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Assessing the overall benefits of programs enhancing human capital and equity: a new method with an application to school meals

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ABSTRACT

Poverty reduction and nutrition are often joint outcomes of many public policies and programs which have education as their primary outcome. Quantification of overall benefits for these programs in a common metric is challenging. We propose a new method to incorporate distributional benefits from poverty reduction into standard education economic evaluations. We apply this to a randomized controlled trial (RCT) evaluating a large-scale school feeding program in Ghana. We first map effect sizes from the RCT in learning-adjusted years of schooling. We then convert these into long-term monetary gains from increased learning, to which we finally add the distributional benefits under different scenarios of inequality aversion preferences. We show that the program has substantial long-term economic gains. While these primarily stem from improved human capital, depending on different scenarios, up to half of total benefits are driven by current gains from the social protection transfer. Beyond school meals, our methodology is relevant to programs that have impacts covering both human capital and distributional benefits, and to economic evaluations beyond education.

1. Introduction

Education, nutrition, and poverty reduction are joint outcomes of many education, health, or social protection programs, including scholarships and fee waivers, school meals, cash and food transfers, and deworming. Yet, the overall economic value of these programs – including productivity gains from human capital formation and equity from poverty reduction[1] – is hard to quantify in a single metric. This general challenge is especially salient to the educational sector, as school-based programs have benefits that often go beyond schooling and learning (Verguet et al., 2023). One prominent example relates to school meals, a program that daily reaches around 418 million children globally (WFP, 2022). Existing evidence shows that these programs support education, health, and poverty reduction (Alderman et al., 2024; Drake et al., 2017), often with larger benefits accruing to most disadvantaged groups (Aurino et al., 2023; Lundborg et al., 2022; Ruffini, 2022; Gordanier et al., 2020).

A large literature in education has focused on evaluating program effectiveness from randomized controlled trials (RCTs), with particular attention to ascertaining the magnitudes and probabilities of the effect sizes as well as their internal and external validity (Evans & Yuan, 2022). Benefit-cost analyses (BCA) are frequently employed to assess the effectiveness of such programs. To do so, one must account for multi-dimensional benefits under a common unit, typically currency. Education BCAs typically include benefits that focus on schooling rather than learning, a related - but likely more relevant - measure of skills driving returns to education (Filmer et al., 2020; Hanushek & Woessmann, 2007). Only occasionally are researchers able to track the test scores of individuals from early education to their adult years and thus calculate a money metric for BCAs based on a measure of learning (Barnett & Masse, 2007; Bartik et al., 2012). Further, just a handful of studies has quantified quality- or learning-adjusted years of schooling (LAYS) and applied these to BCAs (Gelli et al., 2014; Angrist et al., 2023).

Frequently, RCTs include a prespecified plan to measure the heterogeneity of impacts over disadvantaged subgroups. It is often difficult, however, to assess what well measured effect sizes for subgroups imply

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[1] For clarity, we offer definitions of productivity, equity, distributional benefits, and transfers in Section 2.

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for underlying objectives of social welfare and equity (OECD, 2012). Thus, to account for such distributional effects, one not only requires a metric to convert effect sizes from RCTs to gains from human capital improvements, but also a similar measure to capture the welfare contribution in terms of the poverty and inequality reduction attributable to the intervention. This is a challenge common to a range of policies and programs that encompass productivity and equity gains, as well as non-monetary improvements in health (Ferranna et al., 2024). BCAs that do not quantify distributional effects within the net benefits measure implicitly assign no value to equity (Alderman et al., 2019; Coady, 2000).

We tackle these limitations by illustrating a methodology that allows for a comprehensive welfare assessment of the impacts of educational interventions that affect both future productivity through gains from improved learning as well as distributional benefits directly stemming from the program. First, we model effect sizes on learning from RCTs into anticipated increased lifetime earnings by exploring two conceptually related, but empirically distinct, methods presented in Evans and Yuan (2019). While one approach utilizes specialized data on adult skills, the other relies on data collected within the RCT and commonly available wage regressions. In doing so, we focus on increased productivity due to better skills acquired in the school years rather than focusing on the number of years accrued. We then extend the BCA framework by including distributional benefits from the program. We employ a social welfare function introduced by Atkinson (1970) that sums up individual welfare weighted by society's aversion to inequality, if any. As discussed further below, although this approach has been employed to study a range of policy choices, a key issue to application to BCAs is to obtain a cardinal value that reflects the societal aversion to inequality, which is not directly observed. We approach this issue by employing plausible simulations based on the distribution of income of program beneficiaries relative to the national distribution with alternative parametrized social welfare functions (Alderman et al., 2019).

We outline our methodology stepwise to allow for easier replication in assessing the total economic value of a range of interventions that span multiple benefits. As an illustration, we draw on experimental data from a large-scale RCT of a national school feeding program in Ghana (Aurino et al., 2023; Gelli et al., 2019). School meals offer an appropriate example to illustrate our approach, as they share many common features with other educational and social programs affecting multiple outcomes. There is extensive evidence on the effectiveness of school meals on different domains (Drake et al., 2017), including long-run incomes (Lundborg et al., 2022). These goals are often reflected in policy formulation and budgeting. For instance, across the 183 school meal programs captured in the 2021 Global Survey of School Meal Programs, 93 % and 82 % report an objective to meet nutritional and/or health goals, and educational goals, respectively; 71 % of these programs aim to serve as a social safety net. Among the 125 countries with large-scale school feeding in this survey database, a dedicated line item for school feeding was present in 69 % of the national budgets (GCNF, 2022).

