is similar to the student-level availability model, except that the key independent variables are the student's participation status and an interaction between participation status and the school's USBP status. The interaction is included to account for the fact that participation in the USBP may influence student outcomes differently from participation in the regular SBP. In particular, the estimated effect of participation in the regular SBP is represented by α_4, whereas the estimated effect of participation in the USBP is represented by $(\alpha_4 + \beta_4)$.

For the usual estimation techniques (OLS, logit/probit, tobit) to yield unbiased estimates of α_4 and β_4, the assumption that participation status is not correlated with the error terms of the model is necessary. In other words, unobserved factors influencing the outcomes of interest must not be correlated with whether or not students eat school breakfasts. As described previously, the nonrandom nature of the process by which students' participation status is determined leads us to question this assumption. If the assumption does not hold, then selection bias is possible.

The USBP design proposes three approaches to address the possibility of selection bias. The first approach attempts to ensure that selection bias does not arise in the first place. To prevent a correlation between participation status and unobserved determinants of key student outcomes, the model must explicitly control for all such factors. Ponza et al. (1999) identify three categories of factors that must be controlled for in the model: (1) detailed information on the students' socioeconomic status, (2) dietary habits of the sample members, and (3) dietary knowledge and attitudes of the food preparer in the sample member's household.

This approach for dealing with the possibility of selection bias is the best (if it is feasible), because it does not require complex econometric techniques or stringent assumptions. The limitation of this approach is that it is difficult in practice to control in sufficient detail for these three factors or for any other important factors affecting a student's decision to participate.

The second possible approach for dealing with selection bias in the student-level participation model is to estimate a fixed-effects model (that is, to directly control for an individual-specific fixed effect). In the model, this is a term representing unobserved factors that are specific to a given individual but constant over time, and that influence the outcome of interest. This approach requires data from more than a single point in time on the value of the outcome variable and on students' participation status (as well as on selected individual and school characteristics). However, given these data requirements and the fact that this approach does not control for time-varying sources of selection bias, Ponza et al. (1999) do not suggest that the USBP evaluation rely exclusively on fixed-effects methods for dealing with selection bias.

The third suggested approach is the estimation of instrumental variables (IV) and/or selection correction models. These are two-stage approaches to account for selection bias, with a first-stage equation that has participation status as the dependent variable, and a second-stage equation that is a version of the student-level participation model shown. In both models, information drawn from the estimation of the first-stage equation is used in the second stage to prevent a correlation between the participation variable and unobserved determinants of the outcome.

The major challenge to using this approach successfully is that it is necessary to find identifying variables for the model. In practice, identification of either model requires the inclusion in the first-stage equation of variables that influence students' participation status, but that do not directly influence the key outcome of interest in the second-stage equation. Finding appropriate identifying variables generally is difficult. The USBP design suggests the following variables:

- ***Timing considerations.*** These include variables that reflect the time that students must leave for school in the morning, the time that they arrive at school relative to the time school starts, the time that breakfast is served, and so on.
- ***Breakfast price and students' certification status.*** In regular SBP schools, these reflect what students must pay to obtain a school breakfast.
- ***Planted identifying variables.*** The design calls for exploration of an approach in which the evaluation team randomly assigns students to a treatment group that receives some incentive or encouragement to participate in the SBP/USBP and a control group that does not.
- ***Welfare status.*** The welfare status of students' families may reflect their attitudes about any stigma they might associate with receiving public assistance.

6. Statistical power

In the USBP design, Ponza et al. (1999) generated estimates of the MDDs of both USBP availability (based on an experimental design) and SBP/USBP participation. These calculations were based on a specific design in which the sample included 144 schools in the 6 demonstration

districts and 30 students from each school, with the schools equally divided by random assignment between treatment and control schools. The authors calculated statistical power for both the experimental availability analysis and nonexperimental participation analysis, although the calculations of statistical power for the nonexperimental participation analysis assumed no selection bias. If this assumption is relaxed, the resulting analysis will have much less statistical power.

For the experimental analysis, two types of statistical power calculations were made. First, power was calculated under the assumption that students would be sampled randomly from each school. The second set of calculations assumed that there would be a preimplementation survey, and that students most likely to be new participants would be oversampled.[13]

Ponza et al. (1999) calculated the statistical power of models with test scores and several other outcomes as dependent variables, but we will focus on the former. The target MDD for this outcome is 10 percent of a standard deviation. A model that has an MDD greater than this target is not sufficiently powerful for our purposes.[14]

With a preimplementation survey, the MDD for the experimental availability analysis with student test scores as the outcome variable is 16 percent of a standard deviation. In other words, the true effect of breakfast program participation on achievement test scores would have to be more than 16 percent of a standard deviation in order for the USBP design to have at least an 80 percent chance of finding the estimated effect of USBP availability to be statistically significant. Thus, the design does not reach the target level of statistical power for detecting impacts on achievement test scores, given the assumptions that were made. If the impact of the USBP on test scores is larger than anticipated, however, the design may have enough power to detect it.

In the nonexperimental analysis of the effects of SBP/USBP participation on test scores, the MDD is just under 9 percent of a standard deviation in the test score measure (regardless of whether the design includes a preimplementation survey). As mentioned, however, this calculation assumes no selection bias. As long as this assumption is credible, the nonexperimental analysis may be sufficiently powerful to determine the influence of breakfast program participation on learning.

7. Design Costs

The costs of the USBP design have been broken down into two components, demonstration costs and evaluation costs. The costs of implementing the USBP demonstration have been calculated assuming a design in which 72 treatment schools would implement a USBP in six districts. The costs are based on the difference between the free meal reimbursements for all participating students in

[13]See Ponza et al. (1999) for an in-depth description of the details of the power calculations conducted without a preimplementation analysis.

[14]However, the target MDD refers to the effect of eating a school breakfast, not the effect of attending a school in which the USBP is available. The target MDD for USBP availability depends on the percentage of new participants in the total sample, as USBP availability will influence the mean outcome in the sample only through its effect on those who become new participants. If 25 percent of the sample consists of new participants, the actual MDD from the models of USBP availability must be multiplied by four before being compared with the target effect size. This calculation has been made in the MDDs shown below.

USBP schools and the combination of free, reduced-price, and paid reimbursements for participating students in the control schools (which offer the regular SBP). In addition, the estimates of demonstration costs are based on the following assumptions:

- Each school includes seven grades (kindergarten through sixth grade), with 70 students per grade.

- The participation rate in control (SBP) schools is assumed to be 30 percent, with 80 percent of breakfasts for students certified for free meals, 6 percent for students certified for reduced-price meals, and 14 percent for students paying the full price.

- The participation rate in treatment (USBP) schools is assumed to be 55 percent.[15]

Under these assumptions, the estimated cost of implementing the USBP demonstration is in the range of **$6.6 to $6.9 million**. The cost estimate for the evaluation based on the USBP design is based on the size of the winning proposal by Abt. This estimate includes the cost of the base contract (which covers the collection and analysis of two years of school records data and a single year of survey data), contract option 1A (which covers the collection and analysis of preimplementation data), and contract option 3 (which covers the collection and analysis of two additional years of data). The winning Abt proposal was approximately **$6.4 million**. Combining the demonstration and evaluation costs leads to overall USBP design costs of **$13.0 to $13.3 million**.

8. Design summary: strengths and weaknesses

The USBP design proposes an experimental approach to estimate the impacts of USBP availability on student outcomes, including measures of learning. Under this approach, schools in the six demonstration districts would be paired and randomized, with treatment schools implementing the USBP and control schools continuing to offer the regular SBP. Both cross-sectional and longitudinal data would be collected from a sample of students at these schools (as well as from the schools), and the outcomes of students in the treatment and the control schools would be compared to determine the impact of USBP availability.

This analysis would include both school-level and student-level analysis. Finally, the design also includes an analysis of the effects of breakfast program participation on student outcomes. This portion of the design would use a nonexperimental approach, with the outcomes of students who chose to participate in the breakfast program compared with those of nonparticipants. Statistical methods would be used to control for the relevant observed and unobserved differences between participants and nonparticipants.

A primary strength of the USBP design is that it uses a rigorous methodology to determine the impacts of USBP availability. This methodology should lead to unbiased estimates of the effect

[15]The actual participation rate in the USBP is likely to depend largely on how the program is implemented. In schools in which free breakfasts are served in a cafeteria, the actual participation rate is likely to be below 55 percent. In schools in which free breakfasts are served in classrooms, the participation rate may well be above 55 percent.

of availability. In addition, within the constraints of the demonstration and evaluation (in particular, the limitation to schools from six districts), funding is available to collect information from a large number of students in a large number of schools. Another strength of this design is that it involves the collection of data on a large number of outcomes. Although the discussion has focused on test score data, the design also calls for collection of data representing dietary outcomes, cognitive function, and attendance in school, as well as other school outcomes.

A final strength of the USBP design is that it already is being implemented. This Congressionally mandated 3-year project, which began at the start of school years 2000 and 2001, is in its second year of implementation. Thus, many of the normal obstacles to implementing an experimental evaluation of a large program have been overcome.

The USBP design has three major weaknesses with respect to informing the debate on the effects of the SBP on learning. First, although the USBP availability analysis is based on an experimental approach, the part of the design that examines the impact of SBP participation is based on a nonexperimental approach. As a result, unobserved factors that are related to students' decisions to eat a school breakfast and that are correlated with the outcomes of interest—or selection bias—may hinder the evaluator's ability to generate unbiased estimates of the effect of SBP participation.

The second weakness of the USBP design results from the dilution effect. The fact that the treatment and control groups will include many students who are not influenced by the intervention hinders the design's ability to produce sufficiently precise estimates of the effects of USBP availability. In other words, some students who eat a school breakfast under the USBP would have eaten a school breakfast under the regular SBP.

Others would not eat a school breakfast under either set of circumstances. Because there is no way to eliminate these students without undermining the beneficial aspects of the experimental methodology, their presence reduces the expected size of the impacts and makes it more difficult to detect these impacts statistically. Consequently, the USBP design does not have sufficient power to detect the expected impacts of USBP availability on one of the key outcomes of the demonstration, academic achievement.

Third, the USBP design is limited in that its data will not be nationally representative. Sample members will come from only the six school districts selected for the USBP demonstration. Furthermore, because of the demonstration requirements, these districts will not necessarily be even qualitatively representative of school districts (and students) nationally.

Despite these weaknesses, the USBP design produces a wealth of useful information on the SBP in general and on the USBP in particular. The data collected as part of the evaluation and the rigorous methodological approach will help minimize the weaknesses and emphasize the strengths of the design.

C. SBP Applicant Design

Although the USBP design uses an experimental approach, it does not entail the random assignment of students into breakfast program participant and nonparticipant statuses. Under the methodologically ideal experimental approach to examining the effects of SBP participation, students within schools would be randomly assigned to a treatment group that participates in the SBP or to a control group that does not.

This experimental approach would be difficult to implement in practice, however, as denying school breakfasts to eligible students would raise ethical questions, and it would be hard to find schools willing to do this. The SBP applicant design uses an experimental design that would be easier to implement, and that would come closer than the USBP design to the methodological ideal of randomly assigning students to SBP participant and nonparticipant statuses.

The SBP applicant design involves schools that apply to participate in the SBP for the first time. Because these schools will not have offered SBP breakfasts, randomly assigning students to a control group that is denied access to the program would not amount to taking away a benefit they already had. However, randomly assigning students to treatment and control groups would be difficult to implement at a practical level. Thus, the SBP applicant design calls for randomly assigning the classrooms of applicant schools to a treatment group that participates in the breakfast program or to a control group that does not. Finally, so treatment group sample members would be most likely to eat a school breakfast, these meals would be free to all students in treatment classrooms.

1. Intervention and counterfactual

The intervention in the SBP applicant design is the offer of a free breakfast; this offer would last only for one school year. During that year, students in the treatment group would attend classrooms in which free school breakfasts were available. The breakfasts could be served in any way the participating schools chose, as long as they were available only to students in treatment classrooms. Two possibilities are that (1) breakfasts are served in the classroom, and (2) cafeteria breakfasts are served only when students in treatment classrooms have access to the cafeteria.

In the SBP applicant design, the counterfactual is not having access to any breakfast program (and thus not participating in one). Control group students would attend classrooms in which, as in the previous year, they do not have the option of eating a school breakfast.[16] However, these students may have eaten a breakfast at home or away from home.

[16]Although no control group students would participate in the breakfast program according to the design, some of these students may "cross over" and obtain school breakfasts in practice. For example, if breakfasts are served in the school cafeteria, control group students may somehow leave their classes and go to the cafeteria for breakfast. Alternatively, treatment group students who already have eaten potentially could obtain a breakfast and then give it to friends who are control group students. An evaluation based on the SBP Applicant design would have to monitor such possibilities closely.

2. Basic design approach

The SBP applicant design uses an experimental approach consisting of five steps: (1) selecting participating schools, (2) randomly assigning classrooms, (3) implementing the breakfast program, (4) selecting a student sample, and (5) collecting and analyzing the data.

a. Selecting participating schools

SBP applicant design schools would be chosen from the pool consisting of schools applying to USDA to join the SBP. The underlying logic is that denying school breakfasts to control group students would be more feasible than it would be in schools already offering the program. In other words, the control group would not lose a benefit it already had; these students would be in the same situation they were in during the previous year. The following schools would be chosen from the pool of SBP applicant schools:

- Elementary schools
- Schools with more than some minimum number of low-income students
- Schools that are willing to implement random assignment and to delay full implementation of the SBP for one year in control classrooms.

Schools applying for the SBP are clearly signaling their desire to implement the SBP in full immediately. Therefore, they would likely need an incentive to agree to delay full implementation for one year and instead implement random assignment. In this design, allowing these schools to implement a USBP in the treatment classrooms would be the incentive. Thus, although control classrooms would not participate in a breakfast program, students in treatment classrooms would receive free breakfasts. Additionally, USDA would reimburse the school at the higher free-breakfast rate for each breakfast served, regardless of students' certification status.

We expect that there would be enough appropriate applicant schools to meet requirements for generating sufficient statistical power. In the most recent year for which data are available, 1,500 schools applied for SBP benefits (Food Research and Action Center, 1999). Many were not elementary schools, and many probably had few low-income students. However, even if only one-fourth the schools that applied were elementary schools with a nontrivial number of low-income students, 325 schools were potentially eligible for a demonstration based on the SBP applicant design. That number of eligible schools would be large enough to support a rigorous study (assuming a substantial number could be persuaded to participate).

Of course, including only SBP applicant schools in the demonstration limits the generalizability of the results from an evaluation based on this design. In effect, the results would not be generalizable beyond the SBP applicant schools meeting the criteria listed here. These schools probably are different in a number of respects from schools that currently participate in the SBP. For example, current SBP schools tend to serve a larger percentage of low-income students than do non-SBP schools.

b. Randomly assigning classrooms

After applicant schools have been chosen for the demonstration, the next step would be to randomly assign classrooms in those schools to treatment and control groups. Random assignment at the classroom level gives the SBP applicant design greater statistical power than would random assignment at the school level.

Such assignment would have to occur prior to the beginning of the school year, to give schools and classrooms time to prepare to implement the program. In each school, classrooms would first be matched by grade level, with the set of classrooms restricted to grades 2 through 5.[17] If the school had any tracking of students across classrooms according to ability level, then classrooms consisting of students with similar ability levels would be matched prior to random assignment. After classrooms were matched in this way, random assignment would be implemented, and one classroom within each matched pair would be assigned to the treatment (breakfast program) group and the other to the control (no breakfast program) group.

c. Implementing the breakfast program

The schools would have to decide where breakfast should be served for each treatment classroom, and at what time. In addition, a key implementation issue would involve how to ensure that control group students do not have access to the breakfast program. To strictly control which classrooms are served breakfast, the breakfast could be served in the classroom. Many USBP schools currently serve breakfasts in the classroom (see Wahlstrom et al., 1997). Another possibility would be to serve breakfast in the school cafeteria, but only after the official start of the school day. Treatment group classrooms would then be brought to the cafeteria as a group and would be allowed either to eat a school breakfast or to engage in some other activity there.

The breakfasts served to treatment group students would be free, for two reasons. First, receiving the free meal reimbursement rate from USDA would serve as an incentive to SBP applicant schools to participate in the demonstration. Second, free breakfasts for all students would encourage student participation in the program, thereby increasing the statistical power of the analysis.

Participating schools would have to determine the activities in which control group classrooms would engage while the treatment group classrooms ate breakfast. If these activities include instruction or study periods, the design could lead to a negative effect of the program on the amount of time devoted to teaching in treatment classrooms. Therefore, teachers in control group classrooms could offer their students some type of noninstructional activity instead.

This design calls for the demonstration to last for one school year. After that, schools would be eligible to participate in the regular SBP. All students (regardless of treatment status) would become eligible for the SBP under the regular rules for free- and reduced-price meal certification, meal pricing, and reimbursement.

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[17]The classrooms randomly assigned would be limited to those in grades 2 through 5 because baseline data (including test scores, we hope) would be required from the previous school year. This type of data may not be available for first-grade students, who would have been in kindergarten the previous year.

d. Selecting a student sample

Although the SBP applicant design calls for the random assignment of classrooms, data would be collected on individual students from those classrooms. Because collecting survey data from each student in each treatment and control group classroom would likely be prohibitively expensive, the design calls for sampling of students in the classrooms. In particular, the most advantageous sampling plan would involve stratified random sampling, with oversampling of low-income students.

Information on students' income levels would be difficult to obtain prior to sampling, so students certified for free and reduced-price *lunches* would be oversampled, a process that would produce two benefits. First, low-income students would be the most likely ones to participate in the breakfast program. Second, these students probably would be less likely than higher income students to eat a breakfast both at home and at school. Among program participants, the fewer students who have already eaten breakfast at home, the larger the potential impact of the program.

e. Collecting and analyzing the data

The SBP applicant design would use a cross-sectional data collection plan primarily. Sampled students would be interviewed at the end of the school year and asked about their breakfast program participation during that year, as well as about other experiences in school. Administrative data on sample members' experiences and school performance during the year also would be collected. A key purpose of the data collection would be to obtain information on students' test scores.

Ideally, schools would administer their own tests during the spring, and this information would be available in the schools' administrative data systems. Otherwise, the evaluator would have to administer tests during that spring. Finally, administrative data from the previous school year would be collected so that any change in students' outcomes between the predemonstration year and the demonstration year could be measured. This information would be particularly helpful if it included test scores.

3. Data collection

The SBP applicant design data collection effort would be similar to that planned for under the USBP design. The specific data collected would depend on the level of resources to be expended on the evaluation but could include the following components:

- ***Student and parent data collection.*** Surveys of students in the sample and surveys of the students' parents would gather information on students' breakfast-eating habits, participation in the breakfast program (for treatment group students), experiences in school, and demographic/socioeconomic characteristics. If resources allow, dietary intake data could be collected from students, with their parents' assistance. Another option would be to administer short-term cognitive tests to students, provided administration was on a day for which we had information on breakfast program participation status.

- ***School records data.*** The collection of school records data would be an important part of the overall data collection plan. In particular, information on test scores for the spring of the demonstration year and for the previous year would be important, as would information on

attendance, tardiness, grades or other measures of classroom performance, disciplinary problems, visits to the school nurse, and other relevant information.

- ***Teacher data collection.*** As in the USBP design, the teachers of sample members would be able to provide useful information on student outcomes and could be surveyed at the end of the demonstration year. They could provide information on students' behavior and performance in class. Scales such as the Connors Teacher Rating Scale (CTRS) or the Pediatric Symptoms Checklist (PSC) should be considered for a teacher survey.

- ***Cafeteria manager data collection.*** Because it would be important to understand how breakfasts were served to treatment group students, a cafeteria manager survey would be useful. This survey would yield information on the time and place that school breakfasts were served, as well as on other characteristics of school breakfasts.

- ***Qualitative data.*** A final way to collect information on the implementation of school breakfasts in treatment classrooms and on any treatment-control differences not apparent from other forms of data would be to collect qualitative data. This form of data would be collected through evaluation staff's site visits to treatment and control classrooms of participating schools during the demonstration year.

4. Measurement of key characteristics and outcomes

Since school classrooms will be randomly assigned to the treatment and control groups, a key focus of the data collection will be classroom characteristics, which will serve both as control variables in the models measuring the impacts of breakfast program availability and as outcome measures. Such characteristics as the number of students in a classroom and the teacher's educational attainment and experience would be important control variables in the model.

Other variables, such as teachers' assessments of disciplinary incidents and the level of student performance, could serve as outcomes; the effect of school breakfasts being available in a classroom on student behavior and performance would be measured. The teacher survey would be used to collect the data on classroom characteristics and outcomes. Any additional classroom-level data available through school records also would be collected.

Other measurement issues and the appropriate way to address them are similar in the SBP applicant design and the USBP design. Accurate measurement of SBP participation is critical, as is measurement of student achievement as an indicator of learning. For a full description, see Section B of this chapter.

5. Analysis plans

Unlike the USBP design, which has several types of analysis and models to estimate, the SBP applicant design calls for a single type of analysis and model that should be sufficient to determine the impact of the breakfast program on student learning.

This type of analysis involves estimating the effect of breakfast program availability on learning at the student level. Because each participating school will have both treatment and control group classrooms, there is no school-level analysis.[18]

Although it would be feasible to conduct a separate analysis of the effects of breakfast program participation (analogous to the student-level USBP participation analysis), we hope that this step would not be necessary. If the availability analysis yields a sufficiently precise estimate of the impact of being in a classroom that offers school breakfasts, the estimate of this impact, along with the participation rate in treatment group classrooms, can be used to generate an estimate of the impact of participating in the breakfast program.

In particular, because the overall impact of breakfast program availability would occur entirely through students who actually eat a school breakfast, dividing the estimate of the impact of availability by the participation rate will yield an estimate of the impact of participation. For example, if attending a treatment group classroom that offers school breakfasts leads to an increase in test scores of 0.06 of a standard deviation, and if half the students in treatment group classrooms eat school breakfasts, then the impact of participation would be 0.12.[19]

Given the experimental approach of the SBP applicant design, a simple comparison of mean outcomes among treatment and control group students would yield an estimate of the impact of breakfast program availability. However, controlling for other relevant factors in a regression model would yield a more precise estimate of this effect. In particular, the SBP applicant design analysis plan calls for estimation of the following model:

(5) $Y_{ic} = X_{ic}\,\beta + W_c\,\gamma + \alpha\, SBP_c + w_c + \varepsilon_{ic},$

where:

Y_{ic} = value of outcome among student i in classroom c
X_{ic} = vector of characteristics of student i in classroom c
W_c = vector of characteristics of classroom c
SBP_c = treatment status of classroom c.

In this model, the error structure includes a random classroom-specific term and an independent and identically distributed error term. The model's estimate of the coefficient α is the estimate of the overall breakfast program impact. As mentioned, dividing this impact estimate by the participation rate in treatment group classrooms yields an estimate of the impact of participation.

The potential complications in the estimation of this model are similar to those in the estimation of the student-level availability model under the USBP design. Because the availability of the

[18]Conceivably, there could be classroom-level analysis, with classroom-level measures of student performance in treatment group classrooms compared with those in control group classrooms. However, we do not recommend pursuing this type of analysis, as it is unlikely that classroom-level data will be available.

[19]Making an adjustment of this type requires the assumption that nothing in the process of random assignment itself affects the outcome variable.

breakfast program influences only students who participate, this effect is "diluted" by the presence of nonparticipants in treatment group classrooms. If this dilution effect is large enough, the design may lack sufficient power to detect impacts of the size likely to be generated by the demonstration.

In this case, we could turn to nonexperimental methods to examine the effects of breakfast program participation on student outcomes. In particular, mean outcomes in participants in treatment group classrooms would be compared with mean outcomes in nonparticipants, after controlling for relevant characteristics in a regression framework. Because design is nonexperimental, we would be concerned about the presence of selection bias.

6. Statistical power

We must make several assumptions in order to determine the statistical power of the SBP applicant design. Following the USBP design, the key outcome for which we will examine minimum detectable differences (MDDs) will be test scores. The target level for the MDD will also be based on the same estimate, from Meyers et al. (1989), that the impact of SBP participation on test scores is 0.10 of a standard deviation. However, some students in the treatment group do not participate in the breakfast program, so this MDD target is not the relevant one for the SBP applicant design.

In particular, if the true effect of participation is 0.10 of a standard deviation, then the true effect of breakfast program availability will be (0.10) × (participation rate) standard deviation. We would oversample certified students, so we assume that the resulting participation rate will be 40 percent.[20] Thus, the target MDD for this design would be 0.040 of a standard deviation. In other words, the experimental-based analysis of breakfast program availability should be able to detect an effect of availability at least as low as 0.040 of a standard deviation.

Additional assumptions related to the power calculations include the following:

- Estimates based on the design are intended to generalize to the schools participating in the demonstration, but not beyond those schools to a wider population. This assumption is analogous to the assumption in the USBP design that the power calculations are not intended to generalize beyond the six demonstration districts. It is based on the fact that the schools will not be randomly chosen for the demonstration.

- Up to 100 schools will be included in the demonstration and evaluation.

- Each demonstration school includes grades 2 through 5 (the grades included in the demonstration) and has four classrooms per grade that can be evenly divided between treatment and control classrooms. Each classroom has at least 15 students.