While the prevalence of school feeding programs reveals their political popularity, due to the multidimensionality of the outcomes they affect, it is often hard to ascertain the *overall economic value* of school meals - both in absolute terms and relative to alternative public programs. School meals may not be cost-effective relative to specific sectoral investments in any single dimension - for example, compared to programs to target instruction at students' current learning levels or structured lessons plans for teachers (Angrist et al., 2023) - although plausibly the joint benefits may prove as, or even more, effective than other investments under a common evaluation metric. In turn, this challenge hampers resource mobilization among donors and policymakers (Watkins et al., 2024).

This paper contributes in different ways to existing literature. First, we add a new methodology to a growing literature aimed at supporting policy decision-makers by linking causal inference estimates to comprehensive welfare analysis (Alderman et al., 2019; Bhattacharya & Komarova, 2021; Finkelstein & Hendren, 2020). We provide a measure of cost-effectiveness of interventions by linking them with estimates of effect sizes from a RCT. We extend the estimation of a BCA to include a measure of equity that can be aggregated with the value of human capital. Previous literature has not included the distributional benefits stemming from such programs even when they are targeted to low-income populations and thus have revealed a social preference for some degree of redistribution (Turkson et al., 2020; Fernandes & Aurino, 2017; Verguet et al., 2020). This adds a key, yet often neglected dimension – *equity* – to the body of work evaluating the effectiveness of educational interventions (Angrist et al., 2020; Evans & Yuan, 2019, 2022; McEwan, 2015). Equity considerations are particularly important in our example, as the RCT was designed around a government policy change that retargeted the existing school feeding program to more disadvantaged districts in Ghana.

Beyond school meals, this approach is broadly applicable to a range of educational or other programs that have multiple benefits spanning human capital improvements and equity gains, such as, for instance, cash transfers or health programs (Ferranna et al., 2024). Within economics, welfare maximization and equity are often framed as opposing considerations (Okun, 1975). In contrast, our approach integrates both by explicitly incorporating the utility impacts of equity-enhancing policies into the welfare estimate of the efficiency gains stemming from human capital improvements.

Second, we contribute to the school feeding literature by incorporating learning gains into program BCAs, which often focus only on enrolment, and by offering estimates on the *overall* value of school meals. With regards to the first issue, as gross primary school enrollment is approaching universal participation in low- and middle-income settings, a focus on enrollment may overlook broader program impacts on human capital. While the evidence indicating that school feeding programs increase enrollment is extensive, in many settings that contribution is a declining component of the aggregate benefits of school meals in the 21st century. Indeed, this is a generic issue with the assessment of a wide range of education programs and policies, which are shifting their focus from schooling to learning (Angrist et al. 2020). With regards to equity, we show substantial economic gains from the transfer, especially for the poorest groups. These gains stem primarily from future earnings from improved learning, but also include up to more than half of distributional benefits depending on different scenarios related to society's inequality aversion, including a program retargeting simulation. Crucially, although the current analysis is based on an RCT, the intervention studied is not a pilot but rather a fully scaled program administered by the government, which is key to contextualize our findings to the broader evidence and policy debate on school meals. This is because most school meals globally are implemented and financed by local governments (WFP 2022), and usually scaled programs may be less effective than pilots/small programs (List 2022).

2. Methodology

This section offers a step-by-step summary of the approach to estimating comprehensive BCA returns to program impacts that include discounted human capital gains and current (non-discounted) returns to poverty reduction. Before we describe the methodology, we define some terms drawing upon public economics and the closely-related field of cost-benefit analysis. *Productivity* is an individual's capability to translate human capital into output. *Distributional benefits* come from reallocating societal resources to provide essential goods and services to a specific subgroup. This is often motivated by equity concerns. In the context of the Atkinson welfare measure, which we draw upon for our methodology, *equity* is typically evaluated based on the distribution of consumption or expenditures across individuals in a society (Atkinson, 1970). The Atkinson welfare measure is particularly useful because it explicitly incorporates inequality aversion, meaning it allows policymakers to weigh the welfare of poorer individuals based on different values of societal inequality aversion preferences (see 2.3 for further discussion). One form of distributional benefits come from direct *transfers*, where goods or cash are provided to a subgroup. Alternatively, these benefits can take place through social services, like healthcare, without direct financial transfers. In our example of school meals, distributional benefits and transfers are used interchangeably, since the provision of an in-kind good (food) is the main feature of the school feeding program. School meals have both immediate distributional benefits, reflecting societal aversion to inequality, and long-term productivity benefits through improved human capital. Together, the distributional and net present values (NPV) of productivity benefits from school meals represent the *total benefits* from the program, which we set out to estimate by following the steps described in this section.

2.1. Step 1: converting program impacts into estimates of long-run wage increases

In this step, we convert program effects on learning into estimates of long-run wage increases.