[20]Oversampling certified students will create a design effect. Although we do not know the exact magnitude of this design effect, we do know that the greater the extent of oversampling, the greater it will be. For simplicity, we ignore the design effect in calculating the MDDs in this section. However, if a full SBP applicant design plan is to be developed, this design effect should be taken into consideration.

- Overall, 10 percent of the total variation in test scores is explained by cross-school variation, and another 5 percent is explained by cross-classroom (but within-school) variation.

Table III.1 shows the MDDs under alternative assumptions about the number of schools and number of students per classroom in the demonstration. The table suggests that increasing the sample size by increasing the number of schools in the demonstration has the largest effect on the MDDs. For example, moving from a sample of 5 students in each of 16 classrooms in 50 schools (for a total sample size of 4,000 students) to a sample of 5 students in 16 classrooms in 100 schools (8,000 students) leads to a decrease in the MDD from 0.069 to 0.049.

By contrast, increasing the total sample to 8,000 students by increasing the number of students per classroom from 5 to 10 leads to a decrease in the MDD from 0.069 to only 0.054. After the maximum number of schools has been reached, however, the decrease in the MDD caused by an increase in the number of students per classroom from 5 to 10 is greater than the decrease caused by an increase from 10 to 15.

Table III.1—SBP applicant design minimum detectable differences

SCHOOLS	**Classrooms per school**	**Students per classroom**	MDD
Number			
50	16	5	0.069
50	16	10	0.054
50	16	15	0.048
75	16	5	0.056
75	16	10	0.044
75	16	15	0.039
100	16	5	0.049
100	16	10	0.038
100	16	15	0.034

The analysis indicates that, to achieve sufficient statistical power, the SBP applicant design requires 16 classrooms per school (including 8 treatment and 8 control classrooms) and a sample of either 100 schools and 10 students per classroom (for a total sample size of 16,000) or 75 schools and 15 students per classroom (for a total sample size of 18,000).[21]

[21]Actually, with 10 students per classroom and 16 classrooms per school, a sample of 94 schools (for a total sample size of 15,040) would be sufficient to yield an MDD of 0.039.

This result is dependent on the assumptions shown above, which should be more rigorously assessed if this design goes forward. However, this preliminary power analysis suggests that the design would require a large effort to recruit participant schools and to implement the demonstration program in treatment classrooms, as well as to collect data on the sample and to conduct the analysis.

7. Design costs

The cost estimates for the SBP applicant design include three components: (1) demonstration costs, (2) data collection costs, and (3) analysis and reporting costs. The key assumptions on which each component is based are described here.

The SBP applicant design calls for free breakfasts to be served in half the classrooms of demonstration schools and for no breakfasts to be served in the other half. If this demonstration program is not implemented, we assume that all these schools would offer students the regular SBP.

Thus, demonstration costs for the SBP applicant design have been calculated as the difference between the cost of providing free breakfasts to participating students in half the participating schools' classrooms (but with no additional costs, as the remaining classrooms have no breakfast program) and the cost of providing a combination of free, reduced-price, and full-price breakfasts to participating students in all classrooms.

In other words, the implementation of the demonstration includes not only the cost of free meals served to students in treatment classrooms, but also the savings from not serving any breakfasts to students in control classrooms.

Additional assumptions on which the calculation of demonstration costs is based include the following:

- The demonstration involves 100 schools with 8 classrooms (of 25 students each) assigned to the treatment group and 8 classrooms (of 25 students each) assigned to the control group.

- The participation rate in treatment classrooms is assumed to be 75 percent.[22]

- The participation rate under the regular SBP is assumed to be 30 percent, with 80 percent of breakfasts served to students certified for free meals, 6 percent to students certified for reduced-price meals, and 14 percent to students who pay the full price for breakfast.

Given these assumptions, the estimated cost of implementing the SBP applicant demonstration is $0.9 to $1.0 million.

As described, the evaluation costs include both data collection costs and analysis and reporting costs. In addition to the assumption that 100 schools (spread across 80 school districts) would

[22]This assumed participation rate is higher than that assumed for the USBP demonstration; we feel that it would be more likely in this demonstration for breakfasts to be served in the classroom, rather than in the cafeteria.

participate, with 16 classrooms in each school, we assume that a sample of 10 students in each of the 1,600 classrooms would be selected for the evaluation sample, leading to a total sample size of 16,000.

The data collection effort would include two years of school records data collection and the one-time administration of a single set of surveys to school principals, cafeteria managers, classroom teachers, students, and their parents. Data to be collected from students would include dietary intake data and the administration of short-term cognitive tests. With these data collection activities, the estimated total data collection costs for the SBP applicant design are estimated to be $10.0 to $10.5 million.

The estimated analysis and reporting costs assume a two-year evaluation period with one major report. These estimated costs fall in the range of $0.7 to $0.8 million. Together with data collection costs, total evaluation costs for the SBP applicant design are $10.7 to $11.3 million. Total costs for fully implementing the SBP applicant design and evaluation are $11.6 to $12.3 million.

8. Design summary: strengths and weaknesses

The main strength of the SBP applicant design is its experimental design, which yields unbiased estimates of the effect on student achievement and other outcomes of being in a classroom that offers free breakfasts (relative to having no breakfast program). Furthermore, treatment status is much more closely correlated with SBP participation status in the SBP applicant design than it is in the USBP design, the other experimental design.

Although the SBP applicant design does not reach the methodological ideal of randomly assigning students to participant and nonparticipant statuses, it comes as close as is feasible. In addition, by randomly assigning classrooms, as opposed to schools, the design generates more statistical power than does the USBP design. Finally, although this design does not yield nationally representative estimates, the large number of participating schools should give the results broad geographic representation.

One of the primary weaknesses of the SBP design is its difficulty of implementation. While it easy to stipulate in the design that classrooms will be divided into control and treatment groups, at the school level, this type of differentiation can cause problems, such as resentment amongst the teachers and families associated with the classrooms chosen for the control group. Furthermore, not all schools would be willing to serve breakfast in the classroom, as this creates extra work for teachers and custodial staff. Teachers may also feel that meal service takes away valuable instruction time. Finally, it could potentially be very difficult to match four classrooms at each grade level.

Another weakness of the SBP design is the possibility that the resulting estimates would not have sufficient statistical power. The power of the design relies largely on the ability of the evaluator to recruit a sufficient number of schools for the demonstration. In particular, the design requires roughly 100 elementary schools that are applying for participation in the SBP; have a nontrivial number of students certified for free or reduced-price meals; and are willing and able to delay full SBP implementation for one year, conduct random assignment of classrooms into breakfast program and no breakfast program groups; and implement a free breakfast program in treatment

classrooms only. Analysis based on substantially fewer than 100 schools is likely to result in estimates of the impact of the SBP that are statistically insignificant, whether or not the true impact of the program is positive.

The SBP applicant design also has several other weaknesses. If breakfasts are delivered in treatment group classrooms during the school day, it is likely that treatment group classrooms will have less time than control group classrooms for instruction. In addition, free breakfasts delivered to classrooms may induce some students who already have eaten breakfast to participate in the program, an outcome that the evaluation should track.

This participation, if it occurs, would decrease the likelihood that program participation will have a detectable positive influence on learning outcomes. Finally, because the schools eligible for the demonstration are limited to those applying to participate in the SBP, the sample will not be representative of students in current SBP participant schools, which tend to serve large percentages of low-income students.

An additional consideration in assessing the SBP applicant design within the context of the four alternative designs presented in this report is that it is fairly similar to the USBP design, which currently is being implemented. Thus, the degree to which it can provide insights in addition to what is learned from the USBP evaluation should be carefully considered. However, although the USBP design examines the impact of free school breakfasts relative to the regular SBP, the SBP applicant design examines the impact of free school breakfasts relative to no breakfast program. Thus, despite the similarity of the two designs, the SBP applicant design has the potential to answer a unique question.

D. ECLS-K-Based Design

In this section, we describe a design for using the ECLS-K, Kindergarten Cohort, to relate information on students' SBP participation to cognitive performance. The ECLS-K consists of a nationally representative sample of 16,906 students who were in kindergarten programs in 866 schools as of fall 1998.[23] The study is scheduled to include multiple assessments of children's cognitive, physical, social, and emotional development over time; it will also include surveys of families and schools for children in kindergarten, grade 1, grade 3, and grade 5. With information in the ECLS-K, perhaps supplemented by an expanded questionnaire, it is possible to estimate, in a nonexperimental context, the consequences of SBP participation on both children's cognitive performance and related outcomes from kindergarten through grade 5.

1. Intervention and counterfactual

The primary intervention that this design examines is SBP participation, or eating a school breakfast in a school breakfast program funded (presumably) by USDA. In particular, the intervention is defined as usually eating a school breakfast over the course of a school year while attending a school in which the school administrator reports that students are certified to receive

[23]The study originally targeted approximately 21,000 students in 995 schools, but only 16,906 survey respondents have both child and parent information, and only 866 schools have positive sample weights.

free breakfasts.[24] Furthermore, the intervention refers to participation in the SBP *as it is currently administered in schools throughout the country*.

Although policymakers may be interested in examining versions of the SBP that deliver breakfasts in new and/or innovative ways, the ECLS-K is not suitable for this purpose. In addition, the definition of the intervention requires more specifics as to what "usually eats a school breakfast" means.

The counterfactual in the ECLS-K design is "not eating a school breakfast," or "not participating in the SBP." This design can examine the impact of a school simply offering the SBP to students, as well as the impact of actual SBP participation. In particular, the design can address the following four questions:

- What is the impact of SBP participation on current participants?
- What would be the impact of SBP participation on current nonparticipants in schools offering the SBP, schools not offering the SBP, or both?
- What is the impact of offering the SBP in schools where it currently is offered?
- What would be the impact of offering the SBP in schools where it currently is not offered?

To address the first two questions, the impact of SBP participation would be inferred by using statistical methods to compare outcomes in a group of current SBP participants and a comparable group of nonparticipants. To address the third and fourth questions, the impact of offering the SBP would be inferred by using statistical methods to compare outcomes in a group of students attending SBP schools and a comparable group of students attending non-SBP schools.

Because the counterfactual includes both students who eat no breakfast and those who eat a nonschool breakfast, the main impact being estimated does not tell us the effect of eating breakfast versus no breakfast. Instead, it tells us the effect of eating a school breakfast versus eating no school breakfast. This could occur both because the school breakfast program makes children more likely to eat breakfast and/or because the program influences the foods consumed by those who do eat breakfast. This issue can be examined by comparing outcomes among SBP participants, nonparticipants who eat breakfast, and children who eat no breakfast.

2. Basic design approach

The ECLS-K design uses a nonexperimental, comparison group design. This approach involves comparing key outcomes in a group of students who participate in the SBP with outcomes in a group of students who do not eat a school breakfast. As suggested in Section 1, these comparison group students could attend SBP schools, but choose not to eat a school breakfast, or could attend

[24] Some schools may operate school breakfast programs of their own, without USDA funding. However, we assume that, in such instances, the school administrator would be less likely to report that students are certified as eligible for free breakfasts at the school.

non-SBP schools and therefore not have the option of eating a school breakfast. The analysis would be conducted primarily at the student level, but there also would be some school-level analysis.

Because this design is nonexperimental, students are not randomly assigned to a treatment group that receives breakfast and a control group that does not receive breakfast. Therefore, SBP participants and nonparticipants may differ in ways that influence outcomes related to learning. The nonexperimental design must account for the extent to which differences in key outcomes are the result of these differences in student characteristics, rather than the result of eating a school breakfast. Statistical methods, such as instrumental variables models, may be used for this purpose.[25] After the differences in participants' and nonparticipants' characteristics have been accounted for, any remaining differences in key outcomes can be attributed to the influence of SBP participation.

The ECLS-K design is based primarily on cross-sectional analysis. This analysis would involve measuring students' cognitive functioning at a point in time and comparing this outcome in students who report (at the same point in time) that they are SBP participants versus those who report that they are not participants. The analysis is cross-sectional because the cognitive functioning outcomes and the SBP participation variable each are reported at a single point in time for each student.

The primary strength of the ECLS-K data for studying the impact of SBP participation on learning is that it contains detailed information on various aspects of children's cognitive development. One of the primary motivations for the ECLS-K data collection was to track various aspects of young children's development, so great efforts were made to collect a broad range of high-quality data on cognitive outcomes. The resulting data will provide the most accurate picture available on the cognitive functioning of a large, nationally representative sample of elementary students.

In addition to having high-quality measures of students' learning, the ECLS-K data include several questions related to SBP participation. This information is available at both the school level and the student level. When combined with the detailed information available on student outcomes, family income and demographics, and school characteristics, the breakfast information in the ECLS-K enables researchers to estimate the effects of SBP participation on learning, accounting for a variety of characteristics that distinguish SBP participants from nonparticipants.[26]

Two other aspects of the ECLS-K make it particularly appealing for a study of the relationship between SBP participation and learning. First, it is based on a very large, nationally representative sample. The sample of more than 16,000 students is large enough to support analysis of the relationship between SBP participation and learning both in the overall sample and among key

[25] These methods are described in greater detail in Chapter IV.

[26] See Chapter IV for further discussion of ECLS-K data and measurement issues.

subgroups. Second, the longitudinal nature of the data not only provides the analytic advantages described here, but also allows the participation-learning relationship to be estimated at various points in time. Thus, the analysis can reveal whether the effects of eating a school breakfast are different for children of different ages.

3. Data collection

The ECLS-K collects some breakfast-related information in its surveys of parents, teachers, and school administrators. However, we recommend collecting supplemental data to better evaluate the impact of SBP participation on learning at both the student and the school levels. A major goal of this additional data collection is to address the issue of selection bias by identifying factors that contribute to school breakfast participation but not directly to learning outcomes. Supplemental data also would help researchers measure school breakfast and school lunch participation more accurately on the student and school levels.

Additional information collected from parents would contribute to the analysis of variation in student outcomes *within* schools, whereas information collected from school administrators would contribute to the analysis of the variation in outcomes *between* schools. Accounting for more of the variation in student outcomes will produce more precise estimates of the SBP participation.

Additional data can be collected most conveniently by adding targeted questions to existing surveys of parents and school administrators. In Chapter IV, we discuss these surveys as well as our reasons for not recommending the collection of dietary intake data in the context of this evaluation design.

4. Measurement of key characteristics and outcomes

The ECLS-K measures various characteristics of children, their families, and their schools that are relevant to a study of how school breakfasts influence learning. This section briefly discusses those variables, indicators of SBP participation, and student and school characteristics that can be used as control variables. A more detailed description of the variables is presented in Chapter IV.

a. Outcome variables

The ECLS-K includes a rich set of variables that measure three types of outcomes: (1) a student's cognitive development; (2) events and processes associated with learning, such as school attendance and tardiness; and (3) other aspects of a child's growth, including emotional, social, and physical growth. Analysis of ECLS-K data should therefore enable researchers to estimate differences in a wide range of outcomes by SBP participation status.

b. SBP participation

Participation in the SBP is a key variable for the analysis of the link between school breakfast and learning. The ECLS-K collects information on SBP participation at the school and child levels. On the school level, principals are asked how many students are eligible for, and how many students participate in, the *free* breakfast program at the school. Principals are *not* asked how many other students receive school breakfasts, at either the reduced or the regular rate. This information could be gathered through the supplemental survey.

At the child level, the ECLS-K collects data from parents on whether their child's school serves breakfast, whether the child usually eats a school breakfast, and the number of times the child ate the breakfast during the previous 5 school days. The SBP participation variable can be defined differently, depending on the nature of the analysis. For an analysis of within-school differences in individual student outcomes, participation might be measured using the number of school breakfasts students ate during the previous 5 school days.

In a linear model, that definition would imply that the effect of SBP participation on learning is proportional to the number of days the child ate a school breakfast during the previous week, with the implicit assumption that the number of breakfasts consumed during the previous week is a good proxy for the number of breakfasts consumed in the typical week. A less restrictive specification would have separate indicators for each level of weekly participation—1, 2, 3, 4, or 5 days, to allow the effect of the number of breakfasts consumed per week to influence learning nonlinearly.[27]

c. Background characteristics of students and schools

Because the ECLS-K collects information from both schools and parents, it includes a rich set of background characteristics on students and schools. Access to rich data is particularly important because it makes it possible to control for factors important to SBP participation as well as to learning. Previous research on the determinants of SBP participation (based on data from the School Nutrition Dietary Assessment study for the 1991–1992 school year), indicate that a wide variety of factors are related to whether or not children eat school breakfasts.

Among children attending schools that offer the SBP, for example, participation has been found to be more likely among children who are younger, male, black or Hispanic, living in rural or suburban areas, and who are certified for free or reduced-price meals (Gleason, 1996). In addition, participation is correlated with the region in which the child lives and with the full price charged for breakfast in the school.

5. Analysis plans

This ECLS-K-based approach to analyzing the impact of school breakfasts on learning is nonexperimental in nature. The design must therefore rely on multivariate statistical methods to infer the difference that SBP participation makes on the educational outcomes of students.

Let us assume that we observe a particular learning-related outcome, Y_{is}, for student i in school s. We assume that the outcome is a function, $g(.)$, of the availability of school breakfasts at a child's school, A_s; of other school characteristics, Z_s ; of student background characteristics, X_{is}; and of school breakfast participation for the student, P_{is}:

(6) $Y_{is} = g\,(A_s\,,\, Z_s\,,\, X_{is}\,,\, P_{is})$

Examples of Y_{is} include the growth in student test scores from one year to the next and attendance patterns in a given year. Examples of characteristics in Z_s include teachers'

[27] Of course, any school breakfast participation variable will be affected by the degree of parental accuracy in reporting on participation, which is unknown at this point.

characteristics and factors related to a school's decision about whether and how to offer the SBP to students, such as the proportion of students at the school certified as eligible for free or reduced-price school *lunches*.

Examples of characteristics in X_{is} include family income, parental education, household food security, and a student's prior test scores.[28] (We also assume that X_{is} includes a constant term.) The participation variable, P_{is}, may be defined in a variety of ways, but for simplicity, we assume that it is a single indicator for "usual" SBP participation, as indicated through the ECLS-K parent survey.[29] Note that when the school does not offer school breakfasts ($A_s = 0$), students do not receive any school breakfasts ($P_{is} = 0$).

To estimate the relationship between SBP participation and learning outcomes, it is necessary to make assumptions about the relationship between the variables contained in equation (6). When using linear regression methods, we assume that equation (6) has the following form:

$$(7) \quad Y_{is} = \alpha A_s + \beta Z_s + \gamma X_{is} + \delta P_{is} + u_s + e_{is} ,$$

where u_s is a school-specific error term, and e_{is} is an individual-specific error term. In this equation, the coefficient α represents the contribution of the availability of school breakfasts to learning for nonparticipants; the coefficient matrix β represents the contribution of other school characteristics to learning; the coefficient matrix γ represents the contribution of personal and family characteristics to learning; and the coefficient δ represents the contribution of school breakfast participation to learning.

For simplicity of presentation, equation (6) specifies that a given level of SBP participation has a uniform effect on learning, regardless of the characteristics of the school or individual students. In practice, researchers should try estimating separate equations for different subgroups of students.[30]

Because we are likely to have multiple observations of students from each school, we can subtract the school-level means of *Y, X,* and *P* from equation (7) and estimate the following:

$$(8) \quad (Y_{is} - Y_s) = \gamma (X_{is} - X_s) + \delta (P_{is} - P_s) + (e_{is} - e_s).$$

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[28] The ECLS-K includes an 18-item food security module that can be used to categorize households as "food secure," "food insecure without hunger," or "food insecure with hunger." In addition, ERS is currently developing a children's hunger scale from these items to assess whether there is hunger among children in the household.

[29] In the basic model, we will not use the variable measuring the actual number of days of participation during the previous five days at school, because—as we note in Chapter IV—this variable likely measures with some error the level of SBP participation contributing to educational outcomes. In practice, most students who "usually" participate in the SBP have received breakfasts during the preceding 5 days at school.

[30] In this chapter, we discuss the estimation of separate impacts of attending an SBP school for students with different propensities of SBP participation.

Note that the estimates obtained from equation (8) are the same as those that would be obtained by using a dummy variable for each school in equation (7) to capture the combined effect of $(\alpha A_s + \beta Z_s + \gamma X_s + \delta P_s + u_s)$. This "fixed school effects" estimator allows us to account for *all* time-invariant school-specific characteristics affecting outcomes without having to specify what those characteristics actually are.

If we believe that the existing school-level variables contain enough school-level information to predict school-level learning outcomes (that is, that there are no unobserved school-level effects correlated with unobserved individual-level effects), we can improve on the efficiency of the fixed-effects estimator through a method known as "random-effects." Assuming that $e_s = 0$, we can estimate the parameters of using the variation in average outcomes between schools, as expressed by the equation:

(9) $Y_s = \alpha A_s + \beta Z_s + \gamma X_s + \delta P_s + u_s.$

Equation (8) therefore captures the within-school variation in learning outcomes, whereas equation (9) captures the between-school variation in learning outcomes. Under the assumption that e_{is} and u_s are uncorrelated (that is, that there are no missing variables in equation (9) correlated with the unexplained portion of individual outcomes), it is possible to obtain efficient estimates of γ and δ as an optimally weighted average of the corresponding estimates from equations (8) and (9).[31] If this assumption does not hold, then the random-effects estimator is biased, and the fixed-effects estimator is preferable.[32]

Using a linear, fixed-, or random-effects model, we can include in the analysis schools that do not offer the SBP and can assume that the impact of SBP participation in these schools would be the same as in schools offering the SBP. Because this assumption may be unrealistic, researchers should estimate equations (8) and (9) both with and without schools that do not have an SBP. In this way, the researchers can determine whether including nonparticipating schools in the sample has any substantial effect on the parameter estimates of interest.

In general, including non-SBP schools will improve the efficiency of estimates of the effects of SBP participation on learning and will enable researchers to estimate the impact of attending a school offering the SBP. Various alternative models of the effect of SBP participation on learning can be estimated using ECLS-K data. Two of these alternatives are instrumental variables and propensity score models, each of which is designed to address different limitations of the basic model described above. Details of the alternative models, along with their strengths and weaknesses, are presented in Chapter IV.

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[31] The assumption that e_{is} and u_s are uncorrelated is a particularly strong one. If the assumption does not hold, then the random-effects model will lead to biased estimates. However, it is possible to test the validity of this assumption using a specification test developed by Hausman (1983). For additional information on the formation of the random effects estimators, see Greene (1997).

[32] In addition to estimating fixed- and random-effects models, investigators may want to consider estimating hierarchical linear models (HLMs) that take into account the clustering of students in observation schools and that also allow the effects of the characteristics of classroom units on the outcomes of interest. See Bryk and Raudenbush (1992) for more details.

6. Statistical power

MDDs are related to the "power" of an analysis. The power of an analysis refers to the likelihood that it will detect, at a given level of statistical significance, a certain magnitude of difference in outcomes between program participants and nonparticipants. For given levels of power and statistical significance, the minimum difference between outcomes that can be detected by a particular analysis is known as the MDD. We calculate MDDs that can be detected with 80 percent power using a 95 percent confidence interval and a one-tailed test (details are discussed in Chapter IV). To do this in the context of the nonexperimental evaluation of the SBP, we rely on formulas developed by Gleason (2000).

The logic underlying these MDD calculations (both with and without selection bias) is that they are very similar to the calculations based on an experimental design except that treatment status (participation) is not independent of other components of the regression model—the control variables and, in the case of selection bias, the error term.

Thus, there is less "useful," or exogenous, variation in participation status on which to base the estimates. This problem is essentially one of the multicollinearity of the treatment variable: the greater the multicollinearity, the larger the MDD for any given level of power (or, conversely, the less the power of the analysis for any given MDD).

Given the large number of schools and students in the ECLS-K dataset, the models we propose for estimating the impact of SBP participation on learning have high levels of statistical power under a given set of assumptions. One of these assumptions is that the model does not suffer from selection bias and can be estimated without using an instrumental variables (IV) model. However, if this assumption does not hold, and an IV model must be estimated, the MDDs are much higher and the statistical power much lower. Additional details on the statistical power of the ECLS-K design can be found in Chapter IV.

7. Design costs

Because the ECLS-K-based design is based on analysis of secondary data, the estimated costs of the design do not include demonstration costs or data collection costs (except for the costs associated with supplemental data collection). The main design costs are associated with analysis and reporting. These costs fall in the range of $0.4 to $0.5 million.

Although the design does not include primary data collection, we suggest supplemental data collection activities; namely, adding questions to the ECLS-K parent and school administrator questionnaires (as described in Chapter IV). Although we do not have specific cost estimates for these supplemental data collection activities, we have estimated the implications of the suggested additions to the surveys in terms of the length of the questionnaires. In particular:

- Supplemental questions to the ECLS-K parent survey in the spring of 2002 (as well as in the spring of 2004) will increase the length of the existing instrument by approximately 2 pages, from 146 to 148 pages (or 1.4 percent). This instrument is administered to approximately 20,000 parents.

- Supplemental questions to the ECLS-K school administrator survey in the spring of 2002 (as well as in the spring of 2004) will also increase the length of the existing instrument by approximately 2 pages, from 36 to 38 pages (or 5.6 percent). This instrument is administered to approximately 1,000 school administrators.

8. Design summary: strengths and weaknesses

An ECLS-K-based design for evaluating the impact of the SBP on learning has strengths and weaknesses. The major strengths are that it relies on a large, nationally representative sample of elementary school students and schools and on a database that includes a rich variety of outcome measures and student background characteristics. The weaknesses are related to selection bias and the resulting difficulties of obtaining reliable impact estimates for certain subgroups of students.

Although IV methods may help correct for selection bias, and propensity score methods may help construct comparable subgroups of SBP participants and nonparticipants, each method requires the researcher to make what may be unreasonable assumptions about the determinants of SBP participation. That is, the researcher must assume either that certain variables affect participation but not outcomes or that the unobserved factors influencing outcomes are uncorrelated with the propensity to participate in the SBP. Supplemental data collection from parents and schools on additional factors influencing SBP participation (but not learning) can reduce the need to make unreasonably restrictive assumptions when using these methods.

Even if the assumptions required for unbiased estimation were true, however, the small number of observed SBP participants among higher income students makes it difficult to use ECLS-K-based impact estimates to generalize about the likely impact of a universal SBP. To understand the likely consequences of universal-free school breakfasts, an experimental USBP evaluation would appear to be necessary. At the same time, an experimental demonstration of the USBP would not enable researchers to estimate the impact of the SBP on the *current* participants, because this group presumably would continue to receive school breakfasts under the USBP.

In an experimental context, testing the impact of the SBP on current participants would require denying SBP benefits to them. This type of experiment would be both politically infeasible and ethically questionable. Consequently, using the ECLS-K to construct a comparison group of SBP nonparticipants may be the most feasible way to determine the impact of school breakfasts on current participants' learning.

E. NHANES Design

In this section, we describe a design for using NHANES to study factors associated with SBP participation and learning. The NHANES design is similar to the ECLS-K design in that it is nonexperimental and based on a national survey of school-aged children that contains information on SBP participation and family background.

Unlike the ECLS-K, however, NHANES includes comprehensive information on dietary intake, nutritional status, and health status but contains little information on learning outcomes, school attendance, social and emotional development, or school characteristics. Supplemental data collection to capture information on academic achievement, cognitive function, and school performance is recommended if the NHANES design is to be used to full advantage.

1. Intervention and counterfactual

In the NHANES design, the intervention is participating in the SBP (that is, usually eating a school breakfast). The counterfactual is nonparticipation (not usually eating a school breakfast). To study learning outcomes, we are most interested in an intervention that covers a substantial period of time—"usual" participation over the school year.

This information is collected in the NHANES as the number of times per week that the child selects a school breakfast. For the intervention group, we are interested in defining participation as selecting a school breakfast on most school days. In terms of the population targeted by the intervention, children in elementary school are of greatest interest; however, we are also interested in older children (especially girls), who are less likely to report eating breakfast.

The main counterfactual condition is attending a school in which the SBP is offered but not "usually eating" a school breakfast during the year. However, we may also want to examine the counterfactual condition of attending a school in which the SBP is not offered. Finally, it will be useful to examine differences in outcomes among SBP participants, nonparticipants who eat breakfast outside of school, and children who do not usually eat breakfast.

The last would require the addition of a survey question on children's usual breakfast habits. This analysis would involve examining whether the relationship between SBP participation and the outcomes of interest arises simply because SBP participants are eating any breakfast or because of the composition of the breakfasts they are eating.

2. Basic design approach

The NHANES design uses a nonexperimental approach to examine the effects of SBP participation on learning relative to the counterfactual of not eating a school breakfast. We would observe students' participation status and would then compare mean outcomes in participants and nonparticipants. The key to the design lies in the strengths and weaknesses of NHANES, the secondary data source to be used by the design.

The NHANES is conducted by the National Center for Health Statistics (NCHS), Centers for Disease Control and Prevention (CDC), U.S. Public Health Service, to collect information about the health, nutritional status, and diet of people in the United States (National Center for Health Statistics 1994). The NHANES is unique in that it combines a home interview with health and nutrition assessments conducted in a mobile examination center.

The NHANES covers a representative cross-sectional sample of the U.S. civilian, noninstitutionalized population. The sample design is a multistage, complex, stratified survey design of individuals living in households. Previous NHANES surveys were conducted on a

periodic basis, but, beginning in 1999, the sample design was transformed into an annual nationally representative sample (National Center for Health Statistics NHANES web site, 2001).

Continual data collection increases the availability of timely data for health and nutrition policymaking and provides flexibility in changing the sample design and the survey content. These design features offer an opportunity for future funding to increase the sample size of school-aged children or to add content to the NHANES to study the relationship between SBP and learning.

The current survey design includes participants of all ages and racial/ethnic groups. The first three years of the survey (1999–2001) oversample of adolescents, blacks, and Mexican Americans. In 2000, oversampling of low-income white people was added to the design requirements in order to produce reliable estimates for the total population with incomes at or below 130 percent of the poverty line. Oversampling of the low-income population will provide a larger sample of children who are eligible for or participate in the SBP.

Data are collected on approximately 5,000 people from 15 primary sampling units (PSUs) per year. Although each year provides a national sample, data can also be aggregated across survey years to provide reliable national estimates for more detailed age, gender, and racial/ethnic groups. Table III.2 shows the total sample sizes expected for school-aged children by race/ethnicity for three survey years: (1) 2000, (2) 2001, and (3) 2002. The design calls for equal numbers of boys and girls in each age group.

Table III.2—Expected sample sizes for NHANES in 2000, 2001, and 2002

RACE/ETHNICITY	6–11 years	12–15 years	16–19 years	6–19 years
Non-Hispanic White/other	176	176	176	528
Non-Hispanic black	176	183	183	542
Mexican-American	176	190	190	556
Total for one year	528	549	549	1,626
Total for three years	1,584	1,647	1,647	4,878

Future decisions about the sample design requirements and annual sample sizes will depend on federal data needs for national data for specific populations groups. The potential integration of NHANES and the Continuing Survey of Food Intakes by Individuals (CSFII) may change the annual sample size of people for whom sociodemographic and dietary intake data are available.

However, the exact nature of the sample design and of the oversampling by age, socioeconomic, and race/ethnicity for survey years beyond 2005 is unknown at this time. Given uncertainty about the post-2005 period, our discussion of the NHANES design is based on currently known sample sizes and sample design.

The current survey sample includes about 1,626 school-aged children each year. Because the data are nationally representative, the information they provide on the relationship between SBP participation and learning is generalizable to all school-aged children nationally. The cross-sectional nature of the data means that key outcomes are measured at a given point in time in the school year. Thus, the mean values of key outcomes are collected at the same time and cover the same time period as the information on SBP participation.

There is possible selection bias related to the availability of the SBP in schools, parents' knowledge about the availability of SBP, and parents' decisions about their children's participation in the program. Accounting for SBP participation decisions in the analysis is important, because there is a risk that nonrandom selection into the sample of SBP participants will bias estimates of the impact of SBP participation on learning.

3. Data collection

NHANES measures much of the same family and school information that other national surveys, such as the ECLS-K, measure; however, it does not measure learning or other school-related measures. Nevertheless, the NHANES is unique in that it assesses the intermediate outcomes that may affect learning.

Nutrition and health status are potentially affected by SBP participation, but they in turn may also affect learning outcomes. Section 3.a describes the current NHANES data collection plans. Section 3.b describes possible supplemental data collection that might be conducted to expand the usefulness of the information for studying learning outcomes.

a. Current data collection plans

This section summarizes the current data collection plans for NHANES 1999-2002, by general topic area. Table III.3 contains a more detailed list of variables currently collected.

- ***Family characteristics.*** Information on the family's income, food assistance program participation, and use of emergency feeding assistance in the past year is available to characterize the food security and socioeconomic status of the child's family. Information on parents' education, health insurance coverage, and sources of medical care is also collected.

- ***School meals and behavior.*** Information on whether the child's school offers the SBP or the National School Lunch Program (NSLP), the child's frequency of participation in the SBP and NSLP, and whether the child receives free or reduced-price school meals is available from the parent interview. Additional information on the child's grade level, attendance, suspensions, expulsions, and skipped grades is also collected in the parent interview.

- ***Dietary intake and behavior.*** Dietary intake is assessed using 24-hour dietary recall methodology and additional interview questions about dietary habits. At least one 24-hour recall is collected per person, with a second day collected on a subsample. The 24-hour dietary recall provides information on whether breakfast was consumed, the time and source

of breakfast(s), and the foods and amounts consumed at breakfast, as well as the total day's intake. Total nutrient intake is estimated using information collected about dietary supplement use, discretionary salt use, and water intake. Dietary habits, such as the frequency of eating away from home, are also collected.

Table III.3—Current variables in NHANES pertinent to school breakfast and learning

Family Characteristics
Head of household
Education level
Country of birth
Employment
Occupation
Health insurance coverage
Source of medical care
Number of families in household
Family members' relationships
Housing characteristics
Income, past 12 months
Food security instrument (18-item instrument used in Current Population Survey), past 12 months
Emergency Feeding, past 12 Months
Food Program Participation (Family-Level)
WIC: Number of months in past 12 months and current month
Food Stamp Program: Number of months in past 12 months and current month
Individual Characteristics
Age
Race/ethnicity
Country of birth
School Meals and Behavior
National School Lunch Program: Does school offer? Number of times per week has free/reduced-price/full-price lunch
School Breakfast Program: Does school offer? Number of times per week has free/reduced Price/full-price breakfast
Grades at school

Suspensions and expulsions
Grade level and skipped grades
Attendance (missed days due to illness)

Dietary Intake and Behavior

One in-person 24-hour dietary recall (second day on subsample)
Use of dietary supplements
Additional food security questions
Number of times the child eats away from home per week
Salt usage

Blood Determinations Related to Nutrition or Health Status

Iron status (hemoglobin, hematocrit, serum iron, tibc, serum ferritin, transferrin saturation)
Serum and RBC folate
Serum vitamin E
Serum vitamin A and retinyl esters
Serum carotenoids
Plasma homocysteine
Methyl malonic acid
Serum vitamin B_{12}
Cotinine (passive smoke or cigarette exposure)
Lead

Health-Related Behaviors

Number of times per week child plays or exercises enough to sweat or breathe hard
Number of hours of television/video watching yesterday
Number of hours of computer use yesterday
Smoking
Alcohol and drug use

Health Interview Data

Reported medical conditions
Whether mother smoked during pregnancy
Birthweight and whether full-term
Vision problems; need for corrective lenses

Health Examination Data

Height, weight, anthropometric measures (for assessing growth, overweight)
Bioelectrical impedance analysis, body composition (ages 8 and older)
Dental exam for caries, periodontal disease
Blood pressure (ages 8 and older)

- ***Blood determinations related to nutrition or health status.*** These variables provide information on intermediate outcomes relating to short-term and long-term nutritional status and health. Iron, B-vitamins, and lead are related to brain development while the nutritional biochemistries have the potential to serve as biomarkers of dietary intake and status. Increased dietary quality or dietary status would be expected to result in improved nutritional status.

- ***Health-related behaviors.*** Health behaviors assessed for younger and older children include physical activity levels and time spent on sedentary activities, such as watching television, playing video games, and using personal computers. Risk behaviors include smoking for children aged 8 years and older and alcohol and drug use for those aged 12 years and older. Risk behavior information is collected during private, self-administered computer-assisted personal interviewing (CAPI) in the mobile examination center.

- ***Nutritional status.*** The NHANES provides the most comprehensive picture of nutritional status available on a national sample of school children. Precise anthropometric measurements, such as height and weight, are used to assess growth and overweight in relation to the revised CDC growth charts (Kuczmarski et al., 2000). Blood and urinary measurements provide an assessment of vitamin and mineral status for a wide variety of nutrients, such as B vitamins and iron. Iron status is of particular interest, because iron deficiency is related to developmental and behavioral disturbances that may affect mental performance and learning in young children (Centers for Disease Control and Prevention, 1998).

- ***Health status.*** General health status measures, such as physical fitness, blood pressure, and respiratory disease, provide an overall picture of the child's health and readiness to learn. Other health components included in the NHANES related to a study of breakfast and learning include visual acuity and hearing problems, which may affect classroom learning, and environmental exposures, such as to lead. Frequent health problems and illnesses may lead to more missed days of school and fewer opportunities to learn. Elevated levels of lead in the blood can be associated with iron deficiency anemia and are higher among low-income children (Centers for Disease Control and Prevention 1998). Variables that relate to the child's prenatal environment, such as low birth weight or exposure to smoke, have been shown to relate to growth and development and are collected in the parent interview.

b. Supplemental data collection

The primary weakness of the NHANES for a study of SBP and learning is that there are no current plans to collect cognitive measures or achievement data for school-aged children. Another weakness

is that the current measure of SBP participation may be somewhat imprecise. We recommend the following supplemental data collection to compensate for these weaknesses in the NHANES design:

- Additional interview questions about school breakfast participation and usual breakfast patterns
- Cognitive and behavioral testing
- Achievement tests
- Administrative school records

Because parents may be unaware of their child's school breakfast consumption or the availability of SBP at their child's school, we recommend that additional information be collected to verify school breakfast participation. During private interviews, children could be asked additional questions about their frequency of SBP participation during the school year, and about their breakfast eating patterns at home and at school.

Cognitive and behavioral testing to evaluate short-term cognition and academic performance (achievement) could be administered in the mobile examination center during private interviews with children. Subtests of the Wechsler Intelligence Scale for Children, Revised (WISC-R) and the reading and arithmetic sections of the Wide Range Achievement Test, Revised (WRAT-R) were successfully administered in the third NHANES (1988–1994) to 6- to 16-year-old children (National Center for Health Statistics, 1994; and Kramer et al., 1995).
Two subtests of the WISC-R, the Block Design and the Digit Span, were administered to serve as indicators of cognitive functioning. The WRAT-R was used to assess academic performance in reading and mathematics. Other behavioral and cognitive tests relating to school performance and learning could also be considered for supplemental data collection. Decisions about adding survey content will depend on the availability of survey time and the usefulness of linking new components with other interview and health examination components in the survey.

Current NHANES plans do not include the collection of information directly from schools. Because SBP participation and test scores are important outcomes in this study, we recommend the collection of school-level information for these two variables. Schools may keep student records of SBP participation over the course of the year. They also have records of students' test scores that we would have to have to measure the effect of SBP on learning.

We would have to assess the students' level of academic achievement at the point in time of their general data collection, and we also would need test scores for a previous year to assess the students' gain in achievement. It would be necessary to obtain parents' consent to obtain the school records of surveyed students. School records also could provide information on the students' attendance, tardiness, nurse visits, disciplinary events, and grades. School records data are an independent source of relevant information on school behaviors that would supplement the NHANES design.

We considered using the longitudinal followup of a cohort of NHANES children but judged this option too costly to recommend. In a longitudinal design, information on children's SBP participation and related variables, such as school attendance, dietary intake, nutritional status, and health, would be assessed at baseline in the annual NHANES.

With parental consent, a sample of children could be interviewed and examined one to several years later to assess SBP participation, cognitive performance, and academic performance. School records could also be collected to obtain information on academic performance and achievement tests. Learning outcomes, such as academic performance and achievement tests, could be compared between SBP participants and nonparticipants at baseline and at followup.

4. Measurement of key characteristics and outcomes

Participation in the SBP is a key variable for the study of learning and school breakfast. The NHANES includes "usual" participation in the SBP during the school year (that is, the number of times per week the child receives school breakfast, and whether it is free or at a reduced price), based on the parent interview. However, because participation information could be subject to reporting error by the parent, we recommend directly observing students' SBP participation.

Another crosscheck to the parent's report of the child's SBP participation is the 24-hour dietary recall, which captures where breakfast was consumed. During this session, most children aged 6 to 11 years report their dietary intake with the assistance of a parent or guardian. Children 12 and older report their dietary intake alone. Information collected in the dietary recall could be used to identify whether breakfast was consumed in school for children interviewed from Tuesday through Saturday (to reflect Monday through Friday intakes).

One weakness of this approach is that this information would not be available for all children; some children would be interviewed on Sunday and Monday (about Saturday and Sunday intakes), and some would be interviewed during the summer, when school is out of session. Therefore, school day dietary intakes would be unavailable for about 25 to 30 percent of the total sample of school-aged children. However, for children who are not given 24-hour dietary recall interviews, we could compare SBP participation on a sample day with the frequency of weekly SBP participation reported in the parent interview.

The data collected and the desired analysis create various alternative methods of measuring SBP participation. As described earlier, one option would be to define "usual participants" as students who eat a school breakfast on at least 3 of 5 school days. Alternatively, participants and nonparticipants could be distinguished on the basis of whether or not they usually eat a school breakfast five times per week.

Dietary intake is assessed through 24-hour dietary recalls. The current NHANES design calls for one 24-hour recall per child and a second 24-hour recall on a subsample. The second day's intake on a subsample provides information to adjust nutrient intake distributions using statistical software that takes into consideration the day of the week and within- and between-person variability.

Adjusted distributions of nutrient intake could be used to estimate the proportion of SBP participants and nonparticipants who meet dietary recommendations and dietary adequacy. This approach provides information for comparing group dietary data but does not provide a better measure of individual students' usual dietary intake for use in regression analysis.

The 24-hour dietary recall provides information on current dietary intake. Longer-term dietary status is reflected in nutritional biochemical assessments of blood and urine, hematologic determinations, and anthropometric measurements. Nutrition and health outcomes, such as growth, overweight, and iron deficiency anemia, may relate to children's readiness to learn in school.

Other lifestyle and risk behaviors, such as alcohol and drug use, provide information that may relate to poor school performance, missed days of school, suspensions, and expulsions. This type of behavioral information collected in NHANES can be used as control variables when comparing learning outcomes in SBP participants and nonparticipants.

5. Analysis plans

The primary analysis in the NHANES design compares mean differences in dietary, nutrition, health, and (possibly) learning outcomes among SBP participants, nonparticipants, and children who do not eat breakfast. Descriptive analysis would include, but would not be limited to, comparisons of the following outcomes between SBP participants and nonparticipants or nonbreakfast eaters:

- Mean nutrient and food group intakes for breakfast and for the day
- Mean dietary quality and variety score, assessed by the Healthy Eating Index[33]
- The proportion meeting current dietary recommendations or dietary requirements for nutrient adequacy, as defined by the Recommended Dietary Allowances, the Dietary Reference Intakes, and the *Dietary Guidelines for Americans* (U.S. Department of Agriculture and U.S. Department of Health and Human Services, 2000)[34]
- The proportion defined as underweight, at a healthy weight, or overweight based on height and weight measurements and the revised CDC growth charts (Kuczmarski et al., 2000)
- The proportion with iron deficiency anemia or low levels of specific vitamins, based on biochemical test results
- Mean academic test scores and grades (conditional on data availability)

[33]The Healthy Eating Index is a summary measure of diet quality, developed by Kennedy et al. (1995). It measures individuals' food group intake, compliance with dietary guidelines, and assesses the variety of individuals' diets. To the extent that the SBP influences any of these areas of children's diets, then it would also influence the Healthy Eating Index.

[34]Software from Iowa State University can be used to adjust nutrient intake distributions. The program considers the within- and between-person variability in intake and the skewness of nutrient intake distributions (Nusser et al. 1996). However, as described above, regression-adjusted comparisons of the proportion of the groups that meet dietary recommendations cannot be made, because the procedure does not generate estimates of individuals' usual dietary intakes.

- Mean number of missed days of school, tardiness, suspensions, expulsions, and skipped grades (conditional on data availability)

- Mean cognitive test scores (such as from the WISC-R and WRAT-R) and mean scores on composite measures of behavior (conditional on data availability).

To estimate the effects of SBP participation, we would use regression analysis to compare the outcomes of participants and nonparticipants, after controlling for relevant factors that can be measured. Important factors to account for in comparing academic and learning outcomes include food insecurity and poor nutritional status, and factors relating to prenatal nutrition, such as low birth weight and exposure to cigarette smoke. Environmental exposures, such as to lead, revealed by elevated blood lead levels, should also be considered in interpreting the results of cognitive tests and academic performance.

In addition, we would have to consider the important issue of selection bias. We could use several approaches, including IV models or switching regression models to help account for this. These approaches are described in detail in Chapter IV, in the discussion of the ECLS-K design, but also are applicable to the NHANES design.

6. Statistical power

The calculations of the statistical power of the NHANES design focus on the comparison of SBP participation and nonparticipation. We expect about 75 percent of the NHANES sample to attend SBP schools, and the expected total sample size of school-aged children is about 1,600 per year; thus, we expect a sample of about 1,200 children attending SBP schools per year. Using this estimate, we calculated the minimum detectable differences (MDDs) on achievement based on the following assumptions:

- 30 percent SBP participation rate
- No selection bias

- Available prior-year test scores

Average design effect of 1.3 based on NHANES III information or estimated design effect of 2.5 for NHANES 1999-2001[35].

We estimated that a one percentile change in an achievement test score is equivalent to 0.025 of a standard deviation. Therefore, collecting data for one year for 1,200 children (assuming a design effect of 1.3) provides the power to detect a difference of 0.155 standard deviations, or six percentiles, in the achievement test score (see Table III.4). Three years of data collection would

[35]We selected a design effect of 1.3 as a lower bound based on the average design effect in NHANES III. However, the design effect in the current NHANES is likely to be higher. We selected a design effect of 2.5 as a more realistic estimate of the average design effect.

detect a difference of 0.090 standard deviations, or 3.6 percentiles, and six years would detect 0.063 standard deviations, or 2 percentiles.

Table III.4—Minimum detectable differences on achievement test scores

Years of data collection	Sample size	Test score change (SD) for DEFF = 1.3	Test score percentile change for DEFF = 1.3	Test score change (SD) for DEFF = 2.5	Test score percentile change for DEFF = 2.5
1	1,200	0.156	6.2	0.215	8.6
2	2,400	0.11	4.4	0.153	6.1
3	3,600	0.09	3.6	0.125	5.0
4	4,800	0.078	3.1	0.108	4.3
5	6,000	0.07	2.8	0.097	3.9
6	7,200	0.064	2.5	0.088	3.5

MDDs would be higher with a design effect of 2.5. In particular, the MDD would be 0.215 standard deviations with one year of data, 0.125 with three years, and 0.088 with six years.

Thus, with somewhat optimistic assumptions, including the assumption of no selection bias, achieving the target MDD of 0.10 standard deviations would require a minimum of three years of data. With less optimistic assumptions about NHANES design effects, five years of data would be required. Some degree of selection bias likely exists, so it may be necessary to aggregate more years of data collection, or to expand the currently planned annual sample sizes to achieve sufficient statistical power.

7. Design costs

The two cost components of the NHANES-based design are supplemental data collection costs and analysis and reporting costs. Unlike the ECLS-K-based design, the supplemental data collection activities in this design are a critical part of the research effort, so we have generated a cost estimate for the supplemental data collection costs. In generating this estimate, we have made the following assumptions:

- Three years of supplemental data collection would be conducted for NHANES sample members between 6 and 16 years old

- One additional minute of interviewing time would be added to the NHANES parent interview to collect information on usual breakfast patterns and children's school breakfast participation patterns

- Twenty minutes of examination time would be added for trained interviewers to conduct cognitive tests and academic achievement tests, such as the WISC-R and WRAT-R

- Individual school records, including information on academic achievement test scores, would be collected from the sample members' school.

We estimate that the cost of these supplemental data collection activities would be in the range of $2.9 to $3.0 million for a three year period.

The remaining evaluation costs would cover analysis and reporting of the results. These costs would be $1.0 to $1.1 million. Thus, total evaluation costs for the NHANES-based design would fall in the range of $3.9 to $4.1 million.

8. Design summary: strengths and weaknesses

The analytic approach in using data from the NHANES design is to describe and compare mean differences in dietary, nutritional, health, and learning outcomes among SBP participants, SBP nonparticipants, and students who do not eat breakfast. To estimate the effects of SBP participation, we would use regression analysis to compare the outcomes of participants and nonparticipants after controlling for all measurable relevant factors (for example, prenatal exposure to smoke, low birthweight, iron deficiency anemia, and elevated blood lead levels)

One weakness is possible selection bias due to parents' selection of SBP participation. The approach to account for potential selection bias in the NHANES design is similar to that described in the ECLS-K design—using IV models or switching regression models.

The NHANES design offers the advantage of an existing national survey of school-aged children with comprehensive information on family background, SBP participation, dietary intake, nutritional status, and health. The design's primary strength is that it captures many of the important domains needed to link SBP participation and learning (for example, dietary intake and short-term and long-term nutritional status and health.)

The survey also provides a framework for supplemental data collection on short-term cognitive function, school behavior, and academic performance. These domains are needed to fully study the relationship between SBP participation and learning.

Another advantage of the NHANES design is that low-income children are oversampled, which increases the potential sample size of SBP participants available for study. A national sample of about 1,600 school-aged children is drawn from 15 PSUs each year. However, fairly large design effects may occur with few PSUs per year, and the annual sample size is probably too small to support detecting differences in test scores of SBP participants and nonparticipants.

We estimate that, under optimistic assumptions, it would require three years of academic test score data for sufficient power to detect a change in test score of four percentiles (the equivalent of 0.10 standard deviation). Under less optimistic assumptions, more years of data would be required.

Although the NHANES provides a rich database on SBP participation, diet, nutrition, and health variables, the absence of plans to collect information on cognitive functioning and academic performance is a major design weakness. Supplemental data collection would therefore be required.

F. Assessing the Alternative Designs

This chapter presents four alternative designs for estimating the impact of the SBP on learning. Because they use different methodologies and are based on different types of data, each has particular strengths and weaknesses. Given the infeasibility of conducting all four designs, this section identifies the most feasible one. The recommendation has been made with the knowledge that the USBP design already has been funded and is currently being implemented. Thus, the chapter considers whether implementing an alternative design (in addition to the USBP design) would be worthwhile and, if so, which should be implemented. The chosen design—a modified version of the ECLS-K design—is developed more fully in Chapter IV.