We focus on LAYS, a measure that adjusts years of schooling with a measure of school quality, i.e. the learning outcomes per year of schooling (Filmer et al., 2020). While LAYS was not initially designed for program evaluations, Evans and Yuan (2019) propose two approaches to measuring LAYS resulting from program exposure. The first method regresses a measure of learning for a representative sample of the labor market against their earnings, if data on both learning and earnings are available for the same individual (Step 1.A).

The second, alternative, method is more widely applicable given a general lack of data on learning for laborers. This approach (Step 1.B) has the advantage of relying on treatment effects from RCTs – as in our case – or school surveys (e.g. by measuring the value-added of learning throughout a school year), and combining these outcomes with the commonly used Mincerian regression[2]:

$$Log(Earnings) = b_1 * years\_education \tag{1}$$

where *years_education* is the number of completed years of schooling for the worker. However, total years of education is not a good measure of learning-adjusted years of schooling (LAYS), *years_education_LAYS*, as ideally, we want to regress:

$$Log(Earnings) = b_2 * years\_education\_LAYS \tag{2}$$

In the absence of a direct measure of *years_education_LAYS*, we need to scale program effects in terms of LAYS so that the latter can be entered into a Mincerian regression. In other words, if the program boosts learning by n compared to that of a counterfactual group, normalized as 1, a year of schooling now provides the same learning as $1+n$ years achieved without the program. That is, n is the "years equivalence" of the learning increment attributed to the program.

$$LAYS = \left(\frac{1}{1+n}\right) * years\_education, \tag{3}$$

We would expect that $b_1 < b_2$. Then an estimate of b_2 is $(1+n) * b_1$ and b_1 can be applied to n to get the program impact in terms of expected earnings. Two caveats are worth noting here. First, this approach assumes that the increment to learning is linear and applies equally to all years of schooling. As the evaluation of the GSFP we use for our illustration only covered two years, we modify years of schooling in an additive manner and not as a proportional scaling of years. Second, *LAYS* is relative to the counterfactual derived from the progression of test scores in the control group of the RCT and not the international standardization used in Filmer et al. (2020).

2.2. Step 2: estimating net present values (NPV) of lifetime human capital increases due to program exposure

Following the calculation of LAYS, we estimate the NPV of returns to human capital due to program exposure over a lifetime. These capture the *productivity benefits* from program exposure. A key assumption is that such productivity gains correspond to human capital increments that have benefits that accrue over the student's working lifetime.[3] Additional assumptions are required to estimate NPVs. We assume wages will be earned from age 20 to 60, as in Evans and Yuan (2019). Moreover, as there is no consensus rate at which the future stream of benefits is discounted, results over this span of years can be sensitive to the choice of discount rates often with profound implications across generations (Stern, 2008). We employ three scenarios combining different discount and economic growth rates to test the sensitivity of our results to different assumptions. In the first scenario, we follow standard BCA guidelines (Robinson et al., 2019) and use a constant annual discount rate of 3 percent and no real income growth. The second and third scenarios assume a higher discount rate of 8 percent noting that Ghana, like many low-and-middle-income countries, has high projected per capita growth rates warranting higher social discounting under the Ramsey equation (Haacker et al., 2020). In the second scenario, we recognize that worker productivity increases with capital as well as with the skills of the labor force. Thus, it is not uncommon to assume future labor earnings increase together with estimated real economic growth (Hoddinott et al., 2013; Angrist et al. 2023; Wong & Randin, 2019). Therefore, in the third scenario we also assume wages grow in tandem with real per capita income growth rates based on projections of the Shared Socioeconomic Pathway (middle-of-the-road scenario) supplied by the International Institute for Applied Systems Analysis (IIASA) (Riahi et al., 2017).[4]

Based on these assumptions, we can estimate the NPV of human capital returns from program exposure summing over each student (subscript i) with the following equation:

$$NPV_i = \sum_{k=20-a_i}^{n=60} \frac{\Delta L * b_1 * w_{j,t}}{(1+d)^{k+1}} \tag{4}$$

Where:

ΔL =schooling-equivalent treatment effect of school feeding (LAYS)

$b_{1,j}$: estimate from a Mincerian equation of returns to schooling in country *j,* which in this case is Ghana

$w_{j,t}$: average wage at time t[5] for country j

k: years between start of work life and age of the child at endline

n: number of years in the workforce

a_i: age at which child i is receiving school feeding

d: discount rate

These values are then summed across all children that report receiving the program. In our sample of children receiving school meals, we analyze the evaluation endline data to estimate the NPV of human

[2] The pros and cons of this standard methodology are reviewed in Patrinos (2024). For application to an RCT of students, we require an assumption that the observational results of current workers maps to students in a younger cohort. We return to this in the concluding section.

[3] We do not include productivity gains from nutrition to avoid double-counting as these often operate through learning gains. We expand on this choice in the discussion section.

[4] IIASA provides country level projections of real GDP and population for every 5th year until 2100. To convert to an annual time series of growth rates $g_{t,j}$:, we assume constant GDP and population growth rates between each 5-year interval.

[5] We note that wages $w_{j,t}$ may change over time. In the first and second scenarios laid out in the Step 3, wages are constant, but in the third they may change based on income growth, such as: $w_{j,t+1} = w_t(1+g_{j,t})$, where $g_{j,t}$ is the income growth rate at time t for country j.

capital gains from school feeding (see Section 4.2).

2.3. Step 3: estimating current (non-discounted) distributional benefits and total NPVs from program exposure

This step focuses on measuring the current, non-discounted distributional benefits stemming from the social safety net function of school meals. We then add this quantity to the NPV estimated so far, so that a measure of total NPV of program benefits (distributional benefits plus productivity returns) can be computed. To assess distributional benefits, we use an additive social welfare function in which social welfare, W, is summed over welfare, x_i, of the N individuals in the society.

$$W = \sum_{i=1}^{N} \frac{x_i^{1-\varepsilon}}{(1-\varepsilon)},\ \varepsilon \neq 1;\ ln\ W = \sum_{i=1}^{N} ln\ x_i,\ \varepsilon = 1 \tag{5}$$

An important characteristic of this welfare function is that a single inequality aversion parameter, ε, indicates society inequality aversion (Atkinson, 1970). If $\varepsilon = 0$ society places no value on distributional issues, while higher ε implies greater inequality aversion. This welfare function also has the property that the ratio of marginal social utility of two individuals is the reciprocal of the ratio of their welfare raised to the power of the inequality aversion parameter, ε. Atkinson points out that the origin of this social welfare function stems from the convexity of utility under the assumption of constant relative risk aversion:

$$\frac{\partial W/\partial x_i}{\partial W/\partial x_j} = (x_j/x_i)^{\varepsilon} \tag{6}$$

Since x_j/x_i is greater than one whenever the *ith* individual is poorer than the *jth*, all values of $\varepsilon > 0$ imply that social welfare increases at a faster rate with an increase of welfare for the *ith* individual than an equal increase in welfare for the *jth*. For this study, we will measure welfare in terms of income so that as the inequality aversion parameter increases, distributional gains from richer to poorer individuals also increase.

Eq. (5) is often applied to assess the distributional effects of fiscal policies (Coady & Skoufias, 2004; Deaton, 1997). As mentioned, the key parameter, ε, is not directly observed. However, a reluctance to consider the value of reduced inequality due to this parameter not being measured ignores the fact that zero is also a number and one that is inconsistent with the effort taken to target transfer programs. Thus, it is useful to offer a range of simulations to assess the sensitivity of results to the choice of ε. Deaton (1997) and Skoufias et al. (2010) present results using value of ε ranging from 0 to 2. Behrman and Birdsall (1983) attempt to estimate ε from the revealed behavior of public educational investments in Brazil, obtaining an estimate of 0.7. We consider this a reasonable value and use it as our main value of ε, although we include a range of alternative values in sensitivity analyses.

While many studies employing social welfare functions focus on relative inequality, to aggregate the benefits from education we require a cardinal rather than ordinal measure. That is, we need to be able to value changes in income distribution[6] in the same metric as the wage gain and the cost of the program. This necessitates additional assumptions. Following Alderman et al. (2019) we set x_j to mean income and then also set $\delta_W/\delta x_j = 1$ to generate values for $\delta_W/\delta x_i$ for any given value of ε. Benefits are estimated based on:

$$D = \sum_{i=1}^{N} v\left(\frac{x_j}{x_i}\right)^{\varepsilon}, \tag{7}$$

where v is the transfer value.

[6] In the application, we operationalize income with consumption expenditures. These are considered a good proxy of lifetime income (Deaton 1997).

2.4. Step 4: estimates of program costs and calculation of benefit:cost ratios

Once we have computed the estimates of long-run and current gains from the program, the next step is to measure program costs, so that benefit:cost ratios (BCRs) can be finally calculated. We offer details on the costs of school meals in Section 3.4. We note that in Ghana, as in most middle-income countries, school feeding and other educational or social programs are mostly financed by the central government (WFP, 2023). Thus, we include a deadweight cost for raising revenue of 25 % in the denominator of the BCA in keeping with the range reported by Auriol and Warlters (2012).

3. Background and data

This section outlines the policy context and the data used to illustrate our method.

3.1. Context: evaluation of the ghana school feeding program and how it relates to previous literature

School meals are one of the most pervasive forms of social protection globally (Alderman et al., 2024), with governments mostly financing such programs (World Food Program 2022). A broad experimental and quasi-experimental literature has shown that school meals can positively improve a range of outcomes, including child schooling, nutrition, cognition, and learning. Appendix 1 presents a list of experimental studies of the impact on school meals on learning, with a brief discussion of key studies. As noted earlier, school meal programs offer a pertinent example to illustrate our method as the multidimensionality of outcomes affected by the program challenges the evaluation of overall benefits, including the long-term productivity gains from human capital accumulation and the current distributional benefits from the transfer. This gap motivates our contribution.

We draw on data from an experimental evaluation of the Ghana School Meals Program (GSFP) (Aurino et al., 2023; Gelli et al., 2019), which we present in more detail in the next section. The program started in 2005 with a four-year pilot and later expanded and mostly integrated into the government annual budget.[7] It currently reaches 3.8 million children across all Ghana (Watkins et al., 2024). GSFP coordination and implementation are undertaken by a National Secretariat with program oversight provided by the Ministry of Gender, Children, and Social Protection. The provision of meals is decentralized, with private caterers being awarded contracts by the GSFP to procure, prepare, and serve a hot lunch to pupils in the targeted schools.