1. Summary of alternative designs

The four alternative designs described in this report are summarized in Tables III.5 and III.6. The first table describes the key features of each design. The second one presents the strengths and weaknesses of each design, as well as each one's estimated costs. A key distinction among the four alternatives is the design type; two designs use an experimental approach, and two use a nonexperimental approach.

a. Experimental designs

The USBP design and the SBP applicant design use an experimental approach to examine the relationship between the SBP and learning. In particular, the USBP design involves the random assignment of schools into a treatment group that serves free USBP breakfasts and a control group that serves regular SBP breakfasts. Thus, this design estimates the effect of being in a school that offers the USBP as opposed to being in a school that offers the regular SBP.

The SBP applicant design calls for the random assignment of classrooms in participating schools (that are applying to become SBP schools for the first time) into a treatment group offering USBP breakfasts and a control group offering no breakfast program. This design estimates the effect of being in a classroom that offers free breakfasts as opposed to being in a classroom with no breakfast program.

Table III.5—Summary of evaluation design options

	USBP design	SBP applicant design	ECLS-K design	NHANES design
Design type	Experimental	Experimental	Nonexperimental	Nonexperimental
Intervention and counterfactual	Availability of USBP versus regular SBP	Availability of USBP versus no breakfast program	Participation versus nonparticipation in the regular SBP	Participation versus nonparticipation in the regular SBP
Basic design approach	Experimental, randomly assign schools	Experimental, randomly assign classrooms	Nonexperimental, compare SBP participants and nonparticipants	Nonexperimental, compare SBP participants and nonparticipants
Coverage	Six school districts, not nationally representative	Up to 100 schools, not nationally representative	Nationally representative	Nationally representative
Data collection	Primary	Primary	Secondary	Secondary
Sample	Elementary students in grades 1 through 5 in SY 2000-2001	Elementary students in grades 2 through 5 in SY 2001-2002	Students in kindergarten programs in fall 1998	School-aged children in years 2000, 2001, and 2002
Sample size	144 schools 4,320 students	100 schools 1,600 classroom 16,000 students	16,906 students[a] 866 schools	1,626 students per year 3 years
Key outcomes	USBP and SBP participation student achievement cognitive functioning attendance / tardiness teachers' evaluations dietary intake health status	USBP participation student achievement cognitive functioning attendance / tardiness teachers' evaluations health status	student achievement cognitive functioning social and emotional development attendance / tardiness	student achievement[b] dietary intake nutritional status health status
Statistical power (minimum detectable differences, measured as a percentage of a standard deviation)	16.0 percent	9.5 percent	5.3 percent[c]	9.0 percent[d]

NOTES:
[a] The number of students who continue in the sample to grade 5 may be lower due to sample attrition.
[b] Supplementary data collection is needed to acquire information on student achievement.
[c] This calculation assumes no selection bias. The minimum detectable effect with selection bias (assuming good instrumental variables) is 16 percent of a standard deviation.
[d] This calculation assumes three years of NHANES data, a design effect of 1.3, two years of test score data for each sample member, and no selection bias.

Table III.6—Strengths and weaknesses of evaluation design options

	USBP design	SBP applicant design	ECLS-K design	NHANES design
Strengths				
Design	Experimental design permits rigorous estimation of effect of USBP availability relative to regular SBP availability; 3-year study with longitudinal data	Experimental design permits rigorous estimation of effect of USBP availability relative to no breakfast program		
Implementation	Already funded and being implemented		Basic version of the design is straightforward to implement	
Sample			Nationally representative	Nationally representative
Measurement of SBP participation	Primary data collection methodology permits careful measurement of participation	Primary data collection methodology permits careful measurement of participation		
Outcomes	Full range of outcome measures to be examined	Large number of outcome measures (except for dietary intake) can be examined	Large number of outcome measures relating to academic achievement and cognitive functioning	Good measures of dietary intake, nutritional status, and health status
Weaknesses				
Design			Comparison group design potentially subject to selection bias	Comparison group design potentially subject to selection bias
Implementation		Getting schools to agree to implement random assignment of classrooms likely to be difficult		Supplemental data collection time consuming and potentially difficult
Measurement of SBP participation			Existing ECLS-K participation measures reported by proxy (usually parents) and limited to a single week during year	Existing NHANES participation measures potentially subject to error
Statistical power	Insufficient power to detect impacts on student achievement under experimental design; fallback position is to estimate effects on achievement using nonexperimental comparison group design	If a sufficient number of schools fail to agree to participate in the demonstration, design will have insufficient power to detect impacts on student achievement under experimental design; fall position is to estimate effects using nonexperimental comparison group design	If there is selection bias, design has insufficient power to detect effects of participation	If there is selection bias, design has insufficient power to detect effects of participation
Estimated design cost				
Demonstration	$6.6 to $6.9 million	$0.9 to $1.0 million	$0	$0
Evaluation	$6.4 million	$10.7 to $11.3 million	$0.4 to $0.5 million	$3.9 to $4.1 million
Total	$13.0 to $13.3 million	$11.6 to $12.3 million	$0.4 to $0.5 million (plus supplemental data collection costs)	$3.9 to $4.1 million

[a] Supplementary data collection is needed to acquire information on student achievement.
[b] This calculation assumes no selection bias. The minimum detectable effect with selection bias (assuming good instrumental variables) is 16 percent of a standard deviation.
[c] This calculation assumes three years of NHANES data, a design effect of 1.3, two years of test score data for each sample member, and no selection bias.

The key strength of the two experimental designs is that they produce unbiased estimates of the impacts of the intervention versus the counterfactual. For example, because random assignment of schools in the USBP design ensures that students in treatment schools and students in control schools differ systematically only with respect to their access to the USBP, resulting systematic differences between the groups in outcomes related to learning likely arise from access to the USBP. Given sufficiently large samples, experimental designs have the potential to yield the most rigorous estimates possible of the effects of interventions.

Another advantage of the USBP and SBP applicant designs is that they include primary data collection. They can therefore include a rich set of control and outcome measures specifically tailored for a study of breakfast and learning. The USBP design in particular offers a broad range of outcome measures ranging from academic achievement and cognitive functioning to health and dietary intake. The SBP applicant design also has the potential to include a broad range of outcome measures, with the exception only of dietary intake data.

A unique advantage of the USBP design is that it has been funded and is currently being implemented. Thus, the demonstration and evaluation designs have been fully developed and adequate resources should be available to carry them out. Results from the USBP evaluation will come within a relatively short period of time, given its size.[36]

The two experimental designs do have weaknesses. Although they are experimental, they are not specifically set up to estimate the effects of SBP participation on student outcomes by randomly assigning individuals to participant and nonparticipant categories. Instead, they measure the effects of program *availability.* If they have sufficient statistical power, estimates from the experimental designs of the effect of program availability could be used to generate *indirect* estimates of the effects of participation. Otherwise, they must rely on a nonexperimental approach to estimating these effects.

Unfortunately, the USBP design is unlikely to generate sufficient statistical power to detect the likely effects of USBP availability on test scores.[37] Even with a successful preimplementation survey, the minimum detectable impact on test scores is about 16 percent of a standard deviation, whereas the expected size of the effect is more likely to be closer to 10 percent. Thus, the USBP design will likely have to rely on nonexperimental methods to estimate the effect of breakfast program participation on student achievement in school.

In theory, the SBP applicant design can attain sufficient statistical power to detect effect sizes of participation of 10 percent of a standard deviation. However, this design is likely to be difficult to implement—a major weakness—implying that the target sample sizes may be hard to obtain. In particular, it will be challenging to identify enough SBP applicant schools that will agree to delay full SBP

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[36]According to the schedule described in the USBP Evaluation Request for Proposals released by FNS in February 2000, the final draft of the first USBP report covering year 1 of the implementation of the program will be submitted to FNS in June 2002.

[37]The design is likely to have sufficient statistical power to detect estimates of the effects of USBP availability on other outcomes, such as program participation rates and dietary intake (Ponza et al., 1999).

implementation and face the ethical questions involved in randomly assigning classrooms to breakfast program and nonbreakfast program groups.

Finally, implementing either of these experimental designs would be quite costly. When the costs of implementing the demonstration and the costs of conducting the evaluation are summed, each has a cost of roughly $12 to $13 million. Given that the USBP study has been funded and the demonstration and evaluation are under way, the high cost of the SBP applicant design (to address a similar set of questions) becomes an even greater drawback.

b. Nonexperimental designs

The ECLS-K and NHANES designs use a nonexperimental approach to estimating the impact of participation in the SBP (versus the counterfactual of nonparticipation) on learning. Each is based on analysis of a secondary data source that includes information on whether or not students eat school breakfasts and on student outcomes.

One of their strengths is that they are based on nationally representative datasets. In contrast to most previous research and to the proposed experimental designs, results from these analyses would be generalizable to students nationally. In addition, the ECLS-K dataset is very large, with information on about 17,000 students in just under 900 schools.

This sample size implies that the ECLS-K design has a high level of statistical power (given the assumption of no selection bias). The NHANES dataset is smaller; it is expected to provide information every year on more than 1,600 students aged 6 through 18 and will be conducted annually.

The wealth of information available in the ECLS-K and NHANES datasets is another strength of these nonexperimental designs. However, each dataset is limited in an important respect. The ECLS-K dataset includes a rich variety of outcome measures on students' academic performance and background characteristics, but it does not provide information on their dietary intakes or nutritional status. The NHANES dataset has information on dietary intake and a wealth of information on nutritional and health status but lacks information on students' achievement or performance.

Addressing the data limitations of the ECLS-K and NHANES datasets through supplementary data collection would be possible, although it would add substantially to the expense of these designs. As described in Chapter IV, given the expense and the fact that dietary intake information is not central to the objective of the study, we do not recommend supplementing the ECLS-K data with dietary intake information from ECLS-K sample members.

However, we do recommend supplemental ECLS-K data collection to obtain improved measures of SBP participation, as well as relevant student characteristics. For the NHANES design, we recommend supplemental data collection of student achievement and/or cognitive performance measures, including the collection of school records data.

The nonexperimental nature of the ECLS-K and NHANES designs is an important limitation of these approaches to studying the effects of the SBP. Because they are based on observations of students' SBP participation statuses rather than on random assignment of students to participant and nonparticipant statuses, selection bias is possible in estimates of the effect of participation.

In particular, students (or their families) who decide that they should eat a school breakfast may differ in unmeasured ways from nonparticipants. If these unmeasured differences are related to the student outcomes of interest, selection bias will result when the standard regression methods are used to estimate the effects of participation on these outcomes. Although statistical methods, such as IVs, can address selection bias, they also have limitations. Thus, results from a nonexperimental study typically do not carry the same weight as results from a well-designed experimental study.

The costs of the two nonexperimental designs differ dramatically, primarily because of the extensive supplemental data collection necessary in the NHANES-based design. The ECLS-K-based design should cost $400,000 to $500,000, plus the supplemental data collection costs necessary to add questions to the ECLS-K survey instruments. The ECLS-K-based design is the lowest-cost option. Given that the NHANES-based design has many of the same strengths and weaknesses as the ECLS-K design, its cost of roughly $3 million decreases its attractiveness as an option.

2. Choosing the most feasible design

A key factor in the choice of the most feasible design is that the USDA is now implementing the USBP design. This suggests that its results and the results of another recommended design could be used together to give additional perspective on the relationship between the SBP and learning. Although the USBP design is strong, it does have limitations, and we believe pursuing an alternative design that addresses some of these limitations would yield more persuasive evidence on the SBP-learning relationship than would relying solely on the USBP design.

We rule out recommending the SBP applicant design for two main reasons. It is similar to the USBP design in that both use an experimental approach, and both examine the effects of an intervention defined as the availability of free school breakfasts. In addition, both designs call for similar types of data collection activities, variable measurement, and analytic approaches.

Furthermore, neither design is nationally representative, and there are questions about their ability of each to achieve sufficient statistical power. The SBP applicant design does not share the USBP design's strength of being a three-year longitudinal study, and the evaluation costs of the SBP applicant design are greater than the evaluation costs of the USBP design. Thus, the contributions of the SBP applicant design do not justify its costs.

In addition, the SBP applicant design is somewhat risky given its implementation challenges. The success of the approach relies on there being enough SBP applicant schools that serve elementary students and enroll a substantial number of low-income students. Its success also relies on convincing enough of these schools to delay full SBP implementation for a year while classrooms are randomly assigned to a treatment group and a control group.

Schools may not want to deny breakfasts to any students, and they may not want to face the challenges of serving free breakfasts in a limited number of classrooms one year and then implementing the regular SBP the next year.

If the SBP applicant design is not to be chosen as the most feasible, this leaves the two nonexperimental designs to consider. For the reasons we have discussed, we do not consider the NHANES design the most feasible of the two nonexperimental designs. This dataset has information on relatively small samples of school-aged children, so that a large number of years of data would have to be assembled before any analysis that would yield sufficiently precise estimates could be conducted.

More important, the current NHANES data collection plans do not include the collection of information on children's levels of achievement and cognitive performance. For the NHANES design to address the relationship of interest, relatively expensive supplemental data collection activities would have to be conducted.

We consider a modified version of the ECLS-K design to be the most feasible of the four alternatives. The ECLS-K design addresses at least two of the limitations of the USBP design: (1) it is nationally representative; and (2) its large sample size is likely to give its estimates sufficient statistical power, assuming that selection bias is a relatively minor problem.

Furthermore, its rich information on students' achievement and cognitive functioning addresses the key outcomes of interest. Although the ECLS-K design is nonexperimental, the fact that the USBP design will examine the relationship between the SBP and learning, and thus will provide a second methodological perspective on this issue, reduces the impact of any weaknesses.

We recognize that the ECLS-K design would not provide information on students' dietary intake and nutritional status. These outcomes are not necessary to estimate the impact of the SBP on learning, but they may be intermediate outcomes that mediate this impact. In this case, for SBP participation to influence students' learning, it must first influence what they eat and their overall nutritional status. Although the program could influence learning via other routes, this one appears to be at least as important as any other.

To address this limitation, we recommend a modified ECLS-K design in which the analysis of ECLS-K data (described in Chapter IV) is supplemented with analysis of data from the 1988-1994 NHANES III dataset. In addition to information on children's SBP participation, dietary intake, and health and nutrition status (as well as on most of the other variables listed in Table III.3), that dataset also includes information on their academic achievement and cognitive functioning.

In particular, two standardized tests were used to assess intellectual functioning and academic performance: (1) the WISC-R, and (2) the reading and arithmetic sections of the WRAT-R. The entire Wechsler scale can be used to assess children's IQs. Two subtests of the WISC-R, the Block Design and the Digit Span, were administered to serve as indicators of cognitive functioning. They were selected because they are the least culturally sensitive.[38]

[38]Findings for the NHANES III test data have been reported by Kramer et al. (1995). They found that lower income, minority status, and lower educational attainment were independently associated with poorer performance on some subtests. General health status, birth complications, and gender also were predictors of performance for some subtests. This analysis was conducted with data for 1988 through 1991 but did not include SBP participation or dietary or nutritional variables.

To more fully investigate the relationship between the SBP and learning, we recommend an expanded analysis of the full NHANES III dataset for 1988–1994, which would produce a sample size of more than 5,000 school-aged children. The analysis would focus on estimating the relationship between SBP participation and the outcomes of dietary intake and nutritional status. It would then estimate the relationships between dietary intake/nutritional status and achievement/cognitive functioning (as well as the relationship between SBP participation and achievement/cognition). Combined with the original ECLS-K design, this supplemental analysis should provide a fuller understanding of the relationship between the SBP and learning and the pathways through which this relationship arises.

Thus, the most-preferred design includes the following features:

- Use of a nonexperimental basic design approach
- Use of nationally representative data
- Analysis of ECLS-K data to estimate the relationship between SBP participation and student achievement/cognitive functioning
- Possible supplemental ECLS-K data collection to obtain improved information on SBP participation and potential identifying variables for IVs or propensity score models
- Analysis of NHANES III data to estimate the relationships among (1) SBP participation, (2) dietary intake/health and nutrition status, and (3) academic achievement/cognitive functioning.

This design is outlined in detail in the following chapter.

IV. ECLS-K-NHANES III Design

This chapter describes the design selected for studying how participation in the SBP influences learning. The study design, which is nonexperimental, incorporates two analyses. The primary analysis is based on data from the Early Childhood Longitudinal Study Kindergarten Cohort (ECLS-K), which tracks educational and related outcomes of students in the kindergarten class of 1998–1999 at various points through the fifth grade.

The outline of this analysis was sketched in the previous chapter and is described in detail in this one. The supplemental analysis is based on data from the third National Health and Nutrition Examination Survey (NHANES III), collected between 1988 and 1994. This NHANES dataset is *different* than the one considered in Chapter III, since it also contains cognitive and academic testing results, although it contains much of the same nutrition and health information.

NHANES III collected comprehensive information on school-aged children's school breakfast participation, dietary intakes, nutritional status, and health status (intermediate outcomes in the conceptual model shown in Figure II.1), and on their performance on standardized cognitive and academic tests (the main outcomes). A supplemental analysis of NHANES III data will provide a unique opportunity to fully examine the relationships between participation in the School Breakfast Program (SBP) and the intermediate outcomes that may ultimately be linked to the main outcomes of cognitive functioning and academic achievement in U.S. schoolchildren.

This chapter describes the full two-part design suggested for estimating the relationship between participation in the SBP and learning. Sections A through E present the proposed analysis of ECLS-K data introduced in Chapter III. Section F presents the proposed analysis of NHANES III data.

A. ECLS-K Nonexperimental Comparison Group Design

As described in Chapter III, the basic design we propose is a nonexperimental comparison group design. This approach involves comparing key outcomes in students who participate in the SBP with outcomes in students who do not eat a school breakfast. The design is primarily cross-sectional, with learning-related outcomes and SBP participation status measured at a given point in time. The ECLS-K is a longitudinal dataset, however, so the design calls for some longitudinal analysis.

We propose exploiting the longitudinal aspects of the ECLS-K data in at least two ways. First, rather than measuring students' level of cognitive functioning at a given point, the design calls for measuring the *growth* in students' cognitive functioning by contrasting their test scores at the most recent point of data collection with their scores at a previous point of data collection.[39]

We propose analyzing the growth in test scores because we recommend using a measure of SBP participation that refers only to the current school year, and we wish to measure only what has been

[39] As described in this chapter, rather than construct a "growth in test scores" measure and using it as a dependent variable, the analysis would use the current test score level as the dependent variable and would control for the previous test score level in a regression framework. In practice, this approach is similar to one that uses a "growth in test scores" dependent variable but is more flexible.

learned during that school year. A test score level reflects students' innate ability and learning throughout their lives, whereas the growth in test scores approaches our ideal of measuring what has been learned in the current school year.

Second, the longitudinal data include information on students' usual SBP participation status that covers more than one year. Thus, if students' level of cognitive functioning is used as a dependent variable, the definitions of SBP participants and nonparticipants could be based on participation (or nonparticipation) over multiple years prior to the measurement of the outcome. For example, when measuring growth in test scores between grades 1 and 3, students who participated in the SBP during both kindergarten and grade 1 could be compared with students who participated during only a single grade, or who did not participate at all.

B. Variables in the ECLS-K

The ECLS-K consists of a nationally representative sample of 16,906 students in kindergarten in 866 schools as of fall 1998. A wide variety of information about these children has been collected and included in this dataset. In this section, we describe in detail the key pieces of ECLS-K data to be used in the analysis.

1. Outcome variables

The ECLS-K includes a rich set of variables that measure three types of outcomes: (1) a student's cognitive development; (2) events and processes associated with learning, such as school attendance and tardiness; and (3) other aspects of a child's growth, including emotional, social, and physical growth.

a. Cognitive outcomes

Children's cognitive skills and knowledge are measured in three broad areas: (1) language and literacy skills (or reading), (2) mathematics, and (3) general knowledge. The ECLS-K uses a battery of assessments to measure these cognitive skills.[40]

The intent of the ECLS-K cognitive assessment battery is to measure children's common skills and knowledge. In particular, the battery assesses "typical and important elements of the curriculum with particular emphasis on content and process areas that are critical to growth and can be expected to reflect growth on the same scale over time" (West et al., 2000). Thus, the survey measures children's skill levels in a way that allows for comparisons over time.

Students' responses to the assessment battery in each of the three broad areas were converted into norm-referenced scores and criterion-referenced proficiency scores. The norm-referenced scores allow for the measurement of a child's performance against the norm of the performances of other children in the same cohort population. These scores were constructed as *t*-scores with a mean equal to 50 and a standard deviation of 10. The criterion-referenced scores evaluate children's performance in a given area on a specific set of skills thought to reflect cognitive development in that area.

The ECLS-K contains measures of children's basic literacy, such as recognizing printed words, identifying sounds, reading words, vocabulary, and reading comprehension. The criterion-referenced scores include the following five levels of reading proficiency (West et al., 2000):

[40] In addition to these direct tests, the ECLS-K includes a teacher survey in which teachers provide their assessments of sample members' cognitive skills and other outcomes.

- Level 1: Recognition of upper and lower case letters of the alphabet
- Levels 2 & 3: Phonologic sensitivity at the subword level (for example, knowledge of letter and sound relationships at the beginning and at the end of words)
- Level 4: Ability to read common words
- Level 5: Comprehension of written text

The battery of mathematical questions, as described by West et al. (2000), measures such skills as understanding the properties of numbers, mathematical operations, and problem solving; understanding patterns and relationships among numbers; formulating conjectures; and identifying solutions. The five proficiency levels by which the mathematics criterion-referenced scores measure students' skill levels are:

- Level 1: Ability to read numerals, recognize shapes, and count to 10
- Level 2: Ability to count beyond 10, sequence patterns, and use nonstandard units of length to compare objects
- Level 3: Ability to recognize simple number sequences, read two-digit numbers, identify the ordinal position of an object, and solve word problems
- Level 4: Ability to calculate sums up to 10 and to recognize relationships among numbers in sequence
- Level 5: Ability to solve problems using multiplication, division, and number patterns

The general knowledge that the ECLS-K questions assess includes factual information, such as information from history and the physical, earth, biological, and social sciences. The questions also test children on "the skills children need to establish relationships between and among objects, events, or people and to make inferences and to comprehend the implications of verbal and pictorial concepts" (West et al.,2000).

In a study of SBP participation on children's cognitive skills using the ECLS-K, the outcome measure would be the growth in children's test scores (or the change in test scores from one year to another). Because the survey would collect test scores for students included in kindergarten and grades 1, 3, and 5, and their scores would be normalized, we would be able to assess changes in children's cognitive skills relative to their peers from one year to another during the period of their early schooling. Measuring test score growth, rather than test score levels, reduces the variability of this outcome measure across students and allows for more precise estimation of the effect of SBP participation on cognitive skills.

b. School attendance and tardiness

Availability of the SBP may increase some students' school attendance. First, the offer of a school breakfast, especially a free one, may draw children to school for the day. Second, if eating school

breakfasts positively influences children's health, they may be less likely to miss school because of illness. Similarly, if eating school breakfast boosts cognitive skills, children may enjoy their classes more and feel less need to be absent from school.

School attendance rates can be constructed from the student transcript data schools provide for the ECLS-K. In measuring attendance, special care must be taken to ensure the comparability of the transcript data across schools. Often, such data as attendance rates are recorded by different schools in slightly different formats.

We believe the best measure of attendance is a measure of the number of days a student attends school as a percentage of the number of days the school has been open that year. In addition, to the extent that data on tardiness are available, the proportion of days on which children are late to school should be used as a measure potentially related to learning.

c. Emotional/social development

Although not the primary outcome of interest for this design, emotional/social development is important because it has direct implications for a child's well-being. It could easily be included in the analysis of outcomes. The ECLS-K assesses the emotional and social development of children at different grades. The assessments cover children's self-control, responsibility, ability to cooperate, and ability to avoid impulsive reactions and verbal and physical aggression. Parents' and teachers' responses are the primary sources of the assessments, at least during for children in kindergarten and first grade. To the extent that the measures are consistent across grades, it may be possible to estimate not only the impact of SBP participation on social competence at a particular grade, but also the impact of SBP participation on growth in social competence over time.

d. Physical development

Nutritious breakfasts may positively affect children's physical development. The ECLS-K measures this potentially important outcome in several ways. A key measure of children's physical development is their height and weight (and, hence, their body mass index, or BMI), which trained ECLS-K assessors measure at each data collection point. It will be important to estimate the effect of SBP participation on the change in children's height and weight (that is, their growth).

ECLS-K assessors also measure children's fine and gross motor skills. Fine motor skills include such activities as copying figures or manipulating blocks. Gross motor skills are measured by the ability to hop on one foot, walk backward in a line, and perform similar activities.

Children's general health and developmental difficulties may influence physical development. The ECLS-K collects information on general health status from the children's parents. Parents also report their children's activity levels and any developmental difficulties with vision, hearing, articulation, attention span, or coordination.

2. Participation in the SBP

The ECLS-K contains information on SBP participation collected at the student level and the school level. Most of the analysis we propose is based on child-level data, so we focus the following discussion on these data.

The number of days during the previous 5 school days that a student ate a school breakfast is a key piece of student-level participation information. However, there are limitations on the accuracy of the SBP weekly participation variable available in the ECLS-K. In particular, three types of errors have the potential to compromise the accuracy of this variable.