Compared with most literature presented in Appendix 1, our focus on the GSFP has the advantage of showing the effects on learning of a *government-run* program *at scale*, over a *longer duration* compared with many other studies (two years), and in the context of *almost universal primary schooling* for the age cohort. All these features are important for contextualizing our results to the broader implementation of school meals in the 21st century. To elaborate, most existing studies in the context of low- or middle-income countries typically focus on *small pilots or programs*, run either by international NGOs, the World Food Program, or researchers. Yet, such relatively small scale-programs may offer larger estimates of treatment effects on children's learning compared with programs run by governments at scale, due to financial or logistical difficulties, general equilibrium effects or endogenous political economy reactions (List 2022; Aurino et al., 2023 for an in-depth discussion related to school meals). Moreover, existing studies mostly evaluate programs in the *short-term*, although large heterogeneities in program effects by duration of exposure may be present (King & Behrman 2006;

[7] Since 2021, the program is also co-financed by the World Bank (https://projects.worldbank.org/en/projects-operations/project-detail/P175588).

Chakraborty & Jayaraman, 2019). One important exception is the study of the Indian Midday Meals Program run by Chakraborty and Jayaraman (2019), which evaluates the largest school program in the world and up to five-year of exposure using a quasi-experimental methodology and offer estimates of the cost of achieving learning gains. The authors also note that while there was no effect after four months of implementation, impacts increased steadily with program exposure, with learning effects after four years that were three-time larger than initial effects. In addition, the GSFP offers an appropriate context to evaluate whether the effects on learning of school meals are distinct from compositional changes in pupil intakes, as baseline enrolment rates were close to universal for the average pupil. We describe this experiment in the next section.

3.1.1. RCT

By coordinating with the government's plans to expand the program to new districts based on a retargeting exercise conducted in 2012, an RCT assessed program effects on child learning and nutrition. The decision to retarget the GSFP was driven by evidence that the program overwhelmingly benefited non-poor households, with only 21 % of benefits accruing to poor families (we return to this point in Section 5) (Aurino et al., 2023).

A restricted randomization procedure that modeled selection using a set of school- and village-level variables allocated communities to school feeding or control groups (for details, see Gelli et al. 2016). The survey design and data collected in the RCT are published in detail elsewhere (Aurino et al., 2023; Gelli et al. 2019). To briefly reiterate the main features of this data, impacts were assessed using a baseline and endline surveys undertaken between June and September 2013, and February-March 2016 in 92 communities. Implementation in most communities started in the academic year 2014/15, thus the program was evaluated after roughly two academic years of implementation. Ninety-two percent of children of target-age (5–15 years) and eligible to receive school feeding were reinterviewed in the second round of data collection. This resulted in a longitudinal sample of 3170 children.

With regards to average treatment effects on learning, after two years, the program led to moderate average increases in standardized math (effect size, e.s.: 0.15, $q < 0.1$), literacy (e.s.=0.13, $q < 0.1$) and composite learning scores (e.s.= 0.17, $q < 0.05$). The program had larger learning impacts for girls and for children from poorer households and regions. The effect size in the improvement in the composite learning score for girls was 0.27 ($q < 0.05$) while it was 0.33 ($q < 0.01$) for children from the poorest households and 0.30 ($q < 0.1$) for children in the northern regions (Aurino et al., 2023). Effects were only significant for children aged 5–11 years (most of the sample), as they were more likely to attend primary schools, where meals were served (Appendix 6, Aurino et al., 2023). Although we mostly focus on learning in this study, we note that the program had no average effect on nutritional indicators. However, the GSFP had significant effects on height-for-age z-scores (a marker of chronic malnutrition) of girls (e.s.= 0.12, $p < 0.05$) and for young children in households living below the poverty line (e.s.= 0.22, $p < 0.05$) (Gelli et al., 2019). We return on these distributional effects on nutrition in the discussions.

3.2. STEP dataset

As outlined in Section 2.1, it is possible to regress directly learning into earnings if data are available (Step 1.A). One of the few datasets that facilitates this exercise is the World Bank's Skills Towards Employability and Productivity program (STEP), conducted in five countries. Using the STEP data for Ghana, Evans and Yuan (2019) found a 17.8 % increase in earnings with a one standard deviation improvement in literacy test scores in Ghana. This result was the second lowest of the five country estimates reported from STEP surveys and only half of the pooled estimate. Moreover, it was not statistically significant. Nevertheless, we use this point estimate of 17.8 as a base case in our calculations of the expected economic returns of the GSFP.

3.3. Ghana living standard measurement survey 7

As the GSFP was targeted on district poverty and food insecurity, the evaluation data cannot provide a benchmark for assessing the distribution of the benefits within the larger Ghanaian population. Thus, we utilize the Ghana Living Standards Survey 7 (GLSS7) (Ghana Statistical Service, 2018) collected in 2016–17 to assess how the expenditure levels of sample households whose children received school feeding (N = 931) compared to that of the general population of Ghana. The GLSS is conducted at regular time intervals since 1987 to assess the living conditions and well-being of the Ghanaian population. It is one of the primary tools used to monitor poverty and inequality trends, and includes data on demographics, consumption expenditures, education, employment, and health, among others. The GLSS7 relies on a nationally representative sample of 140,009 households, which was selected through a two-stage sampling procedure.[8] We inflate all values related to consumption (from both the 2013 impact evaluation data and the 2017 GLSS7) to 2018 using a GDP deflator (2010 = 100) (World Bank, 2021). We use this dataset also to measure the logarithm of total monthly earnings from wages across the agricultural, industry, and service sectors (N = 4.613).