First, only parents who report that their child usually eats a school breakfast are asked the question on breakfast eating during the previous 5 school days. However, even though parents may have reported that their children do not "usually" eat a school breakfast, some of these children may have eaten one or more breakfasts during the previous week, and may do so regularly. The parents of these children would not be asked the question about the number of school breakfasts consumed during the previous week. Therefore, these children would be inaccurately coded as eating no school breakfasts during that 5-day period.

In the proposed design, the ECLS-K information on the number of days of breakfast eating during the previous 5 school days serves as a proxy for the number of days the child ate a school breakfast during a typical week of that school year. This can lead to a second type of proxy error. Having information on the "typical" number of days of SBP participation would be preferable, because we hypothesize that SBP participation over the full school year has a greater influence on student learning than does a single week of program participation.

One type of proxy error arises from using one week of participation as a proxy for participation in the usual week. For example, some children who usually eat a school breakfast every day may have eaten only a few school breakfasts during the previous 5 school days because of some unusual circumstance. Alternatively, children who usually eat school breakfasts only 2 or 3 days a week may have especially liked the food served during the previous week and eaten a school breakfast every day.

A third type of proxy error occurs because parents report their children's experiences.[41] Rather than eating breakfast at school, as the parents report in answer to the question, some children might play or do other things. Alternatively, parents may provide breakfast in the home because they do not realize that their children also receive a breakfast at school. Evidence from the 1994 to 1996 Continuing Survey of Food Intakes by Individuals (CSFII) suggests that parents overestimate their children's SBP participation.

In particular, a question in the CSFII about children's usual participation in the SBP obtained responses from parents suggesting a daily participation rate of 26 percent. According to administrative data from the Food and Nutrition Service (FNS), the actual SBP participation rate during 1994 to 1996 was 20 percent.

A definition of SBP participation that would minimize these errors would be a simple binary variable indicating whether the parent reports that a child usually eats a school breakfast. Although this variable would contain less information than would a variable on the number of days of participation per week, it probably would more accurately measure what it purports to measure. In addition, all parents whose children attend SBP schools would be asked the question.

Finally, although the variable would be subject to proxy error, we believe that parents are more likely to

[41] A different type of reporting error would have arisen if very young children had been asked the participation question. They might not have completely understood the concepts behind the question.

have general information on whether their children usually eat a school breakfast than they are to have more detailed information on the number of days of breakfast was eaten during the previous week.

An alternative definition of SBP participation would expand the scope of the binary variable. It would group parents' responses about participation into three categories: (1) children who do not usually eat breakfast, (2) children who eat breakfast somewhat regularly (one or two times the previous week), and (3) children who usually eat breakfast (three or more times the previous week).

This variable does not contain as much information as a variable giving the exact number of days of participation per week, and proxy error still might occur. Nevertheless, it would measure what it is supposed to measure. Furthermore, it makes it possible to differentiate between children who eat numerous breakfasts per week and those who eat only a few. Finally, having two alternative measures of SBP participation would provide an opportunity to test which measure is more accurate.

The more general SBP participation variable is likely to be more accurate than the more detailed variable, but it also contains less information. We place more importance on the accuracy of the participation data than on obtaining more information, however, so the ECLS-K design calls for the use of the binary participation variable throughout most of the analysis. Because it also would be important to test for the robustness of the results with respect to alternative definitions of SBP participation, the design also calls for estimating alternative versions of the key models that include the SBP participation variable.[42]

For any analysis of between-school differences in students' average learning, researchers would have to create a variable or variables measuring schoolwide SBP participation. Measuring schoolwide participation is somewhat difficult using the ECLS-K. The ECLS-K asks school principals how many students are eligible for free school breakfasts, and how many receive free breakfasts.

Principals are not asked how many students are eligible for reduced-price breakfasts, nor are they asked how many receive reduced-price breakfasts or pay the full price for breakfast.[43] As an alternative to this incomplete information on SBP participation at the school level, it is possible to construct school-level participation variables from student-level data.

There are likely to be approximately 20 students (and 4 or 5 SBP participants) per school in the ECLS-K, so it should be possible to base the school-level participation rate on the participation rate of the sampled students at that school. This estimate is inefficient relative to a percentage reported by the principal (which would be based on all students in the school), but it is unbiased because it includes all SBP participants (those paying a reduced price, those paying full price, and those receiving free breakfasts).

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[42] Of course, any school breakfast participation variable will be affected by the degree of parental accuracy in reporting, which is unknown at this point.

[43] CFSII data indicate that 77 percent of school breakfasts were free, with the remaining 23 percent offered at reduced or full price (Gleason and Suitor, 2001).

3. Background characteristics of students and schools

The following text and tables describe the school-level and student-level background characteristics we believe are most relevant for the proposed analysis of learning outcomes. The discussion is based on the research described in Chapter III and on additional hypotheses about the factors potentially related to SBP participation.

a. Student-level background characteristics

The variation in student learning outcomes in a school depends on the characteristics of individual children and their families (and schools). Table IV.1 lists student background characteristics available in the ECLS-K that might affect learning outcomes, and that could be included in the multivariate analyses specified under this design. (Specific variables relating to these characteristics are included in Appendix B.) These characteristics include:

- Child's prior levels of learning (including prior test scores)
- Child's age, sex, and race/ethnicity
- Child's ESL (English as a second language) and disability status
- Number of siblings in the household and the child's relative birth order
- Whether the child is a member of a two-parent household
- Parents' education and employment
- Family income and food security
- Participation in federal nutrition programs (the Food Stamp Program [FSP], SBP, and National School Lunch Program [NSLP])

Table IV.I—Student background characteristics in the ECLS-K

VARIABLE	SOURCE	COMMENTS
Characteristics Affecting Learning		
Prior level of learning	Assessment data, teacher survey, transcripts	Captures starting point for growth in cognitive, social, and emotional skills
Age of child	Parent survey	Measured in months at time of assessment
Sex of child	Parent survey	
Race/ethnicity of child	Parent survey	
ESL status of child	Parent survey	

Disability status of child	Parent survey	
Number of siblings	Parent survey	
Indicator for youngest child	Parent survey	Captures birth-order effects
Indicator for middle child	Parent survey	Captures birth-order effects
Indicator for eldest child	Parent survey	Captures birth-order effects
Two-parent family	Parent survey	
Parental education	Parent survey	
Hours parent(s) work	Parent survey	
Family income	Parent survey	Measured relative to poverty level, including indicators of eligibility for free and reduced-price meals at school
Family food security	Parent survey	Based on a combination of questions
FSP participation	Parent survey	Assumed exogenous
SBP participation	Parent survey	Possibly endogenous
NSLP participation	Parent survey	Possibly exogenous; measured similarly to breakfast participation
Additional Characteristics Potentially Affecting SBP Participation		
Parents' knowledge of SBP	Parent survey, principal survey	Lack of awareness may reduce likelihood of SBP participation
Child's distance from school	Parent survey	Increased distance may reduce SBP participation

ECLS-K = Early Childhood Longitudinal Study, Kindergarten Cohort; ESL = English as a Second Language; FSP = Food Stamp Program; NSLP = National School Lunch Program; SBP = School Breakfast Program.

The ECLS-K also contains information on characteristics that may influence whether the child eats a school breakfast, but that do not influence learning-related outcomes. Variables representing these characteristics, also called instrumental variables or identifying variables, are highly important ones in estimating models of the influence of SBP participation on learning that account for selection bias (that is, bias resulting from unobserved differences between participants and nonparticipants).

The distance between the child's home and school, reported by parents, is a possible instrumental variable in the ECLS-K. Distance from school indicates the relative convenience of sending a child to school to receive a breakfast compared with having no breakfast or eating breakfast only at home. Although distance from school is likely to affect SBP participation, it is less likely to directly influence outcomes related to learning.

Another potential instrumental variable is parents' knowledge of the SBP (that is, awareness of the availability of school breakfasts), which is likely to affect SBP participation but not learning outcomes (after accounting for parents' education levels and the language spoken at home).[44] The quality of this

[44] Parents' knowledge of the SBP may be a weak instrumental variable because parents are informed at the beginning of the school year whether their child's school offers the SBP. Because most parents therefore are likely to know about the program, there will be little variation

instrumental variable depends on whether it is independent of unobserved factors affecting student learning, or whether it is correlated with them. With more than one potential instrumental variable, specification tests, such as those proposed by Wooldridge (1990) or Hausman (1983) could be used to examine whether questionable instrumental variables should be used for estimation.

Although additional information on parents' morning work schedules may be useful for predicting SBP participation, a variable representing the total hours that parents work is not a good candidate for an instrumental variable because it is likely to affect both SBP participation and learning. Furthermore, information on morning work schedules is likely to be a weak instrumental variable unless supplemented by additional information on the child's school schedule; that information is not included in the ECLS-K. In Section C, we suggest additional data that could be collected for use in identifying determinants of SBP participation.

b. School-level background characteristics

The ECLS-K includes a wealth of information on the characteristics of schools that could be used to estimate the contribution of SBP participation to learning outcomes at the school level. These characteristics include the average individual characteristics of sampled students in a given school (such as the average proportion eligible for free school breakfasts or lunches) as well as school characteristics obtained through surveys of principals and teachers, and through the inspection of school facilities.

Table IV.2 lists the following school characteristics that may affect the learning of students (specific variables relating to these characteristics are included in Appendix B):

- Type of school (regular public, magnet, charter, or private), location (urban, rural, or suburban), and region (Northeast, Midwest, South, or West)
- Number of kindergartners
- Number of full-time equivalent staff per student (distinguishes regular classroom teachers from teachers' aides and other staff)
- Teachers' salaries (minimum and maximum values) and background (education, work experience, and tenure)
- Principal's background (teaching experience, experience as a principal, and tenure)
- Allocation of the principal's time (with students, teachers, and parents)
- Use of standardized tests and grade retention
- Availability of special programs (ESL, bilingual education, and gifted/talented programs)

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in this instrumental variable. Parents who report not knowing about SBP availability in their child's school may simply have responded incorrectly to the survey. Alternatively, their response may indicate that they do not carefully monitor information their child's school provides.

- Availability of full-day kindergarten
- Number of computers per pupil
- Physical characteristics of school library and classrooms

The ECLS-K also includes the following characteristics of schools that are likely to affect average levels of SBP participation without influencing learning outcomes:

- Availability of before-school care[45]
- Physical characteristics of the school cafeteria

Table IV.2—School background characteristics in the ECLS-K (in addition to average levels of student characteristics)

VARIABLE	SOURCE	COMMENTS
Characteristics affecting learning		
School type	Principal survey	Distinguishes regular public schools, magnet schools, charter schools, and private schools
School location	Principal survey	Distinguishes urban, rural, and suburban schools
Number of kindergartners	Principal survey	Average number of students can be calculated using the total school enrollment
Full-time equivalent staff per student	Principal survey	Distinguishes regular classroom teachers, teachers' aides, and other staff
Teachers' salaries	Principal survey	Minimum and maximum values
Teachers' background	Teacher survey	Education, work experience, tenure
Principal's background	Principal survey	Experience as teacher and as principal, tenure
Principal's time allocation	Principal survey	Time with students, teachers, and parents
Use of standardized tests	Principal survey	Proportion of grades assessed
Use of grade retention	Principal survey	Proportion of kindergarten students held back
Special programs	Principal survey	ESL, bilingual, and gifted/talented programs
Full-day kindergarten	Principal survey	
Computers per pupil	Principal survey	
School library and classroom characteristics	Facilities checklist	Includes observations on space, light, ventilation, physical condition, room temperature, noise, and handicap accessibility
Parents' involvement	Principal survey	

[45] Before-school care programs may directly influence learning if they include an educational component. This possibility should be more carefully examined before definitively concluding that this characteristic does not influence learning outcomes.

Additional characteristics affecting SBP participation		
Availability of before-school care	Principal survey	
School cafeteria characteristics	Facilities checklist	Includes observations on space, light, ventilation, physical condition, room temperature, noise level, and handicap accessibility

These two school characteristics could be used as instrumental variables in an analysis of the importance of SBP participation on schoolwide learning. Other school characteristics that the ECLS-K does not collect, such as school bus schedules, the time classes start, and the content of school breakfast menus, are also likely to affect SBP participation. The following section suggests questions that could be added to the ECLS-K survey of school administrators and parents to obtain this supplemental information.

C. ECLS-K Supplemental Data Collection

In a report by Jacobson and Briefel (2000), we discussed a broad range of supplemental data collection activities, including supplemental questions on the parent and school administrative surveys and an entirely new survey component—a brief interview with school food service personnel.[46] Given the high cost of adding survey items and survey components to a data collection effort already involving a broad range of data collection activities on more than 16,000 students in 850 schools, we have limited the number of supplemental data collection questions.

This section describes what we, along with ERS, consider to be the supplemental data collection activities that would have the greatest usefulness in an ECLS-K-based study of the impacts of SBP participation on learning. We suggest adding supplemental questions to the parent and school administrator surveys to facilitate the construction of instrumental variables. (The original questions in the ECLS-K survey that relate to these topics are included in Appendix C.) We have dropped our earlier suggestion (Jacobson and Briefel, 2000) to survey food service employees since the cost of collecting that data would be quite large and some of the data could be collected through the school administrator survey.

1. Parent survey

The ECLS-K parent survey asks parents about their child's school breakfast and lunch participation, and about factors that may influence SBP participation but not learning, such as parents' awareness of the SBP and the distance of the child's home from the school. Table IV.3 lists the proposed high-priority supplemental questions that we suggest be added to the parent survey. We also suggest that the ECLS-K questions on the number of school breakfasts and lunches received during the previous 5 school days be asked of all children, not just of those who usually receive these meals.

[46]Jacobson and Briefel (2000) discussed, but dismissed, the possibility of conducting dietary recall surveys of children.

Table IV.3—Supplemental survey questions for parents

Breakfast habits of the child and parent

1. During the last 5 days (CHILD) was in school, how many breakfasts did (he/she) eat that were NOT school breakfasts (for example, breakfasts eaten at home, at child care, at school but not part of a school breakfast, and so on)? Please count only one breakfast per day.

   None 1 2 3 4 5 Don't Know

(Skip to Question 3, if response is "None.")

2. Where did (CHILD) eat these breakfasts? (check all that apply)

————At home
————At a relative's or friend's home
————At child care location
————At school, but not part of school breakfast
————At a restaurant (this includes food taken-out from fast food restaurants)
————Other (specify)

3. During the last 5 days (CHILD) was in school, how many breakfasts did *you** eat? Please count only one breakfast per day.

   None 1 2 3 4 5 Don't Know

   *Assumes the question is addressed to the main caregiver.

Child's morning schedule

4 (a) How does (CHILD) usually get to school in the morning?

————School bus
————Parent drives {him/her}
————Carpool
————Walk
————Other

4 (b) How long does this take?

————Less than 15 minutes
————15-30 minutes
————More than 30 minutes

5. On school days, how much time does (CHILD) have between arriving at school and classes starting?

————Less than 10 minutes
————10-20 minutes
————More than 20 minutes

The first three questions in the table provide information on the breakfast habits of the child and parent. Question 1 asks the parent to provide the number of nonschool breakfasts the child ate during the previous

5 school days. ("Nonschool" breakfast is defined clearly in the question.)

Question 2 asks about where the nonschool breakfasts were eaten. The information from these two questions would present a more complete picture of children's overall breakfast habits. In particular, when these items are combined with the ECLS-K question on the number of school breakfasts received during the same period, it will be possible to determine how often children skip breakfast.

This information could be used to address such issues as whether the availability of the SBP in a school is related to the probability of skipping breakfast, and whether eating any breakfast influences school learning. It will also be possible to determine, amongst the children who do regularly eat a nonschool breakfast, what breakfast alternatives are favored.

The information on parents' breakfast habits, obtained from question 3, would serve as a proxy for parents' attitudes about breakfast eating at home. The more often a parent or other adult in the household eats breakfast at home, the greater the likelihood their child will eat at home with them, and the lower the likelihood that the child will eat a school breakfast.

By themselves, however, parents' breakfast habits could be exogenous with respect to the student learning outcomes in this study. In this case, parents' breakfast habits could be a useful instrumental variable for children's SBP participation status.

The information that can be drawn from questions 4 and 5, on a child's morning schedule, could be used to construct useful instrumental variables. The questions on children's morning schedules are designed to capture as much relevant data as possible to account for various possible scheduling arrangements, and to give researchers flexibility in constructing variables representing the morning schedule.

Children's morning schedules are likely to influence their SBP participation status but have no direct effect on learning. For example, a child who arrives at school via school bus well before the start of school would have time to eat a school breakfast. A child who must rise early because of a long school bus commute would have limited time at home for breakfast but might be more likely to eat a school breakfast, if time is available to do so.

A child who is driven to school by a parent might be more likely to eat at home with his or her parents (because parent and child would leave the house at the same time). Because variables derived from school bus schedules are likely to be independent of parental behavior and parental characteristics and are unlikely to directly affect learning outcomes, they are good candidates to be instrumental variables to predict school breakfast participation.

2. School administrator survey

Table IV.4 presents supplemental questions that we suggest be added to the school administrator survey. To derive the greatest benefit from the supplemental parent survey questions on children's morning schedules, we would have to add supplemental school administrator survey questions on the school's morning schedule and SBP participation.

Table IV.4—Supplemental survey questions for school administrators

Morning school schedule

1. What time does the FIRST bus usually arrive at school in the morning? _____ AM
2. What time does the LAST bus usually arrive at school in the morning? _____ AM
3. What time does school officially start in the morning? _____ AM

School-level breakfast and lunch eligibility and participation

4. Does your school participate in USDA's school breakfast program?
   Yes [skip TO Question 6] No
5. What are the reasons why your school does not participate in USDA's school breakfast program?
   _____ Too few eligible students
   _____ Program too costly
   _____ School starts too late to serve breakfast
   _____ School lacks facilities to serve breakfast
   _____ School lacks staff to serve breakfast
   _____ Other reason (SPECIFY)

   [skip to Question 10]

6. What time is breakfast served at the school? _____ start time _____ end time

7. Where is the breakfast typically served for 3rd graders?
   _____Cafeteria
   _____Classroom
   _____School bus (as a bag breakfast)
   _____In some other common area of school (as a bag breakfast)
   _____Other (SPECIFY)_____

8. Are children who are served breakfast in the cafeteria allowed to take it to the classroom?

   Yes No

9. How many children in your school were eligible for and participating in the school breakfast program as of October of the current (or most recent) school year? Write in numbers below.

	Eligible Children	Participating Children
a. Any school breakfast?	All Enrolled	_____
b. Free school breakfast?	_____	_____
c. Reduced-price breakfast?	_____	_____

10. How many children in your school were eligible for and participating in the school lunch program as of October of the current (or most recent) school year? Write in numbers below. If service is not provided, write "zero."

		Eligible Children	Participating Children
a.	Any school lunch?	All Enrolled	_____
b.	Free school lunch?	_____	_____
c.	Reduced-price school lunch?	_____	_____

USDA = U.S. Department of Agriculture

Questions 1 and 2 would add information on the time that children arrive at the school by bus, and Question 3 would add information on the time that morning classes start. Question 4 checks whether a school even participates in the SBP and question 5 asks school administrators in schools that do not offer the SBP why the school does not participate in the program.

This information would be useful because a school's lack of participation in the SBP implies that all children of that school will be nonparticipants. The information could be used to develop strategies to make school breakfasts accessible to a broader range of students. In addition, because students in non-SBP schools are a potential comparison group to SBP participants, the information would help researchers determine the extent to which students at these schools are in fact comparable to students at SBP schools.

Question 6 would add information on the time that breakfast is served, and questions 7 and 8 would add information on where breakfast is served or eaten (to a typical class, such as third graders). The key feature of these questions, along with questions 1 through 3, is that they could be combined with the supplemental questions on children's morning schedules to construct more detailed measures of the scheduling and logistical constraints that might increase or decrease the likelihood that a particular child would participate in the SBP.

For example, combining the information from parents on the time that a particular child has between arriving at school and going to class with the information from the school administrator on the time and place that breakfast is served and on the time that classes start would enable us to determine whether the child has the opportunity to eat a school breakfast.[47]

The final supplemental questions, 9 and 10, add several options to a question in the ECLS-K school administrator survey that asks school administrators to provide the (certified) eligibility and participation data for free breakfasts, free lunches, and reduced-price lunches. To obtain complete data on school-level certification and participation, we suggest adding questions about eligibility for reduced-price breakfasts

[47] If the bus arrives at 8 a.m., breakfast is served from 8 a.m. to 8:30 a.m., and classes start at 8:30 a.m., the child could eat breakfast. Children who arrive by bus at 8:25 a.m. would not be able to eat breakfast, unless breakfast were served in the classroom or provided by the cafeteria in bags, to be eaten in the classroom.

and about participation among all students in the breakfast program, eligible students in the reduced-price breakfast program, and all students in the lunch program.[48]

This information would be especially helpful, as we would be able to increase the precision of the measures of school-level SBP participation, and to assess the quality of the parent-reported, child-level SBP participation data by comparing the implied participation rates based on this information with the implied participation rate based on the school-level data reported by principals. Finally, the ECLS-K already contains a question to obtain similar information, so it would be easy to add these extra items.

In isolation, the supplemental questions to the parent and school administrator survey would be less useful than when combined. In combination, the questions would enable researchers to construct a potentially powerful instruments that would address selection bias in estimations of the impact of SBP participation on learning. One simple example of such a variable would be a binary indicator of whether or not the child takes a bus to school in the morning and whether the bus arrives at least 20 minutes before school starts. We believe that these children would be much more likely to participate in the SBP.

D. ECLS-K Analytic Approach

We propose using the ECLS-K in a nonexperimental analysis. We must therefore rely on multivariate statistical methods to infer the differential effect of school breakfast participation (or, alternatively, attendance at a school offering the SBP) on the educational outcomes of otherwise identical students.

A strategy for estimating the impact of SBP participation on learning must account for the selection of SBP participants into the sample, conditional on students attending a school that offers the SBP. It also may be necessary to account for the selection of schools into the sample of schools offering the SBP.

In this section, we discuss the general framework for the analysis and the way we propose to address selection bias. We then discuss methods for identifying students with varying propensities of receiving school breakfasts, conditional on their attendance at a school offering the SBP. In the last two sections, we discuss, respectively, the minimum detectable differences (MDDs) that are likely to arise from our methodology and different procedures we might use to present estimated results.

The basic analytic framework was introduced in Chapter III. Because this model is based on multiple observations of children from the same school, it can be written in the form of two equations, one representing within-school variation in student participation and outcomes and the second representing between-school variation in mean participation and outcome levels.

(1) $(Y_{is} - Y_s) = \gamma (X_{is} - X_s) + \delta (P_{is} - P_s) + (e_{is} - e_s).$
(2) $Y_s = \alpha A_s + \beta Z_s + \gamma X_s + \delta P_s + u_s.$

[48] Alternatively, each of these questions could be broken down into three parts: (1) the number of students certified for free or reduced-price meals as of October (when certifications are usually completed), (2) the number of free, reduced-price, and full-price meals served during a month or school year, and (3) the number of serving days covered in the period in the second part of the question. However, this step would increase the complexity of the survey. Furthermore, because the change would occur in the third or fourth year of the study, the longitudinal quality of the data would be affected adversely.

The variables in these equations are defined as in Chapter III.

Given the assumption that the independent variables are uncorrelated with the error terms (that is, that there is no selection bias), the models can be estimated through linear estimation methods, such as ordinary least squares (OLS) estimation. The assumption of no selection bias is a relatively strong one, however. It implies that, after controlling for observable characteristics, participants and nonparticipants are identical in all other ways related to the outcome of interest. The following section discusses methods for estimating the impact of SBP participation on learning if the selection bias assumption is relaxed.

1. Selection bias

In estimating the parameters of equations (1) and (2), we must recognize the possibility that selection into the sample of school breakfast participants is endogenous with respect to learning outcomes. That is, school breakfast participation levels (P_{is} and P_s) may be correlated with the unexplained portion of learning outcomes within schools (e_{is}) or between schools (u_s).

This correlation could occur because of unobserved characteristics of families or schools that both promote school breakfast participation and advance a child's learning. If such a correlation were present, the estimated relationship between school breakfast participation and learning (δ) would be biased, as would the other parameters of equations (1) and (2).

A standard correction for selection bias is to use an instrumental variables (IV) procedure to predict the endogenous explanatory variable, using a linear equation that has the following form for within-school participation:

(3) $(P_{is} - P_s) = \pi (X_{is} - X_s) + \theta (I_{is} - I_s) + (r_{is} - r_s),$

and the following form for between-school participation:

(4) $P_s = \kappa Z_s + \phi X_s + \theta I_s + v_s,$

where P_{is} refers to each student's school breakfast participation; P_s refers to average school breakfast participation in each school; X_{is} refers to characteristics of each student; X_s refers to the average levels of these characteristics in each school; I_{is} refers to additional characteristics of each student affecting school breakfast participation but not student outcomes; I_s refers to the average levels of these characteristics in each school; Z_s refers to additional school characteristics affecting both school breakfast participation and student outcomes; r_{is}, r_s, and v_s are error terms (with $r_s = 0$); and κ, φ, and θ are parameters to be estimated (κ represents the estimated effect of Z_s on P_s, φ represents the estimated effect of X_s on P_s, and θ represents the estimated effect of I_s on P_s).[49] These equations would be estimated conditional on a school offering the SBP (that is, conditional on $A_s = 1$).