3.4. Data on GSFP transfer value and costs

We calculate the value of the transfer implicit in the school meal provision as GHS 1.5 per day under the assumption that households tend to value school feeding at a higher value than its budgetary costs to the government at the time of the evaluation (GHS 1.1). There are various reasons to justify such an assumption. First, there are scale economies in food contracting and preparation. Indeed, food assistance commonly take these economies of scale into account (Clay, 2005; Lentz & Barrett, 2008). Thus, it is plausible that households evaluate the implicit transfer of school meals differently from the implementation costs of governments or international organizations. In surveys in both Armenia and Ghana, households that have access to school meals reported their assessment of the transfer (comprising of budget and time savings in meals preparation) above the cost to the government for provision[9] (Bakhshinyan et al., 2019; Fernandes et al., 2017; Abreh et al., 2024). In Armenian households where a student was not eligible for a school meal the median reported daily cost of meals purchased exceeding what the program cost by the same amount as was reported for out-of-pocket costs of non-recipients (Bakhshinyan et al. 2019), while a recent study focusing on the school meals program in Ghana showed that the opportunity cost associated with preparing meals at home drove parental preference for school meals over cash or in-kind equivalents. Also, it highlighted that most food insecure households preferred on-site meals over cash transfers due to inflation (Abreh et al. 2024). Second, the value of GHS 1.5 is in line with other evidence from the GLSS7 which indicates that mean per capita food consumption was GHS 11.6 per day or a ballpark figure of GHS 4–6 per meal per person. If children eat half as much as the average adult, then GHS 1.5 seems a reasonable figure.

For calculating program costs, we start by a budgetary allocation of GHS 1.1 per child per day in direct costs paid to caterers over 400 hypothesized feeding days over the two years of the evaluation.[10] We then add opportunity costs, as students and teachers spend more time at school – roughly 20 min per day on average due to school feeding. Half

[8] For more information, see: https://catalog.ihsn.org/catalog/7967#metadata-sampling

[9] The convenience yield and the reallocation of time in household tasks distinct from the transfer might partially explain the popularity of school meals programs where food security is not a major policy concern.

[10] This is a likely overestimation of actual financial costs as the program faced disbursement challenges during the implementation period.

Table 1
Summary of data used and rationale.

Metric	Value	Units	Source	Rationale
Benefits				
Increase in *literacy* test scores from exposure to two years of school feeding (SD test scores)	0.13	SD	Aurino et al. (2023)	This is one of the key parameters required to estimate long-run productivity gains in Step 1.A i.e. program treatment effects on *literacy* are multiplied by the 17.8 % increase in wages per SD of literacy among adults for Ghana reported by Evans and Yuan (2019)
Percent increase in wages per 1 S.D. improvement in test scores	17,8	%	Evans and Yuan (2019)	This parameter linearly translates program impacts to a percentage increase in lifetime wages for beneficiaries as per Step 1.A
Increase in *composite* test scores from exposure to two years of school feeding (SD test scores)	0.17	SD	Aurino et al. (2023)	This is one of the key parameters required to estimate long-run productivity gains in Step 1.B i.e. program treatment effects on *composite test scores* are multiplied with the equivalent years of schooling estimated for the control group in the absence of the program.
Estimated household value of one meal	1.5	GHS	Authors' estimate based on Fernandes et al. (2017) and Bakhshinyan et al. (2019)	This parameter multiplied by the number of days of meals, is the transfer value used to measure program redistribution benefits in Step 3 (v in Eq. (7)).
Mean p.c. consumption in national data	3843	GHS	Analysis of GLSS7 data	To measure program distributional benefits in Step 3 (x_j in Eq. (7)).
Costs				
Cost per pupil (2018 GHS)	1.1	GHS	Calculation cost	Budgetary allocation per child per day for the GSFP. Used to calculate program costs in Step 4
Extra hours per day spent in school due to school feeding	20	minutes	Aurino et al. (2023)	Used to calculate opportunity costs of additional schooling induced by the program for children and teachers in Step 4
Wage rate of primary school graduates	5119	GHS	Analysis of GLSS7 data	This value is to estimate the opportunity costs of schooling for children aged 11 and above in Step 4
Proportion of children aged 11–15 years in intervention sample	20	%		Used to measure opportunity costs of schooling
Value of time for 11–15 year olds relative to adults	75	%	Authors' assumption	Factor to apply to the primary school graduates wage rate to measure opportunity costs of schooling for children aged 11 and above in Step 4
Monthly teacher salary	800	GHS	Authors' estimate	To measure opportunity costs induced by the program for teachers in Step 4
Value of leisure time relative to productive time	50	%	Whittington and Cook (2020)	To measure opportunity costs induced by the program for teachers in Step 4
% of extra time that teachers spend in school that substitutes leisure time	50	%	Authors' estimate	To measure opportunity costs induced by the program for teachers in Step 4
Number of school days in an academic year	200	Days	Authors' estimate	To measure program costs in Step 4. We note that the RCT refers to two academic years, thus the figure we use is 400 days
General				
Beneficiaries	931	#	Aurino et al. (2023)	Number of children that reported receiving school meals in the endline of the RCT. Used to measure benefits in Step 3
Beneficiaries, % poor	27	%	Authors' calculation based on Aurino et al. (2023)	Number of children in households below the national poverty line that at baseline reported receiving school meals in the endline of the RCT. Used to measure benefits for this sub-sample
Low discount rate	3	%	Robinson et al. (2019)	Discount rate to measure net present values in Step 2
High discount rate	8	%	Robinson et al. (2019)	Discount rate to measure net present values in Step 2
Deadweight loss (including administration costs)	25	%	Alderman et al. (2019)	Deadweight loss from financing school meals in Step 4

of the extra teacher time is assumed to substitute for teaching responsibilities valued at the full wage rate of GHS 800 per month, while the remainder is assumed to substitute leisure time valued at 50 % of the wage rate (Whittington & Cook, 2019). Following Turkson, Baffour, and Wong (2020), we do not value the opportunity costs of children below the age of 10 due to uncertainties around these costs for young children and their caregivers. However, for children aged 11 or older than 11 years receiving the intervention at endline (20 % of the sample), we value time at the implied wage rate of primary school graduates which equals GHS 5119 per year and apply a 75 % factor to account for age, leading to an estimated average yearly wage of GHS 3839.

Thus, we estimate total economic costs of the intervention to be GHS 1.12 per child per day, for a total of GHS 418,224 for the full endline intervention sample over the two years (USD 90,918[11]). Finally, we add the value of a deadweight loss from financing school meals. With this additional cost included, total programmatic costs for two full years of implementation are GHS 522,362 (USD 113,557), for a total cost per child per year of GHS 280 (USD 61).

Table 1 summarizes the data employed in our analysis and their rationale.

[11] We use the average exchange rate of 2018, which was 4.6 Cedis to 1 US$, given the wage and consumption data are in 2018 prices.

4. Results

Consistent with Section 2, we present our results stepwise, highlighting how each of these explicitly links with the methodology.

4.1. Step 1: converting program impacts into estimates of long-run wage increases

We start by estimating the long-run productivity gains from program exposure. As noted earlier, this can be done in two ways: one approach (Step 1.A) derives from the direct product of the average treatment effect of the program on literacy (0.13) from Aurino et al. (2023) and the 17.8 % increase in wages per SD of literacy score among adults for Ghana reported by Evans and Yuan (2019), leading to a 2.3 % increase in wages after two years of program exposure[12].

As noted earlier, the generalizability of Step 1.A is limited to a lack of data that directly map learning to wages, such as the STEP dataset. The alternative approach (Step 1.B) is to estimate LAYS from estimated treatment effects. To operationalize Step 1.B, we start by estimating the

[12] We focus on literacy test scores to be consistent with Evans and Yuan's results. If we would apply the treatment effect coefficient on the composite learning score (e.s.=0.17), we would have an estimate of increased future earnings of 3.1% after being exposed to the program.

underlying Mincerian equation from the GLSS7, adjusted for sample selection for inclusion into wage employment (Heckman, 1979) in Table 2. In modeling both the selection and earning equations we follow Psacharopoulos and Patrinos (2018) with a 'bare bones' wage equation - including only gender, years of education, experience and experience squared - and a rich specification for the selection equation, encompassing several household, region, and rural controls. We estimate the returns to education in Ghana as 8.2 %.[13] This is close to the international evidence on surveys conducted after the 2000s reported in Psacharopoulos and Patrinos (2018), whereby one additional year of school increases life-time wages by about 9.1 % at the mean of the distribution.[14]

After having estimated the Mincerian returns to years of schooling, these need to be adjusted by learning that occurs in the absence of the program (thus, for the control group of the GSFP evaluation). Table 3, Column 1, reports OLS estimates of the composite index of standardized math and literacy test scores as a function of grade attained (as per the OLS approach of Evans & Yuan, 2019), controlling for child gender, age,[15] whether her family was below the poverty line at baseline, and region fixed effects. Column 2 includes baseline test scores. Columns 3 and 4 present the same models by focusing on literacy test scores only. Results presented in column 1 shows that each additional school year contributes to an increase of 0.21 of a SD in composite learning scores. Thus, it takes 4.76 years (in the absence of the program) to increase learning by a standard deviation. In keeping with Evans and Yuan (2019), we call this estimate "equivalent years of schooling" (EYOS). We note that our estimate is in line with the 4.4 EYOS in Evans and Yuan (2019). The treatment effect in LAYS corresponds to an increase of 0.81 LAYS, which was calculated by multiplying the treatment effect from the RCT (0.17SD) with 4.76 EYOS. We focus on deriving estimates of LAYS from column 1, rather than the corresponding estimates from column 2–4, as this is the most conservative scenario in terms of EYOS.[16] Finally, to get a measure of lifelong returns to increased learning due to program exposure, we multiply the estimates of the LAYS and the coefficient of one additional year of schooling on wages from the Mincerian regression in Table 2 [0.81*0.082], leading to a 6.6 % increase in lifetime wages after program exposure. In the next step, we apply this estimate of earning increments to the flow of estimated wages received between 20 and 60 years. This allows to measure total lifetime earning returns due to the increased human capital from program exposure.