[49] When P_{is} is defined as a binary variable, equation (3) becomes equivalent to a linear probability model in which a heteroskedasticity correction is required to produce correct standard errors (see Maddala 1983). In contrast, P_s is an average participation rate and is assumed to be a continuous variable between 0 and 1 for the purpose of OLS estimation.

Researchers could use the estimates of the parameters in equations (3) and (4) to derive predicted values of school breakfast participation levels and could then substitute these values for the reported values in equations (1) and (2).

The resulting estimates of α, β, γ, and δ will be unbiased if the variables I_{is} and I_s (the instrumental variables) are validly excluded from equations (1) and (2). To qualify as a valid instrumental variable, a variable must be uncorrelated with the outcome variable but correlated with the endogenous explanatory variable.

In this case, I_{is} and I_s would be factors affecting school breakfast participation but *not* learning outcomes. Family income and household food security are likely to be important factors affecting SBP participation. However, they also are likely to have independent effects on learning, so they are not valid instrumental variables.

In this chapter, we discussed candidates for instrumental variables from the original ECLS-K survey and supplemental data collection from parents and schools that might be used in this analysis.

The strength of IV estimates depends on the availability of valid instruments as well as on their predictive power. As we note in Section B.3 of this chapter, without strong instruments, the ability of the analysis to detect small impacts of SBP participation on learning is seriously limited. An equally serious issue is the practical difficulty of identifying valid instrumental variables, even among those that appear to be theoretically sound. As illustrated by Bound et al. (1993), having instruments of questionable validity can lead to biased impact estimates.

A further complication of the analysis would arise if, in addition to the endogeneity of a student's SBP participation (P_{is}) with respect to learning outcomes, the student's attendance at a school offering the SBP (A_s) also were endogenous with respect to these outcomes. This situation would involve endogenous selection into the sample of students in SBP schools. For example, parents who expect their children to benefit academically from SBP participation might be more likely than other parents to place their children in schools offering the SBP. Alternatively, schools might adopt the SBP in the belief that their students are especially likely to benefit academically from SBP participation.

At least two approaches could be used to model the selection of schools into the SBP. First, linear instrumental variables could be used to predict a school's participation in the SBP, as well as the student's participation in the program. Second, a bivariate probit procedure and the proposed set of instrumental variables could jointly estimate the probability of the school offering the SBP and the probability of the student participating in the SBP where it is offered.

This procedure would not only make it possible to predict each student's probability of attending a school offering the SBP, it would also make it possible to predict each student's *joint* probability of attending an SBP school and participating in the program. The joint probabilities could be substituted for the reported SBP participation of the school and student in equation (2). The difficulties of identifying valid instruments in the previously mentioned IV models apply to an even greater extent to this model, which contains two possible sources of selection bias.

2. Propensity score methods

Estimating probabilities of SBP participation for students in SBP schools would make it possible to use propensity score methods to create subgroups of likely SBP participants in SBP schools and likely SBP participants in non-SBP schools. These methods effectively match participants and nonparticipants on the basis of their observable characteristics. However, because the two groups might differ in their unobservable characteristics, propensity score methods would not address the underlying selection bias problem, if it exists.

Under the propensity score methodology, the subclassification of participants and nonparticipants on the "propensity score" could be accomplished as follows. First, assume that the probability of SBP participation is estimated according to the following equation:

(5) $probability\ (P_{is} = 1 \mid A_s = 1) = F\ (\pi Z_s + \sigma X_{is} + \tau I_{is}),$

where P_{is} is each student's actual SBP participation; A_s is the availability of the SBP at the student's school; Z_s is a set of observed school-specific characteristics; X_{is} is a set of personal background characteristics for the student; I_{is} is a set of additional characteristics correlated with SBP participation; π, σ, and τ are parameters to be estimated; and $F(.)$ is the normal density function.[50]

Next, assume that equation (5) is used to assign all students, including those in non-SBP schools, to categories (Q_{is}) on the basis of their probability of SBP participation if they were in a school offering the SBP. At a minimum, Q_{is} would define quintiles of the distribution of students in non-SBP schools (or of students in SBP schools, depending on which population was of greater interest for policy purposes).[51] The following equation could be estimated separately for each subgroup k defined by Q_{is}:

(6) $Y_{is} = \alpha^k A_s + \beta Z_s + \gamma^{kk} X_{is} + w_{is},$ for each category k of Q_{is},

where Y_{is}, A_s, Z_s, and X_{is} would be defined as before; w_{is} would be an error term; and α^k, β^k, and γ^k would be parameters to be estimated. The key parameter of interest would be α^k, which would measure the impact of offering the SBP to students with a propensity to participate indicated by subgroup k. We expect students in lower-income families and in food-insecure households to be in subgroups with higher propensities of SBP participation. Averaging the values of α^k across all the subgroups would produce an estimate of the impact of expanding the SBP to students in all schools that do not offer the program.[52]

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[50] If equation (5) is estimated jointly with the probability of the school offering the SBP, a bivariate normal density function also may be used.

[51] To study the effects of offering the SBP to students in non-SBP schools, the subclassification must be based on the distribution of estimated propensity scores for students in non-SBP schools. For some subclasses, relatively few observations may be available for comparison, suggesting that comparisons for these subclasses will be imprecise, at best, and impossible to make, at worst. This issue is relevant to an analysis of the SBP, because relatively few ECLS-K students in non-SBP schools might have high propensities of SBP participation.

[52] For additional information on propensity score methods, including methods for matching observations directly using the propensity score, we refer the reader to Rosenbaum and Rubin (1983, 1984, and 1985). For an interesting example of subclassification used to create subgroups for policy analysis, consult Kemple et al. (2000).

This propensity score approach is valuable for estimating the impact of *offering* the SBP to students, but it also could be used to develop an alternative set of estimates of the impact of SBP *participation* on learning. If, as appears likely, the estimated impact of offering the SBP is zero for nonparticipants, the impact of SBP participation on participants in each subgroup may be estimated by dividing the corresponding SBP participation rate in schools offering the SBP into the estimate of α^K. As before, averaging the different estimates for the various subgroups could be used to obtain an overall estimate for the entire population of interest. *Note that, to generate unbiased impact estimates, this analysis also would have to assume that attendance at a school offering the SBP is itself independent of unobserved variables influencing learning outcomes.*

3. Minimum detectable differences

The formula for calculating the MDD for the participation model (assuming no selection bias) is the following with 80 percent power:

$$MDD = 2.486\sqrt{\left(\frac{1-R_1^2}{1-R_2^2}\right)\left[(1-\rho)\sigma^2\left(\frac{1}{sn_t}+\frac{1}{sn_c}\right) + \rho\sigma^2\left(\frac{1}{s}+\frac{1}{s}\right)\right]}$$

where σ^2 is the variance of the outcome, ρ is the proportion of the total variance due to cross-school variance, R_1^2 is from the outcome equation, R_2^2 is from a supplemental regression of treatment status (participation) on the X variables from the outcome equation, s is the number of schools in the sample, n_t is the number of participants per school in the sample, and n_c is the number of nonparticipants per school in the sample.[53]

The main difference between this formula and the formula for an experimental evaluation, as described by Bloom (1995), is that the nonexperimental formula divides by $(1 - R_2^2)$, which is the proportion of variation in participation status remaining after we control for the X variables. The greater the correlation between participation status and the X variables, the smaller this term will be and the larger the MDD will be. The formula is based on the formula for the standard error of a regression coefficient, taking into account multicollinearity.

In the case of selection bias, there is even less exogenous variation in participation status. All the techniques for controlling for selection bias effectively control for the unobservable determinants of participation status. Therefore, the only exogenous variation in participation comes from the portion of the variation explained by the identifying variables. The resulting formula is:

$$MDD = 2.486\sqrt{\left(\frac{1-R_1^2}{R_3^2-R_2^2}\right)\left[(1-\rho)\sigma^2\left(\frac{1}{sn_t}+\frac{1}{sn_c}\right) + \rho\sigma^2\left(\frac{1}{s}+\frac{1}{s}\right)\right]}$$

[53] This formula accounts for the clustering of observations in particular schools. In addition, the formula assumes that observations from students attending schools that do not offer the SBP are excluded, and that each school in the included sample has both SBP participants and nonparticipants.

where R_3^2 is from a regression of participation status on both the X variables from the outcome equation and the identifying variables. The term (R_3^2 - R_2^2) represents the increase in the R^2 that comes from adding the identifying variables to the participation equation. The better the identifying variables predict participation, the more they will add to the R^2 and the lower the resulting MDD will be.

We can use the two formulas shown here to calculate sample MDDs for an achievement test score outcome under different assumptions. In the following three sample scenarios, we assume that the analysis excludes schools that do not off the SBP. We also assume that, after accounting for sample attrition, there will be at least 750 schools in the ECLS-K that could be used in the analysis. We expect the schools to include a total of 15,000 students (20 students per school). An average of one-quarter of the students (3,750) will be SBP participants, and an average of three-quarters (11,250) will be nonparticipants.

Scenario I

$\rho = 0.05$
$\sigma^2 = 1$
$R_1^2 = 0.30$ (reasonable when estimating achievement growth as a function of prior achievement)
$R_2^2 = 0.25$ (0.23 was obtained using ECLS-K data and a reduced set of explanatory variables)
Participation rate = 25 percent
750 schools (that is, $s = 750$)
20 students per school
thus, $n_t = 5$
$n_c = 15$
no selection bias

MDD = 2.486 * 1 * sqrt {(.70)/(.75) * [(.95)(1/3,750 + 1/11,250) + (.05)(1/750 + 1/750)]} = 0.052 of a standard deviation.

(For comparison, the average distance between adjacent percentiles of the test score distribution could reasonably equal 0.025 of a standard deviation, so this magnitude of MDD should enable us to detect relatively small differences in outcomes, for example, a change of three percentiles.)

Scenario II

Same as Scenario I but with selection bias and
$R_3^2 = 0.35$ (under this assumption, the identifying variables add 0.10 to R^2)[54]

MDD = 2.486 * 1 * sqrt {(.70)/(.10) * [(.95)(1/3,750 + 1/11,2500) + (.05)(1/750 + 1/750) } = 0.143 of a standard deviation.

[54] We analyzed actual ECLS-K data for 1998-1999 and did not find any evidence of such an increase in predictive power from the addition of the identifying variables available in the ECLS-K to the participation equation. A dramatic increase would be possible only if supplemental data collection obtained an especially powerful set of identifying variables.

(This example indicates that selection bias can dramatically increase the MDD of the analysis, making it more difficult to detect small differences in outcomes between SBP participants and nonparticipants.)

Scenario III

Same as Scenario II but with identifying variables that are much less powerful predictors of selection:

$R_3^2 = 0.29$ (under this assumption, the identifying variables add only 0.04 to R^2)[55]

MDD = 2.486 * 1 * sqrt {(.70)/(.04) * [(.95)(1/3,750 + 1/11,250) + (.05)(1/750 + 1/750)} = 0.226 of a standard deviation.

(This example indicates how the absence of instrumental variables that are strong predictors of SBP participation can dramatically increase the size of the MDD when selection bias is present, making it highly unlikely that small differences in outcomes between SBP participants and SBP nonparticipants will be detected.)

In general, the large number of schools in the ECLS-K increases the power of ECLS-K-based analyses. If, instead of 15,000 students distributed across 750 schools, a research sample contained the same number of students distributed across 100 schools, the MDDs in scenarios I, II, and III would have equaled 0.088, 0.241 and 0.380, respectively, 69 percent larger than the MDDs estimated for the ECLS-K. Consequently, the MDDs for ECLS-K-based analyses are smaller primarily because of the large number of schools included in the study.[56]

E. Presentation of ECLS-K-Based Design Results

Table 5.5 indicates how estimated impacts of SBP participation (or attending an SBP school) might be presented for comparison purposes. Each proposed estimation method has certain limitations. A simple comparison of reported differences in outcomes between SBP participants and SBP nonparticipants would fail to take into account the observed differences between the two groups.

OLS, fixed-effects, and random-effects estimates account for observed differences between students and their schools, but not for the endogenous selection of students into the SBP (and, possibly, into schools offering the SBP). IV estimates may help correct for sample selection bias, but their ability to do so is depends on whether valid identifying variables are found. In addition, the precision of the IV estimates will depend on the predictive power of the instrumental variables used in the analysis.

[55] Preliminary regressions using ECLS-K data for 1998-1999 indicate that this increase in predictive power is obtained by adding parents' knowledge of the SBP and distance from the school as predictive variables.

[56] These calculations ignore the possibility of greater clustering of the schools in the ECLS-K resulting from sampling of multiple schools from the same school district. To the extent that clustering occurs, the MDDs understate the true MDDs obtainable through the evaluation.

Table IV.5—Presentation of estimated impacts of the SBP on a given outcome (for example, test scores measured in standard deviation units)

IMPACT/ESTIMATION	Estimated impact	Standard\ error	Statistical significance
Method			
Impact of SBP participation on SBP participants versus nonparticipants			
Reported values			
OLS			
Fixed effects for schools			
Random effects for schools			
IV with school fixed effects			
IV with school random effects			
IV with school SBP participation endogenous			
Propensity score approach (based on distribution in SBP schools)			
Propensity score approach (based on distribution in non-SBP schools)			
Propensity score approach (based on distribution in SBP schools)			
Students in top propensity quintile			
Students in second propensity quintile			
Students in third propensity quintile			
Students in fourth propensity quintile			
Students in bottom propensity quintile			
Propensity score approach (based on distribution in non-SBP schools)			
Students in top propensity quintile			
Students in second propensity quintile			
Students in third propensity quintile			
Students in fourth propensity quintile			
Students in bottom propensity quintile			

Propensity score methods can account for how attendance at an SBP school is associated with different outcomes for students with different probabilities of SBP participation. However, the ability of propensity score methods to identify the true impact of SBP participation on learning outcomes depends on whether attendance at an SBP school is independent of the unobserved factors influencing learning.

In other words, selection bias may influence the estimation of propensity score models as well. Propensity score methods cannot be used to correct for the endogenous selection of students into the sample of SBP participants, as propensity scores account only for differences in students' observable characteristics.

F. Supplemental NHANES III Analysis

The primary rationale for conducting the supplemental NHANES III analysis is that the ECLS-K-based analysis does not address the question of how SBP participation affects intermediate outcomes, such as nutrition and health status. Furthermore, NHANES III includes cognitive and academic performance tests as key indicators of learning.

By understanding the direct impact of participation on students' dietary intakes, nutritional status, and health status, it will be easier to interpret any impacts of participation on learning-related outcomes. For example, a positive impact of participation on learning could arise because students who eat school breakfasts have improved nutritional status or are more alert during morning classes. Alternatively, if participation improves students' overall health, participants may be absent from school because of sickness less often and thereby perform better academically.

Conducting the supplemental NHANES III analysis would improve the overall design for two additional reasons. First, the analysis would provide additional evidence on the impact of school breakfast participation on learning. Because NHANES III contains information on children's cognitive functioning and academic performance, the supplemental analysis may corroborate or fail to corroborate the primary findings from the ECLS-K.[57]

The issues of selection bias and statistical power are important in the ECLS-K-based design, so having additional evidence on the critical research question will be valuable. Second, the NHANES III data would give the design the ability to examine the relationship between SBP participation and various student outcomes among older children as well as elementary-school-aged children.

In particular, the analysis may suggest whether the effects of SBP participation change as children get older, although we know that SBP participation decreases as children age. Defining the variables that contribute to SBP participation in older children, especially teenaged girls who frequently skip breakfast, could help us understand the potential benefits of promoting the SBP to older age groups.

1. Basic Design Approach

In the NHANES III analysis, the intervention is participating in the SBP (that is, usually eating a school breakfast). To study learning outcomes, we are most interested in an intervention that covers a substantial

[57] The analysis of a main outcome, academic achievement, is more limited in NHANES III because it is measured at only one point in time. Therefore, no measure of change is available.

period—"usual participation" over a school year.[58] This information is collected in the NHANES as the number of times per week that the child selects a school breakfast. In particular, the intervention refers to participation in the SBP *as it was implemented in schools throughout the country from 1988 to 1994.*

The counterfactual against which the intervention is being assessed is nonparticipation in the SBP, or "not usually eating a school breakfast." The main counterfactual condition is attending a school that offers the SBP but "not usually eating a school breakfast". We also may want to examine the counterfactual condition of attending a school that does not offer the SBP. Finally, it may be useful to examine differences in outcomes among three primary groups: (1) SBP participants, (2) SBP nonparticipants, and (3) children who usually do not eat breakfast.

Elementary-school-aged children are of greatest interest among the populations the intervention targets. The design also calls for analysis of models that include older children (especially girls) because (1) the NHANES III data include information on older children and increase the available sample size for the proposed analysis; and (2) older children are less likely to report eating breakfast, which increases the potential role of the SBP.

Although the NHANES III includes a wealth of information pertinent to the research question of interest, the total sample size is smaller than the one available in the ECLS-K design, and the dataset is not longitudinal. Thus, the NHANES III data analysis will supplement the analysis of the ECLS-K data. The cross-sectional nature of the NHANES III dataset (and the resulting NHANES III analysis) means that key outcomes are measured at a given point in time during the school year.

The mean values of key outcomes are collected at the same time and cover the same time period as is covered by the information on SBP participation. One implication of this design is that it is not possible to measure changes in students' achievement levels, as in the ECLS-K-based analysis. This lessens the resulting statistical power of the estimated impact of SBP participation on learning.

2. Data and Measurement

a. The NHANES III Data

The NHANES provides data on a nationally representative sample of school-aged children, including SBP participants and nonparticipants. It measures much of the same family and school information that other national surveys such as the ECLS-K measure, but it also includes detailed diet, nutrition, and health information on younger and older children.

The NHANES sample is a representative, cross-sectional sample of the U.S. civilian, noninstitutionalized population. The sample design is a multistage, complex, stratified survey design of individuals living in households. The survey was designed as two 3-year national samples: (1) 1988 through 1991, and (2) 1991 through 1994. In addition, the entire six-year span constitutes a national sample.

The survey design includes participants of all ages and racial/ethnic groups and equivalent numbers of boys and girls in each age group. Blacks and Mexican Americans are oversampled to produce reliable estimates for these major racial/ethnic groups, and sampling weights are used to adjust for nonresponse

[58] Several possible definitions of "usual participation" could be used. Definitions and related measurement issues are fully described in Section B.2 of this chapter.

and to inflate the sample to produce nationally representative estimates (National Center for Health Statistics 1994). Though NHANES III data collection ended in 1994, when school breakfast participation rates were lower and school environments were different, the data's usefulness to supplement an ECLS-K based analysis outweighs these slight drawbacks.

The specific content in NHANES III that is pertinent to a study of SBP participation and learning is shown in Appendix D and includes a detailed list of recommended study variables. The table also presents the SAS variable names and corresponding NHANES III data file containing the variables. Details of the survey instrument are described in greater detail in other publications (National Center for Health Statistics, 1994) and are summarized here:

- ***SBP Participation.*** Participation in the SBP is a key variable for the study of learning and school breakfast, but the NHANES measure of SBP participation may be somewhat imprecise. The NHANES includes "usual" participation in the SBP during the school year (that is, the number of times per week the child usually receives school breakfast, as well as whether the breakfast is free or at a reduced price). This information is reported by the parents of all sampled children who are younger than age 17.[59]

- ***School Meals.*** Information is collected from the parent on whether the child's school offers the SBP or NSLP, how frequently the child participates in the SBP and NSLP, and whether the child receives free or reduced-price school meals.

- ***School Outcomes.*** Information is collected from the parent on the child's grade level, attendance, suspensions, expulsions, and skipped grades.
- ***Cognitive Tests.*** Standardized cognitive tests to evaluate short-term cognition and intelligence were administered in the mobile examination center during a private interview with 6- to 16-year-old children. Academic performance was assessed using subtests of the Wechsler Intelligence Scale for Children, Revised (WISC-R) and the reading and arithmetic subsections of the Wide Range Achievement Test, Revised (WRAT-R) (National Center for Health Statistics, 1996; and Kramer et al., 1995).

- ***Family Characteristics.*** Information on the family's income, food assistance program participation in the past year, food sufficiency, and socioeconomic status was collected, as was information on parents' education, health insurance coverage, and sources of medical care.

- ***Protective and Risk Behaviors.*** Assessed health behaviors for younger and older children include physical activity levels and time spent performing sedentary activities, such as watching television. Risk behaviors collected during private interviews with the child in the mobile examination center include smoking (for ages 8 years and older) and alcohol and drug use (for ages 12 years and older).

- ***Dietary Intake and Dietary Behavior.*** Dietary intake was assessed using 24-hour dietary recall methodology and additional interview questions about dietary habits. At least one 24-hour recall per

[59] Like with a variable constructed from the ECLS-K, any school breakfast participation variable constructed from the NHANES will be affected by the degree of parental accuracy in reporting, which is unknown at this point.

person was collected, with a second day's recall collected on a subsample. The 24-hour dietary recall provides information on whether breakfast was consumed, the time and source of breakfast(s), the foods and amounts consumed at breakfast, and the total day's intake. Total nutrient intake was estimated using information collected on dietary supplements use, discretionary salt use, and water intake. For 6- to 11-year-old children, a combination of self- and proxy-reporting was used for the 24-hour recall and dietary questions. For children 12 years or older, dietary intake and behavior were self-reported.

- ***Nutritional Status.*** The NHANES provides the most comprehensive picture of nutritional status available on a national sample of school children. Precise anthropometric measurements, such as height and weight, are used to assess growth and overweight in relation to the revised growth charts developed by the Centers for Disease Control and Prevention (CDC) (Kuczmarski et al., 2000). Blood and urinary measurements provide an assessment of vitamin and mineral status for a wide variety of nutrients, such as B vitamins and iron. Iron status is of particular interest, as iron deficiency is related to developmental and behavioral disturbances that may affect mental performance and learning in young children (Centers for Disease Control and Prevention, 1998).

- ***Health Status.*** General health status measures, such as blood pressure and presence of respiratory disease, provide an overall picture of the child's health and readiness to learn. Other health components in the NHANES related to a study of breakfast and learning include vision and hearing problems, which may affect classroom learning, and environmental exposures, such as to lead. Frequent health problems and illnesses may lead to more days absent from school and fewer opportunities to learn. Elevated levels of lead in the blood may be associated with iron deficiency anemia and are higher in low-income children (Centers for Disease Control and Prevention, 1998). Variables that relate to the child's prenatal environment, such as low birth weight or exposure to smoke, have been shown to relate to growth and development and are collected in the parent interview.

Table IV.6 presents the NHANES III sample sizes for school-aged children in three groups: (1) children who participated in SBP at least once per week, (2) children who did not participate in the SBP in schools that offered the program, and (3) children who did not have access to the SBP in the school they attended.

Between 1988 and 1994, about 18 percent of the NHANES III sample of school-aged children participated in the SBP, 32 percent did not participate, and 49 percent attended a school that did not offer the SBP. Oversampling produced approximately equal numbers of respondents who were participants, nonparticipants in SBP schools, and nonparticipants in non-SBP schools.[60] The sample contains equivalent numbers of boys and girls.

[60] For specific analytic purposes, nonparticipants can be further divided into subgroups that do or do not consume breakfast at home, or that do not consume breakfast at all. SBP participants may also consume a breakfast at home or at some other place, a factor to be considered when analyzing and interpreting the dietary data.

Table IV.6—Sample size of children aged 6 to 16 years, by SBP participation

SBP participation	6 to 11 years		12 to 16 years		6 to 16 years	
	N	(%)[a]	N	(%)[a]	N	(%)[a]
SBP participation	1,259	21.2	557	14.5	1,816	18.3
SBP nonparticipation	1,017	30.0	801	35.5	1,818	32.4
No SBP offered	1,174	48.8	742	50.0	1,916	49.3
Total[b]	3,450	100	2,100	100	5,550	100

Source: NHANES III, 1988-1994.
SBP = School Breakfast Program.
[a]Weighted to reflect the population based on the household interview weight.
[b]Excludes 186 children (3.2 percent) with missing information on SBP participation.

Table IV.7 presents the available sample sizes for 3 SBP participation groups by 2 income levels: (1) lower-income, defined as a family income at or below 185 percent of the poverty line and eligible for free or reduced-price school meals; and (2) higher income, defined as a family income above 185 percent of the poverty line. Weighted population data show that about 44 percent of the total population had a family income at or below 185 percent of the poverty line, and that 49 percent of the total population attended schools that did not offer the SBP. Only about 7 percent of those in the higher income group participated in the SBP.

Table IV.7—Sample sizes of children aged 6 to 16 years, by SBP participation, income level

SBP participation	Lower income[a]		Higher income[b]		Total	
	Sample size	Population percentage[c]	Sample size	Population percentage[c]	Sample size	Population percentage[c]
SBP participant	1,424	32.2	236	7.1	1,660	18.2
SBP nonparticipant	940	32.3	737	33.0	1,677	32.7
No SBP offered	797	35.5	933	59.9	1,730	49.1
Total (percent)[d]	**3,161**	**100 (44.5)**	**1,906**	**100 (55.5)**	**5,067**	**100**

Source: NHANES III, 1988-1994
SBP = School Breakfast Program.
[a]Lower income is defined as a family income at or below 185 percent of the poverty line.
[b]Higher income is defined as a family income above 185 percent of the poverty line.
[c]Weighted to reflect the population based on the household interview weight.
[d]The total sample size reflects a lower sample size than that shown in Table IV.6 due to missing information on family income.

Most children who participated in the household interview and had SBP information were also interviewed in the mobile examination center. Table IV.8 shows that about 91 percent of the total sample that had SBP information had complete, reliable 24-hour recall data; 89 percent had WISC-R and WRAT-R data, and 86 percent had all three data components. Table IV.9 provides the same information as is

shown in Table IV.8, but broken down by age group. Although the level of nonresponse in the survey was relatively low, the examination sampling weights account for nonresponse to the examination component.

Table IV.8—Sample sizes of children aged 6 to 16 years, by SBP participation and dietary and cognitive test score data

SBP participation	Initial sample	Sample size with dietary data	Sample size with WISC-R/ WRAT-R data	Sample size with dietary and WISC-R/ WRAT-R data
SBP participant	1,816	1,678 (92.4%)	1,646 (90.6%)	1,577 (86.8%)
SBP nonparticipant	1,818	1,667 (91.7%)	1,626 (89.4%)	1,583 (87.1%)
No SBP offered	1,916	1,699 (93.6%)	1,667 (87.0%)	1,603 (83.7%)
Total[a]	**5,550**	**5,044 (90.9%)**	**4,939 (89.0%)**	**4,763 (85.8%)**

Source: NHANES III, 1988-1994

Note: Percentages are weighted to reflect the population based on the examination sample weight. If the analysis is restricted to only intakes on school days, the overall sample size will be reduced by 30 percent or more.

SBP = School Breakfast Program; WISC-R = Wechsler Intelligence Test for Children, Revised; WRAT-R = Wide Range Achievement Test, Revised.

[a]Excludes 186 children (3.2 percent) with missing information on school breakfast program participation.

The study sample with dietary and cognitive test score data includes a higher proportion of 6- to 11-year-olds than 12- to 19-year-olds. However sample sizes are adequate to examine both groups separately.

Table IV.9—Sample sizes of children aged 6 to 16 years—SBP participation, dietary and cognitive test score data

	Sample size with dietary data				Sample Size with WISC-R/ WART-R data[a]				Sample size with dietary and WISC-R/WRAT-R data[a]			
	Age 6–11		Age 12–16		Age 6–11		Age 12–16		Age 6–11		Age 12–16	
SBP participation	N	(%)[b]	N	(%)[b]	N	(%)[b]	N	(%)[b]	N	(%)[b]	N	(%)[b]
SBP participant	1,161	22.1	517	14.2	1,148	21.9	498	14.3	1,094	21.6	483	13.9
SBP nonparticipant	929	30.2	738	35.7	914	30.1	712	36.0	886	30.7	801	35.9
No SBP offered	1,039	47.7	660	50.1	1,022	38.0	645	49.7	977	47.7	626	50.2
Total	**3,129**	**100**	**1,915**	**100**	**3,084**	**100**	**1,855**	**100**	**2,957**	**100**	**1,806**	**100**

Source: NHANES III, 1988-1994.
SBP = School Breakfast Program; WISC-R = Wechsler Intelligence Scale for Children, Revised; WRAT-R = Wide Range Achievement Test, Revised.
[a]A child is classified as having WISC-R/WRAT-R data if he or she completed any one of the four subtests.
[b]Weighted to reflect the population based on the examination sample weight.

b. Measurement Issues

SBP Participation. One analytic objective is to examine the mean dietary intakes in the SBP participation groups described earlier. Because parents report children's SBP participation, the measurement may be subject to error. One crosscheck is the 24-hour dietary recall, which captures what was consumed for breakfast and the location where breakfast was consumed. This information could determine whether breakfast was eaten at school on the day of the 24-hour recall.

A significant weakness of this approach is that the information would not be available on all children who also have WISC-R and WRAT-R data—some children are interviewed about their Saturday and Sunday intakes, and some are interviewed during the summer, when school is out of session. School day dietary intakes would be unavailable for at least 30 percent of the total sample. However, for children whose 24-hour dietary recall interviews referred to a school day, we could compare SBP participation on this sample day with the "usual participation" reported by the parents.

Alternative methods could be used to distinguish "usual participation" based on the information collected and based on the desired analysis. One option would define "usual participants" as children who ate a school breakfast on at least 3 of 5 school days. Another option would define SBP participation based on the consumption of two or more food items from the school cafeteria at breakfast.

Participation could be reported based on the sample day that the 24-hour dietary recall measures, or by the weekly or monthly frequency of participation. The day-to-day variability in participation measures must be considered for analysis conducted at the individual-level, in particular (Gleason and Suitor, 2000).

Dietary Intake. Dietary intake is assessed using 24-hour dietary recalls. The proposed analysis proposes to estimate average breakfast intakes and total dietary intakes for groups of children (such as SBP participants, nonparticipants, and nonbreakfast eaters), and to estimate the relative contribution of dietary intakes to nutritional status. The NHANES design includes one 24-hour recall per child and a second 24-hour recall on a subsample.[61]

The second day's intake provides information to adjust nutrient intake distributions using statistical software that takes into consideration the day of the week and within- and between-person variability (Nusser et al., 1996). Adjusted distributions of nutrient intake could be used to estimate the proportion of SBP participants and nonparticipants who met dietary recommendations and dietary adequacy. This approach provides information for comparing group dietary data but does not provide a better measure of individual students' usual dietary intakes for use in regression analysis.

Nutrition and Health Status. The 24-hour dietary recall provides information on current dietary

[61] It should be noted that the meal pattern requirements for a school breakfast changed during the data collection for NHANES III. The requirement to offer two grains/breads, two meat/meat alternates or a combination of both went into effect for school year 1998-1990. The final rule was published on March 30, 1989.

intake at the group level. A single day's intake is insufficient for estimating an individual's "usual intake"; longer-term nutritional status is reflected in biochemical assessments of blood and urine, hematologic determinations, and anthropometric measurements. Nutrition and health outcomes, such as growth, overweight[62] and iron deficiency anemia may relate to children's readiness to learn in school (Briefel at al., 1999; and Chapter II).

Behavioral Measures. Certain behaviors may contribute to a child's overall health status and school attendance. Protective behaviors, such as more physical activity and less time watching television, may improve health and readiness to learn. Certain risk behaviors, such as alcohol and drug use by older children, provide information that may be related to poor school performance, days absent from school, suspensions, and expulsions. These types of lifestyle and behavioral information collected in NHANES III can be used as control or explanatory variables when comparing learning outcomes between SBP participants and nonparticipants.

WISC-R and WRAT-R. Two subtests of the WISC-R, the Block Design and the Digit Span, are indicators of cognitive functioning and academic performance (Kramer et al., 1995). The Block Design is a performance examination; the Digit Span is a verbal test. The WRAT-R was used in the NHANES III to assess academic performance in reading and mathematics. After the WISC-R was given, trained interviewers administered the WRAT-R in the mobile examination center. An automated data collection system ensured that responses were within acceptable ranges. As described in Chapter III, the scores for all four subtests used a common scale and were derived for each child relative to his or her age group based on test-specific samples created by the test developers (Wechsler, 1974; and Jastak and Wilkinson, 1984).

3. Previous Research

Although the NHANES III contains a wealth of information pertinent to the research objectives, little information has been published that describes dietary intakes from breakfast or school breakfast, the cognitive functioning/academic performance tests, or methods to relate the two variables. Kramer et al. (1995) reported on the disparities in cognitive functioning across sociodemographic and health characteristics of children in the United States.

The authors found that lower income, lower education level, and minority status of an adult reference person were independently related to poorer performance on all cognitive subtests. Although less consistent as predictors of test performance, overall health status, a history of birth complications, and sex also were related to cognitive functioning and academic achievement. These results were based on NHANES III data for only the first phase of data collection (1988–1991). Measures of dietary intakes, food sufficiency, or nutritional status were *not* included as variables in the models.

Alaimo et al. (2001a) studied the relationship among poverty, food sufficiency, and health in U.S. schoolchildren, using NHANES III data. Although the study did not assess dietary intakes or school breakfast participation, it found that food-insufficient children were significantly more likely than food-sufficient children to have poorer health status, more frequent stomachaches, and more frequent

[62] Growth and overweight can be assessed using children's height and weight measurements relative to the BMI for age, height-for-age, and weight-for-age percentiles found in the CDC growth charts (Kuczmarski et al., 2000).

headaches. Another analysis of NHANES III relates food insufficiency to cognitive, academic (WISC-R scores and WRAT-R scores), and psychosocial outcomes in children 6–11 years old and 12–16 years old (Alaimo et al., 2001b). After adjusting for confounding variables, 6–11 year old children who were food insufficient were found to have significantly lower math scores and were more likely to have repeated a grade.

4. Analysis Plans

The primary NHANES III analysis for this design would compare mean differences in dietary, nutrition, health, and learning outcomes among the three groups of interest: (1) SBP participants, (2) SBP nonparticipants, and (3) children who do not eat breakfast. Additional subgroups could be analyzed based on whether the child consumes or does not consume a breakfast at home.[63] Descriptive analysis would include, but would not be limited to, comparisons of the following outcomes:

- Mean nutrient and food group intakes for breakfast and for the total day
- Mean dietary quality and variety score, assessed by the Healthy Eating Index
- The proportion meeting current dietary recommendations or dietary requirements for nutrient adequacy, as defined by the Recommended Dietary Allowances, the Dietary Reference Intakes, and the Dietary Guidelines for Americans[64] (U.S. Department of Agriculture and U.S. Department of Health and Human Services, 2000)
- The proportion defined as underweight, at a healthy weight, or overweight based on height and weight measurements and the revised CDC growth charts (Kuczmarski et al., 2000)
- The proportion with iron deficiency anemia or some other vitamin or mineral deficiency, based on biochemical test results
- Mean and distributions of cognitive test scores
- Mean number of missed days of school, tardiness, suspensions, expulsions, and skipped grades

To estimate the effects of SBP participation on these outcomes, the study would use regression analysis to compare the outcomes in participants and nonparticipants after controlling relevant, measurable factors. When comparing academic and learning outcomes, these factors would include food insufficiency; poor nutritional status; and factors that relate to prenatal nutrition, such as low birth weight and exposure to cigarette smoke.

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[63] SBP participants and nonparticipants may consume a breakfast at home in addition to or in the place of a school breakfast.

[64] Software from Iowa State University could be used to adjust nutrient intake distributions. The software takes into account the within- and between-person variability in intake and the skewness of nutrient intake distributions (Nusser et al. 1996). However, as described above, it cannot make regression-adjusted comparisons of the proportion of the groups that meet dietary recommendations because the procedure does not generate estimates of individuals' usual dietary intakes.

Environmental exposures, such as to lead, demonstrated by elevated blood lead levels, should be considered when interpreting the results of cognitive tests and academic performance. Thus, the set of control variables that the NHANES III data could provide would be richer in some ways than those available in the ECLS-K. Unlike the ECLS-K, however, the NHANES III dataset provides few school-level control variables (or indicators of which schools sample members attend). Thus, this analysis would not be able to measure or control for some of the institutional factors associated with the schools students attend.

In particular, the model to be estimated to generate estimates of the impact of SBP participation on learning and other outcomes is the following:

(7) $$Y_{is} = \alpha A_s + \gamma X_{is} + \delta P_{is} + u_s + e_{is},$$

where the variables are defined as in the ECLS-K section of this chapter. (Y_{is} represents the value of a particular outcome for student i in school s; A_s is an indicator of whether the student's schools offers the SBP; X_{is} is a vector of characteristics of student i that influence this outcome; P_{is} is an indicator of whether student i participates in the SBP; and u_s and e_{is} are, respectively, unmeasured school-specific and student-specific factors that influence the outcome.) This equation is analogous to equation (2) from this chapter except that it does not include a vector of school-specific characteristics. (In this case, however, the vector X_{is} includes a richer set of characteristics.)

As long as we assume that there is no selection bias (that is, that the terms u_s and e_{is} are uncorrelated with the independent variables in the model), the estimation of this model is relatively straightforward. If Y_{is} is a continuous variable, OLS regression techniques will produce unbiased estimates of the impact of participation (δ). Because we do not know the extent to which there are multiple observations of students from the same schools, estimating fixed- or random-effects models with NHANES III data is not possible.[65]

Under the equation (7) specification, the model would be estimated on a sample of all NHANES III students, including those not attending SBP schools. To increase the flexibility of the specification somewhat, the participation indicator (P_{is}) could be interacted with other individual characteristics in the model. This specification would allow researchers to investigate whether eating a school breakfast had different impacts on students with different characteristics, such as age or gender.

As was the case with the ECLS-K analysis, the possibility of selection bias would have to be considered. There is possible selection bias related to the availability of the SBP in schools, parents' knowledge about the availability of the program, and parents' decisions about their children's participation in it.[66]

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[65] Furthermore, the NHANES III sample is unlikely to contain large numbers of students from the same schools. Thus, estimating fixed- or random-effects models is unlikely to be necessary in this context.

[66] Although schools participating in the SBP must notify parents, parents might not have received or might not have recalled receiving notification.

Accounting for SBP participation decisions in the analysis is important because there is a risk that nonrandom selection into the sample of SBP participants would bias estimates of the impact of SBP participation on learning. To account for selection bias, the approaches described in earlier in this chapter, such as IV models, could be used.

If we believed that selection bias was not present in the analysis, the impacts of SBP participation on nutritional and learning-related outcomes could be estimated with a reasonable level of statistical precision. Using the approach described in the previous chapter, we calculated minimum detectable difference (MDD) sizes (measured as a percentage of a standard deviation in the outcome variable) under the following assumptions:

- A design effect of 2.5
- Control variables in the regression explain about 10 percent of the variation in the outcome
- About 30 percent of the variation in students' participation status can be explained by the control variables
- Sample sizes of 1,259 SBP participants and 1,017 nonparticipants aged 6–11 years (and sample sizes of 1,816 and 1,818, respectively, for those aged 6–16 years), as reported in Table IV.6

Under these assumptions, the NHANES III analysis would have the power to detect effects of SBP participation of as low as 15 percent of a standard deviation of the outcome measure. With respect to the dietary and nutritional outcomes in particular, effects of this size would be reasonable based on other analysis of these variables (National Center for Health Statistics, 1996). Given selection bias, however, MDDs would be much larger (in the range of 0.4 to 0.9); in other words, true SBP participation effects would be much more difficult to detect statistically.

5. Summary

The analytic approach in using data from NHANES III is to describe and compare mean differences in dietary, nutritional, health, and learning outcomes among SBP participants, SBP nonparticipants, and students who do not eat breakfast. To estimate the effects of SBP participation, we propose that regression analysis be used to compare the outcomes of participants and nonparticipants after controlling for all measurable relevant factors (for example, prenatal exposure to smoke, low birthweight, iron deficiency anemia, and elevated blood lead levels).

The NHANES III analysis offers the advantage of an existing national survey of school-aged children with comprehensive information on family background, SBP participation, dietary intakes, nutritional status, and health, as well as cognitive and academic performance test scores. The NHANES III provides a framework for linking cognitive functioning, academic performance, and school behavior with SBP participation, and for assessing such intermediate variables as dietary intakes and nutrition and health status. The NHANES III analysis would capture many of the important domains necessary to link SBP participation and learning. It would therefore be important for informing and supplementing the ECLS-K design and analysis plans.

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Appendix A

Summary of Expert Panel Meeting

Mathematica Policy Research, Inc. (MPR) hosted a 1-day meeting at the offices of the Economic Research Service (ERS) on May 24, 2000, to discuss design options for studying the impact of the School Breakfast Program (SBP) on learning. All three expert panel members; two consultants; MPR project staff; ERS staff; representatives from the Food and Nutrition Service (FNS), U.S. Department of Agriculture (USDA); and representatives from the Center for Nutrition Policy and Promotion (CNPP) attended the meeting.

The meeting was held between the submission date of a draft report detailing the use of the Early Childhood Longitudinal Study, Kindergarten Cohort, (ECLS-K), to determine the effect of the SBP on learning, and the submission date of the Alternative Designs Report. The Alternative Designs Report presents four alternative designs for studying the relationship between SBP participation and academic achievement.

MPR staff presented the following four potential alternative designs at the meeting: (1) MPR's design for studying the Universal-Free School Breakfast Program (USBP), (2) various possible experimental designs, (3) a design based on the ECLS-K data, and (4) a design based on data from the forthcoming National Health and Nutrition Examination Survey (NHANES). Meeting participants were asked to comment on the designs, and to give their opinions about the most feasible and methodologically sound design of those proposed.

Before the meeting, all participants received briefing materials, including background information on the primary MPR project staff (including consultants), the nature of the work MPR is conducting for ERS, and the SBP. The materials also included a summary of the literature review and of the four alternative designs. The meeting agenda was structured around the materials.

After introductions by Mr. Jay Variyam, Dr. Phil Gleason briefly discussed the background of the study, a literature review (submitted to ERS by MPR in March 2000), and key elements of the alternative designs. More detailed presentations on the designs were by made Dr. Barbara Devaney (the USBP design), Dr. Phil Gleason (the possible experimental designs), Dr. Jon Jacobson (the ECLS-K design), and Dr. Ronette Briefel (the NHANES design). Each presentation was followed by a brief discussion. The meeting ended with a summary discussion of all the design options.

The ***USBP design*** is set up to measure the impacts of providing free school breakfasts to elementary school students, regardless of family income. The central feature of the evaluation strategy is an experimental design that first pairs 144 schools from six school districts, then randomly assigns each school in each pair to a treatment (USBP) group or a control (regular SBP) group. Thus, the treatment and control groups would each contain 72 schools.

The evaluation would collect detailed data on 30 students per school, for a total sample size of 4,320 students. Administrative records, student and parent survey data, student achievement

tests, and teacher survey data would be used to assess the impacts of the USBP on student and school outcomes.

Many topics were brought up in the discussion of the USBP design. The definition of breakfast, relevant to all designs, was mentioned as an important factor that must be determined prior to conducting an analysis. The fact that student participation in the SBP may carry a certain stigma was brought up, suggesting that it might be difficult to obtain sufficient numbers of SBP participants and to avoid "dilution." Meeting attendees also raised a related issue: parents' attitudes about the SBP might be a factor in determining a child's participation. Finally, the difference between cognitive functioning and academic achievement as outcomes was discussed.

Four options for ***experimental designs*** were presented:

1. ***Random Assignment of Students.*** Under this design, a sample of students would be selected from SBP schools and then randomly assigned to treatment and control groups. The treatment group would receive some intervention to promote SBP participation and/or the control group would receive an intervention intended to discourage or prohibit SBP participation.

2. ***Random Assignment of Breakfast Delivery Type.*** Under this design, a sample of SBP schools would be selected and then classrooms within those schools would be randomly assigned to treatment and control classrooms that would use different SBP delivery methods. For example, some classrooms would have a bag lunch delivered to class, others would go to the cafeteria and would be given the opportunity to eat at that time; a third group would be left to its own devices to obtain a school breakfast.

3. ***Random Assignment of Schools' SBP Starting Dates.*** This design would involve using the set of schools applying to USDA to participate in the SBP for the first time. These schools would be randomly assigned to a treatment group that began the SBP immediately and a control group that would be required to wait for one year before starting the program.

4. ***Random Assignment of Schools' Provision 2 and 3 Funding.*** Under this design, a set of schools applying for provision 2 or 3 funding would be selected. These schools would be randomly assigned to a treatment group that would begin the funding cycle immediately and a to control group that would not. From students' perspective, this would amount to randomly assigning treatment schools to begin offering meals free to all students and control schools to continue charging for meals as before.

The discussion relating to experimental designs centered on identifying a feasible social experiment in which students would be randomly assigned to a treatment group that would eat school breakfasts and to a control group that would not. Meeting participants raised concerns about identifying an ethical way to implement such an experiment.

Proposals ranged from randomly varying methods of breakfast delivery to varying the time of implementation of the SBP in a school. One issue focused on whether implementation of the program, rather than the program itself, would become the intervention being assessed. Meeting

participants also debated whether the experiment should examine the effect of nutrients on learning or the effect of eating school breakfast on learning. MPR argued that eating a school breakfast should constitute the intervention because it would be less feasible to randomly assign nutrient intake levels to students than to randomly assign SBP participation status. The participants also agreed that the timing and content of breakfast would be important in assessing the impact of SBP participation on learning.

Several panel members thought that, given the time required to establish the SBP in a school, obtaining the support of administrators, faculty, and parents would be crucial to successful implementation. Opinions varied, also, as to the level at which random assignment should occur (student, classroom, or school). During the discussion, it became clear that the design based on provision 2 and 3 funding applications would not be feasible, primarily because the provisions are administrative mechanisms that do not readily affect the SBP participation of children who often qualify automatically on the basis of food stamp use or receipt of Temporary Assistance to Needy Families.

The ***ECLS-K design*** proposed to use the ECLS-K dataset to investigate the link between SBP participation and learning outcomes. The ECLS-K is a nationally representative sample of students observed from kindergarten through grade five. Although the ECLS-K includes a rich set of information on learning outcomes, SBP participation, and family and school backgrounds, supplemental data collection could shed additional light on the factors affecting a school's decision to offer the SBP, and on a family's decision to let a child participate in the program.

The meeting participants agreed that it would be important to account for these participation decisions because of the risk that nonrandom selection into the sample of SBP participants would bias estimates of the impact of SBP participation on learning.

The presentation on the ECLS-K design sparked discussion about possible supplemental questions to be added to the ECLS-K questionnaire that would provide additional data for an examination of the effect of the SBP on learning. Regardless of the design, identifying an accurate measure of SBP participation can be problematic but is an even greater issue when existing data, such as the ECLS-K, are used.

The use of existing data reduces the choice of which information can be used for analysis. The group discussed various measures, as well as the ramifications of using a categorical or binary variable to measure SBP participation. The group also raised the issue of accounting for children who eat breakfast at home, as the ECLS-K does not ask any questions that provide this information. In addition, participants pointed out that the effect of participating in the SBP could be negative, rather than neutral, justifying the use of two-tailed tests of statistical significance. Finally, measures of achievement were discussed, particularly whether achievement should be measured in the absolute or as change over time.

The nonexperimental ***NHANES design*** proposed to use the NHANES dataset to study the relationship between participation in the SBP and learning outcomes. The NHANES includes a nationally representative sample of children and adolescents who are interviewed and examined in a mobile examination center. The NHANES provides a comprehensive picture of the dietary intake, SBP participation, nutritional status, and health status of school-aged children in the

United States. Supplemental data collection of cognitive, behavioral, and intelligence tests, which were successfully included in the 1988-1994 NHANES, and school records on attendance and achievement test scores, would provide information on learning outcomes. The analysis would compare dietary, nutrition, health, and learning outcomes among SBP participants, SBP nonparticipants, and students who do not eat breakfast.

Discussion about the NHANES design centered around the intricacies of using national data collected for purposes other than the one at hand. In particular, the forthcoming NHANES data collection effort will not collect data on academic achievement, so supplemental data collection would have to be added to acquire the information necessary to measure change in achievement over time.

The meeting participants also observed that resolving the issue of the lack of data on the individual schools that NHANES respondents attend would require obtaining school records long after the original data collection period—a difficult and costly enterprise. The possibility that parents might report inaccurate information about their children's dietary intake or SBP participation was mentioned. (However, this issue would be relevant to any survey.)

Another potential difficulty with a nonexperimental design such as the proposed NHANES design is that schools participating in the SBP might differ significantly from nonparticipating schools. Finally, the meeting participants cited the advantages of using existing data, namely, the low cost and the ability to use a nationally representative sample.

During the concluding discussion, the participants decided that the strongest and most feasible *experimental* design to suggest as one of the four alternative designs would be one that grouped applicants to the SBP (in other words, schools) into control and treatment groups. The control group would then have to wait one year before implementing the program.

However, the group also concluded that the most feasible design was one that was based mainly on the ECLS-K, but that used data from NHANES III. NHANES III data were collected from 1988 to 1994. The group discussed the relative merit of an analysis of NHANES III data on SBP participation, dietary intake, and WISC-R/WRAT-R scores to inform the ECLS-K design. Thus, the ECLS-K would provide information on the characteristics of students and their schools, and the NHANES III would provide dietary intake and nutritional status data on SBP participants and nonparticipants. In addition, the combination of these two datasets would supply information on both intermediate and final outcomes.

AGENDA

DESIGN AND FEASIBILITY STUDY OF THE IMPACT OF THE SBP ON LEARNING

Expert Panel Meeting
May 24, 2000
Economic Research Service
U.S. Department of Agriculture
Rm. N4166, 1800 M Street, NW (North Tower)
Washington, DC 20036-5831

8:30 – 8:45	**Welcome / Coffee and Donuts**
8:45 – 9:00	**Introductions** (Jay Variyam) Study Background (Phil Gleason)
9:00 - 9:45	**Literature Review / Key Elements of Potential Study Designs** (Phil Gleason)
9:45 - 10:30	**Summary of USBP Design** (Barbara Devaney)
10:30 - 10:45	**Break**
10:45 – 11:45	**Summary of Possible Experimental Designs** (Phil Gleason)
11:45 - 12:45	**Lunch**
12:45 - 1:45	**Summary of ECLS Design** (Jon Jacobson)
1:45 - 2:45	**Summary of NHANES Design** (Ronette Briefel)
2:45 - 3:30	**Wrapup Discussion of Four Alternative Designs** (Phil Gleason)

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Appendix B

Table B-1—Variables in the ECLS-K base-year file relevant to the design study

Variable	ECLS-K source	Variable name	Public use file availability	Fall/spring availability
OUTCOME VARIABLES				
Cognitive outcomes				
General knowledge test score present	Child survey	C1GKFLG	Yes	Yes
General knowledge routing test, number correct	Child survey	C1GROUNR	Yes	Yes
General knowledge IRT scale score	Child survey	C1GSCALE	Yes	Yes
General knowledge *T*-score	Child survey	C1GTSCOR	Yes	Yes
Count, number, shape	Child survey	C1MPROB1	Yes	Yes
Relative size	Child survey	C1MPROB2	Yes	Yes
Ordinality, sequence	Child survey	C1MPROB3	Yes	Yes
Add/subtract	Child survey	C1MPROB4	Yes	Yes
Multiply/divide	Child survey	C1MPROB5	Yes	Yes
Count, number, shape	Child survey	C1MPROF1	Yes	Yes
Relative size	Child survey	C1MPROF2	Yes	Yes
Ordinality, sequence	Child survey	C1MPROF3	Yes	Yes
Add/subtract	Child survey	C1MPROF4	Yes	Yes
Multiply/divide	Child survey	C1MPROF5	Yes	Yes
Math routing test, number right	Child survey	C1MROUNR	Yes	Yes
Math IRT scale	Child survey	C1MSCALE	Yes	Yes
Math test score present	Child survey	C1MTHFLG	Yes	Yes
Math *T*-score	Child survey	C1MTSCOR	Yes	Yes
Reading test score present	Child survey	C1RDGFLG	Yes	Yes
Print familiarity	Child survey	C1RPRINT	Yes	Yes
Letter recognition	Child survey	C1RPROB1	Yes	Yes
Beginning sounds	Child survey	C1RPROB2	Yes	Yes
Ending sounds	Child survey	C1RPROB3	Yes	Yes
Sight words	Child survey	C1RPROB4	Yes	Yes
Word in context	Child survey	C1RPROB5	Yes	Yes
Letter recognition	Child survey	C1RPROF1	Yes	Yes
Beginning sounds	Child survey	C1RPROF2	Yes	Yes
Ending sounds	Child survey	C1RPROF3	Yes	Yes
Sight words	Child survey	C1RPROF4	Yes	Yes
Word in context	Child survey	C1RPROF5	Yes	Yes
Reading routing test, number right	Child survey	C1RROUNR	Yes	Yes
Reading IRT scale score	Child survey	C1RSCALE	Yes	Yes
Reading *T*-score	Child survey	C1RTSCOR	Yes	Yes
Art show child score	Child survey	C1SCART	Yes	Yes
Simon Says child score	Child survey	C1SCORD	Yes	Yes
Tell stories child score	Child survey	C1SCSTO	Yes	Yes
Child's total OLDS score	Child survey	C1SCTOT	Yes	Yes
General knowledge ARS score	Teacher survey	T1ARSGEN	Yes	Yes
Literacy ARS score	Teacher survey	T1ARSLIT	Yes	Yes

Variable	ECLS-K source	Variable name	Public use file availability	Fall/spring availability
Math ARS score	Teacher survey	T1ARSMAT	Yes	Yes
School attendance				
Average daily attendance	School survey	S2ADA	Yes—Recoded	No
Emotional and social development				
Social interaction	Parent survey	P1SOCIAL	Yes	Yes
Approaches to learning	Parent survey	P1LEARN	Yes	Yes
Self-control	Parent survey	P1CONTRO	Yes	Yes
Sad/lonely	Parent survey	P1SADLON	Yes	Yes
Impulsive/overactive	Parent survey	P1IMPULS	Yes	Yes
Serious emotional problems	Teacher survey	A2EMPRB	Yes	No
Approaches to learning	Teacher survey	T1LEARN	Yes	Yes
Self-control	Teacher survey	T1CONTRO	Yes	Yes
Interpersonal	Teacher survey	T1INTERP	Yes	Yes
Externalizing problem behaviors	Teacher survey	T1EXTERN	Yes	Yes
Internalizing problem behaviors	Teacher survey	T1INTERN	Yes	Yes
Physical development				
Fine motor skills	Child survey	C1FMOTOR	Yes	No
Gross motor skills	Child survey	C1GMOTOR	Yes	No
Composite motor skills	Child survey	C1CMOTOR	Yes	No
Child composite height (inches)	Child survey	C1HEIGHT	Yes	Yes
Child composite weight (pounds)	Child survey	C1WEIGHT	Yes	Yes
Scale of child's health	Parent survey	P1HSCALE	Yes	No
Last visit for routine health care	Parent survey	P2DOCTOR	Yes	No
Other health impairments	Teacher survey	A2OTHER	Yes	No
Fell behind due to health	Teacher survey	T2FLBHND	Yes	No
SBP participation				
School provides breakfast	Parent survey	P2SCHLBK	Yes	No
Child eats school breakfast	Parent survey	P2CHIEAT	Yes	No
Number of school breakfasts eaten	Parent survey	P2NUMBKF	Yes	No
Number of students eligible for free breakfast	School survey	S2ELIBRK	No—suppressed	No
Number of students participating in free breakfast	School survey	S2PARBRK	No—suppressed	No

Background characteristics

Variable	ECLS-K source	Variable name	Public use file availability	Fall/spring availability
Student-level				
Child identification number	Child survey	CHILDID	Yes	Yes
Child panel weight full sample	Child survey	BYCW0	Yes	Yes
Child composite gender	Child survey	GENDER	Yes	Yes
Child composite race	Child survey	RACE	Yes	Yes
Composite child assessment age	Child survey	R1_KAGE	Yes—recoded	Yes
Child has disability	Parent survey	P1DISABL	Yes	No
Speak non-English language at home	Child survey	C1SCREEN	Yes	Yes
Number of siblings in household	Parent survey	P1NUMSIB	Yes	Yes
Types of relatives living in household	Parent survey	P1REL_1-17	Yes	Yes
Ages of relatives living in household	Parent survey	P1AGE_1-17	Yes	Yes
Types of parent in household	Parent survey	P1HPARNT	Yes	Yes
Child's biological parents are married	Parent survey	P1BIOLOG	Yes	Yes
Respondent married to nonbiological parent	Parent survey	P1MARRY	Yes	No
Respondent lives with nonbiological parent	Parent survey	P1LIVNON	Yes	No
Parent highest education level	Parent survey	WKPARED	Yes	Yes
Mother's education level	Parent survey	WKMOMED	Yes	Yes
Father's education level	Parent survey	WKDADED	Yes	Yes
Mother's current employment status	Parent survey	P1HMEMP	Yes	No
Father's current employment status	Parent survey	P1HDEMP	Yes	No
Person 1 hours per week spent working	Parent survey	P1HRS_1	Yes	No
Person 2 hours per week spent working	Parent survey	P1HRS_2	Yes	No
Categorical SES measure	Parent survey	WKSESQ5	Yes	Yes
Food in household	Parent survey	P2FOODHH	Yes	No
No food, not enough money	Parent survey	P2NFMON	Yes	No
No food, hard to get to store	Parent survey	P2NFSTR	Yes	No
No food, on a diet	Parent survey	P2NFDIET	Yes	No
No food, no working stove	Parent survey	P2NFSTV	Yes	No
No food, not able to cook	Parent survey	P2NFCOOK	Yes	No
Food choice, not enough money	Parent survey	P2FCMON	Yes	No
Food choice, hard to get to store	Parent survey	P2FCSTR	Yes	No
Food choice, on a diet	Parent survey	P2FCDIET	Yes	No
Food choice, not available	Parent survey	P2FCAVAL	Yes	No
Food choice, quality food not available	Parent survey	P2FCQUAL	Yes	No
Worried food would run out	Parent survey	P2WORRFD	Yes	No
Food does not last	Parent survey	P2FDLAST	Yes	No
Reliance on low-cost food	Parent survey	P2LOWCST	Yes	No
Not enough food, could not afford	Parent survey	P2CANTAF	Yes	No
Not enough food, lost weight	Parent survey	P2LOSEWT	Yes	No
Not enough food, children skip meals	Parent survey	P2CHSKIP	Yes	No
Not enough food, frequency skipped meals	Parent survey	P2OFTCUT	Yes	No
Not enough food, children hungry	Parent survey	P2CHIEVR	Yes	No
Received food stamps in last 12 months	Parent survey	P1FSTAMP	Yes	No
Number of months received food stamps	Parent survey	P1WHENFS	Yes	No
Child receives complete school lunch	Parent survey	P2RLUNCH	Yes	No

Variable	ECLS-K source	Variable name	Public use file availability	Fall/spring availability
Child receives free/reduced-price lunch	Parent survey	P2LUNCHS	Yes	No
Number of school lunches received in last week	Parent survey	P2NUMLNC	Yes	No
School-level				
School identification number	School survey	S_ID	Yes	No
School weight full sample	School survey	S2SAQW0	Yes	No
Census region in sample frame	School survey	CREGION	Yes	No
School type	School survey	S2KSCTYP	Yes	No
Location type in sample frame	School survey	KURBAN	Yes—recoded	No
Total school enrollment	School survey	S2KENRLS	Yes—recoded	No
Total school kindergarten enrollment	School survey	S2KENRLK	Yes—recoded	No
Number of full-day kindergarten classes	School survey	S2FDCLS	Yes—recoded	No
Number of days full-day kindergarten meets	School survey	S2FDDAYS	No—suppressed	No
Children can be retained at any grade	School survey	S2ANYGRD	Yes	No
Number of kindergarten students retained in last year	School survey	S2NMBKPT	Yes—recoded	No
Number of FTE total school staff	School survey	S2FTETOT	Yes—recoded	No
Number of FTE administrative staff	School survey	S2ADMFTE	Yes—recoded	No
Number of FTE teachers' aides	School survey	S2AIDFTE	Yes—recoded	No
Number of FTE other noninstructional staff	School survey	S2NONFTE	Yes—recoded	No
Number of FTE classroom teachers	School survey	S2TCHFTE	Yes—recoded	No
Teacher's educational level	Teacher survey	B1HGHSTD	Yes—recoded	No
Lowest annual base teacher salary	School survey	S2LOSLRY	Yes	No
Highest annual base teacher salary	School survey	S2HISLRY	Yes	No
Number of years teacher taught preschool/Head Start	Teacher survey	B1YRSPRE	Yes—recoded	No
Number of years teacher taught kindergarten	Teacher survey	B1YRSKIN	Yes—recoded	No
Number of years teacher taught first grade	Teacher survey	B1YRSFST	Yes—recoded	No
Number of years teacher taught second through fifth grade	Teacher survey	B1YRS2T5	Yes—recoded	No
Number of years teacher taught sixth or higher grade	Teacher survey	B1YRS6PL	Yes—recoded	No
Number of years as teacher at current school	Teacher survey	B1YRSCH	Yes—recoded	No
Number of years as a principal	School survey	S2TOTPRI	Yes—recoded	No
Number of years as a principal at current school	School survey	S2PRINHR	Yes—recoded	No
Number of years teaching before becoming principal	School survey	S2YSTCH	Yes—recoded	No
Hours per week principal works with teacher	School survey	S2INSTRU	Yes	No
Hours per week principal spends on management activities	School survey	S2INRMGT	Yes	No
Hours per week principal teaches	School survey	S2TEEECH	Yes	No
Hours per week principal spends with parents	School survey	S2TALKPT	Yes	No
Hours per week principal meets with students	School survey	S2STUDNT	Yes	No
Hours per week principal spends doing paperwork	School survey	S2PPRWRK	Yes	No
School requires admission test	School survey	S2ADMISS	Yes	No

Variable	ECLS-K source	Variable name	Public use file availability	Fall/spring availability
No grades tested	School survey	S2NOTEST	Yes	No
Prekindergarten tested	School survey	S2TESTPK	Yes	No
Transitional-kindergarten tested	School survey	S2TESTR	Yes	No
Kindergarten tested	School survey	S2TESTK	Yes	No
Pre-first grade tested	School survey	S2TESTP1	Yes	No
First grade tested	School survey	S2TEST1	Yes	No
Second grade tested	School survey	S2TEST2	Yes	No
Third grade tested	School survey	S2TEST3	Yes	No
Fourth grade tested	School survey	S2TEST4	Yes	No
Fifth grade tested	School survey	S2TEST5	Yes	No
Sixth grade tested	School survey	S2TEST6	Yes	No
Seventh grade tested	School survey	S2TEST7	Yes	No
Eighth grade tested	School survey	S2TEST8	Yes	No
Ninth grade tested	School survey	S2TEST9	Yes	No
Tenth grade tested	School survey	S2TEST10	Yes	No
Eleventh grade tested	School survey	S2TEST11	Yes	No
Twelfth grade tested	School survey	S2TEST12	Yes	No
Average number of years ESL services provided	School survey	S2YRSESL	Yes—recoded	No
Average number of years bilingual services provided	School survey	S2YRSBIL	Yes—recoded	No
Percent gifted/talented students	School survey	S2KGFTED	Yes—recoded	No
No gifted/talented program	School survey	S2NOGFT	Yes	No
Number of instructional computers	School survey	S2INSTCM	Yes—recoded	No
Classrooms meet needs	School survey	S2CLSSOK	Yes	No
Classrooms' space and size	Facility checklist	K2Q1A_C	Yes	No
Classrooms' lighting	Facility checklist	K2Q1A_D	Yes	No
Classrooms' ventilation	Facility checklist	K2Q1A_E	Yes	No
Classrooms' condition	Facility checklist	K2Q1A_F	Yes	No
Classrooms' temperature	Facility checklist	K2Q1A_G	Yes	No
Classrooms' noise level	Facility checklist	K2Q1A_H	Yes	No
Classrooms' handicap access	Facility checklist	K2Q1A_I	Yes	No
Cafeteria meets needs	School Survey	S2CAFEOK	Yes	No
Cafeteria's space and size	Facility checklist	K2Q1F_C	Yes	No
Cafeteria's lighting	Facility checklist	K2Q1F_D	Yes	No
Cafeteria's ventilation	Facility checklist	K2Q1F_E	Yes	No
Cafeteria's condition	Facility checklist	K2Q1F_F	Yes	No
Cafeteria's temperature	Facility checklist	K2Q1F_G	Yes	No
Cafeteria's noise level	Facility checklist	K2Q1F_H	Yes	No
Cafeteria's handicap access	Facility checklist	K2Q1F_I	Yes	No
Library meets needs	School survey	S2LBRYOK	Yes	No
Library's space and size	Facility checklist	K2Q1C_C	Yes	No
Library's lighting	Facility checklist	K2Q1C_D	Yes	No
Library's ventilation	Facility checklist	K2Q1C_E	Yes	No
Library's condition	Facility checklist	K2Q1C_F	Yes	No
Library's temperature	Facility checklist	K2Q1C_G	Yes	No
Library's noise level	Facility checklist	K2Q1C_H	Yes	No
Library's handicap access	Facility checklist	K2Q1C_I	Yes	No

Variable	ECLS-K source	Variable name	Public use file availability	Fall/spring availability
School offers before-school Child care	School survey	S2B4SCH	Yes	No
Parent-level				
Distance of child from school	Parent survey	P2DISTHM	Yes	No
Parent has met child's teacher	Parent survey	P1MTEACH	Yes	No
Current school affected home choice	Parent survey	P1CHOOSE	Yes	No
Parent selected school	Parent survey	P1SCHOOL	Yes	No
Other				
Percent free-lunch-eligible students	School survey	S2KFLNCH	Yes—recoded	No
Percent reduced-price-lunch-eligible students	School survey	S2KRLNCH	Yes—recoded	No
Siblings attend the same school	Parent survey	P2BROSIS	Yes	No
School provides lunch	Parent survey	P2SCHLLU	Yes	No
Number of students eligible for free lunch	School survey	S2ELILNC	No—suppressed	No
Number of students participating in free lunch	School survey	S2PARLNC	No—suppressed	No
Number of students eligible for reduced-price lunch	School survey	S2ELIRED	No—suppressed	No
Number of students participating in reduced-price lunch	School survey	S2PARRED	No—suppressed	No

ARS = Academic Rating Scale; ESL = English as a Second Language; FTE = full-time equivalent; IRT = Item Response Theory; OLDS = Oral Language Development Scale; SPB = School Breakfast Program; SES = socioeconomic status.

Appendix C

Original questions from the ECLS-K parent and school administrator surveys proposed to be revised or replaced

FROM THE PARENT SURVEY

PIQ280 DistHometoSchool

About how far would you say it is from your home to the school {CHILD} attends? Would you say . . .

1. Less than 1/8th mile (less than 3 blocks),
2. 1/8th mile to 1/4 miles (3–5 blocks),
3. More than 1/4 mile, but less than 1/2 mile (6–9 blocks),
4. 1/2 mile to less than 1 mile (10–19 blocks),
5. 1 mile to 2.5 miles (less than 5 minute drive),
6. 2.6 miles to 5 miles (5–10 minute drive),
7. 5.1 miles to 7.5 miles (11–15 minute drive),
8. 7.6 miles to 10 miles (16–20 minute drive), or
9. 11 miles or more (more than 20 minute drive)?

PIQ310 HowGetToSchool

How does {CHILD} usually get to school? Does {he/she}. . .

1. Walk or ride a bike.
2. Ride a bus.
3. Is (he/she) dropped off by a parent, relative, or adult friend.
4. Is (he/she) dropped off by (his/her) day care provider?

HEQ500 BkfstTogether

In a typical week, please tell me the number of days at least some of the family eats breakfast together.

WPQ150 SchoolProvideLunch

Does (CHILD)'s school offer lunch to its students?

WPQ160 ReceiveSchoolLunch

Does (CHILD) usually receive a complete lunch offered at school?

Probe: By complete school lunch, I mean a complete meal such as a salad, soup, a sandwich, or a hot meal that is offered each day at a fixed price, not just milk, snacks, ice cream, or a lunch (he/she) brought from home.

WPQ170 LunchAtSchool

Does (CHILD) receive free or reduced-price lunches at school?

WPQ180 FreeReduced

Are these lunches free or reduced-price?

WPQ190 NumCompleteLunch

During the last 5 days (CHILD) was in school, how many complete lunches did (he/she) receive?

WPQ200 SchoolProvideBkfst

Does (CHILD)'s school offer breakfast for its students?

WPQ210 ChildEatSchoolBkfst

Does (CHILD) usually receive a breakfast provided by the school?

WPQ220 NumBkfst

During the last 5 days (CHILD) was in school, how many breakfasts did (he/she) receive?

<u>From the School Administrator Survey</u>

16. How many children in your school were eligible for and participated in the following special services? Write in numbers below. If services not provided, write "zero."

	Eligible children	**Participating children**
a. Free breakfast?	_____	_____
b. Free school lunch program?	_____	_____
c. Reduced-price school lunch program?	_____	_____

Source: National Center for Education Statistics, ECLS-K Web site.

Appendix D

Table D-1—Variables in NHANES III relating to school breakfast and learning among 6–16 year olds

Variable	NHANES III data file	SAS variable name
Individual characteristics		
Sequence number (person identification number)	Household youth data file [a]	SEQN
Final interview sampling weight	Household youth data file	INTPFQX6
Examination/interview status	Household youth data file	DMPSTAT
Age	Household youth data file	HYAITMO
Race/ethnicity	Household youth data file	DMARETHN
Sex	Household youth data file	HSSEX
Country of birth	Household youth data file	HFA6XCR
Family characteristics		
Family reference person		
Education level	Household youth data file	HFHEDUCR
Country of birth	Household youth data file	HFC10, HFC11
Employment	Household youth data file	HFHEMPL
Marital status	Household youth data file	HFA12
Occupation	Household youth data file	HFD9R, HFD10R, HFD12
Health insurance coverage	Household youth data file	HFB1-HFB17
Source of medical care	Household youth data file	HYD2, HYD3, HYD4
Household size	Household youth data file	HSHSIZER
Housing characteristics	Household youth data file	HFE1-HFE18
Income in past 12 months	Household youth data file	HFF18, HFF19R, HFF20R
Poverty income ratio	Household youth data file	DMPPIR
Food sufficiency	Household youth data file	HFF4-HFF18
Family-level food program participation		
WIC participation in last month	Household youth data file	HFF9
FSP participation in past 12 months (number of months)	Household youth data file	HFF10
Current FSP participation	Household youth data file	HFF11
Individual-level food program participation		
Current WIC participation	Examination data file	MYPC22
WIC in last 12 months	Examination data file	MYPC21
Number of months in WIC	Examination data file	MYPC23S
School meals		
School offers NSLP?	Household youth data file	HYJ11
Times per week child has free, reduced-price, or full-price lunch	Household youth data file	HYJ12
School offers SBP?	Household youth data file	HYJ9

Variable	NHANES III data file	SAS variable name
Times per week child has free, reduced-price, or full-price breakfast	household youth data file	HYJ10
School outcomes		
Grade level	Household youth data file	HYJ8
Skipped grades	Household youth data file	HYJ15
Repeated grades	Household youth data file	HYJ16
Suspensions and expulsions	Household youth data file	HYJ19, HYJ20
Attendance	Household youth data file	HYJ14
Health problems affecting school attendance	Household youth data file	HYD9-HYD11
Health interview data		
Reported medical conditions	Household youth data file	HYE1A-HYE9B
Whether mother smoked during pregnancy with child	Household youth data file	HYA3-HYA5
Birthweight	Household youth data file	HYA7S, HYA8, HYA9, HYA94
Newborn intensive care	Household youth data file	HYA6A, HYA7S
Need for corrective lenses	Household youth data file	HYH2, HYH3, HYH4
Hearing trouble	Household youth data file	HYH10-HYH13
Parent's report of child's health	Household youth data file	HYD1
Behavioral problems	Household youth data file	HYE10, HYE11
Headaches	Household youth data file	HYE12
Stomachaches	Household youth data file	HYE13
History of lead poisoning	Household youth data file	HYE16-HYE20S
Health-related behaviors		
Physical activity	Examination data file [a]	MYPA1, MYPA2
Limitations in activities	Household youth data file	HYD8
Time spent watching TV	Household youth data file; examination data file	HYJ23; MYPA3
Smoking	Examination data file	MYPB1-MYPB27B
Alcohol and drug usage	Examination data file	MYPF1-MYPF12
Cognitive tests		
WISC-R, block design	Examination data file	WWPBSCSR
WISC-R, digit span	Examination data file	WWPDSCSR
WRAT-R, math scores	Examination data file	WWPMRSR, WWPMSSR, WWPMSCSR
WRAT-R, reading scores	Examination data file	WWPRRSR, WWPRSSR, WWPRSCSR
Dietary intake and behavior		
24-hour recall status, Day 1	Total nutrient intake file [b]	DRPSTAT
24-hour dietary recall total nutrients (Day 1)	Total nutrient intake file	DPPN---- (series of nutrients)
24-hour recall status, Day 2	Second exam dietary recall data file [c]	DRRSTAT

Variable	NHANES III data file	SAS variable name
24-hour dietary recall total nutrients (Day 2, subsample)	Second exam dietary recall data file	DRRN---- (series of nutrients)
Use of dietary supplements	Household youth data file	HYKA1A, HYK8SP
Frequency of breakfast	Examination data file	MYPE1
Individual food sufficiency	Examination data file	DRPQ5-DRPQ11
Blood and urine determinations related to nutrition or health status		
Iron status (hemoglobin, hematocrit, serum iron, total iron binding capacity (tibc), serum ferritin, transferrin saturation)	Laboratory data file [a]	HGP, HTP, FEP, TIP, PXP, FRP
Serum and RBC folate	Laboratory data file	FOP, RBP
Serum vitamin E	Laboratory data file	VEP
Serum vitamin A and retinyl esters	Laboratory data file	VAP, PEP
Serum vitamin C	Laboratory data file	VCP
Serum carotenoids	Laboratory data file	ACP, BCP, BXP, LUP, LYP
Selenium	Laboratory data file	SEP
Serum vitamin B_{12}	Laboratory data file	VBP
Cotinine (measures exposure to passive smoke or cigarettes)	Laboratory data file	COP
Total, HDL-, and LDL-cholesterol	Laboratory data file	TCP, LCP, HDP
Lead	Laboratory data file	PBP
Urinary cadmium	Laboratory data file	UDP
Health examination data		
Final examination sampling weight	Household youth data file	WTPFEX6
Height	Examination data file	BMPHT
Weight	Examination data file	BMPWT, BMPWTLBS
Body mass index	Examination data file	BMPBMI
Bioelectrical impedance analysis	Examination data file	PEP12A1, PEP12B1
Average systolic and diastolic blood pressure	Examination data file	PEPMNK1R, PEPMNK5R

FSP = Food Stamp Program; HDL = low-density lipoprotein; LDL = low-density lipoprotein; NSLP = National School Lunch Program; RBC = red blood cell; SBP = School Breakfast Program; WIC = Women, Infants, and Children; WISC-R = Weschler Intelligence Scale for Children, Revised; WRAT-R = Wide Range Achievement Test, Revised

[a] Source: NHANES III Household Youth Data File, Examination Data File, Laboratory Data File, CD-ROM Series 11, No. 1A, July 1997.

[b] Source: NHANES III Total Nutrient Intake (EXAMDR) Data File, CD-ROM Series 11, No. 2A, April 1998.

[c] Source: NHANES III Second Exam Dietary Recall Data File, CD-ROM Series 11, No. 3A, July 1999.