[13] We acknowledge the cohorts in the GLSS7 are older than the ones we use in the RCT, and there is a trend for returns to schooling to slightly decrease as school rates increase, but this decrease is much smaller than the rate of increase in schooling globally (Patrinos 2024). We return to this issue in the discussions.

[14] We also run a Mincerian regression with log of total per capita expenditures. In this case, the returns to schooling are 5% and the estimated lifetime returns to consumption of schooling would be 4% as estimated in Step 1.B.

[15] Separate results for younger and slightly older students are presented in an on-line annex to Aurino et al. 2023. See, https://jhr.uwpress.org/content/wpjhr/suppl/2023/04/19/jhr.58.3.1019-10515R1.DC1/1019-10515R1_supp.pdf

[16] If we focus on the estimates from column 2 instead, where baseline test scores are controlled for, schooling contributes to only 0.14 SD of learning, which implies it takes 7.1 years to increase learning by one SD. As the value-added estimates of results in column 2 imply the program impact is equivalent to more learning-adjusted schooling years in the Mincerian equation than the results in column 1, we focus on the latter to be more conservative. We have broadly similar results when we focus on treatment effects on the literacy test scores (columns 3 and 4, Table 3). Yet, we prefer using the estimates based on the composite score, rather than literacy, as the former is likely to be more predictive of the type of skills that are valued on the labor market. As participating in the program leads to an improvement of 0.13 SD in the literacy scores, the program would translate into an effect of 0.62 learning-adjusted years of schooling in the Mincerian results.

Table 2
Mincerian equation.

Years of Education	0.082***
	(0.004)
Selection Variable (Inverse Mills Ratio)	−0.261 ***
	(0.038)
Constant	4.753***
	(0.071)
Number of Observations	**4603**
R-squared	**0.21**

Notes: This table presents estimates of schooling on log of wage earnings across the three primary sectors and all regions of Ghana. Data: GLSS7 (2016–17). Standard errors are clustered at the household level *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Earnings only include values from wage employment due to lack of self-employment earnings in the GLSS7 data. The cohorts included in the calculation of the Mincer equation span from 1927 to 2002 (25th percentile: individuals born in 1974; 75th percentile: individuals born in 1990). Estimates in the wage equation also control for gender, potential labor market experience and its square. Estimates in the selection equation control for: marital status, number of children 0–5 years, number of children 6–14 years, dependency ratio, household land ownership, region, rural. We control for region because data on wages over-represent the Greater Accra region and under-represent the three northern regions. The probability > chi2 of a Wald test of independent equation is 0.000, suggesting that selection into wage work is present.

4.2. Step 2: estimating net present values of lifetime human capital increases due to program exposure

In Step 1, we estimated returns to human capital from program exposure in two ways. One approach is to multiply the average treatment effect on literacy from the RCT to the 17.8 % increase in wages per SD of literacy in Ghana documented by Evans and Yuan (2019), leading to an increase in lifetime wages of 2.3 % (Step 1.A). The alternative

Table 3
Estimates of grade attainment on endline composite test scores index and literacy scores, and of equivalent years of schooling in the absence of the program (control group).

	(1) Composite of literacy and math	(2)	(3) Literacy	(4)
Grade attained	0.208***	0.138***	0.193***	0.128***
	(0.0339)	(0.0377)	(0.0305)	(0.0326)
Baseline test scores		0.199***		0.245***
		(0.0407)		(0.0510)
Poor household	−0.204**	−0.171*	−0.172**	−0.122
	(0.0813)	(0.0850)	(0.0834)	(0.0838)
Child is male	0.120**	0.112*	0.0983*	0.0744
	(0.0567)	(0.0576)	(0.0460)	(0.0460)
Age in years	−0.127***	−0.0789***	−0.126***	−0.0834***
	(0.0198)	(0.0252)	(0.0192)	(0.0221)
Constant	0.268	−0.0107	0.300	0.0662
	(0.266)	(0.313)	(0.279)	(0.319)
Observations	1059	1003	1057	990
R-squared	0.141	0.152	0.135	0.143
Estimated Equivalent years of schooling (EYOS)	**4.8**	**7.1**	**5.3**	**7.7**

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Notes: This model regresses a composite index of endline standardized math and literacy test scores (columns 1 and 2) and endline standardized literacy scores (columns 3 and 4) for the control group of the school feeding evaluation, as a function of grade attained, and other child characteristics. The coefficient for grade attained gives an indication of the learning occurring in the absence of the program. We use this coefficient to estimate equivalent years of schooling (EYOS), which measure how many years of schooling are needed to increase learning by a standard deviation. Data source: school feeding impact evaluation. Estimates also control for region fixed effects. Standard errors are clustered at the community level.

Table 4
NPV of the total benefits from school meals at different scenarios.

		Scenario 1 d=3%, no growth		Scenario 2 d=8%, no growth		Scenario 3 d=8%, income growth	
Panel A: Estimates based on 2.3 % increase in lifetime wages (STEP dataset)							
ε	Distributional benefits	NPV of human capital investment	Total benefits	NPV of human capital investment	Total benefits	NPV of human capital investment	Total benefits
0	0.27	1.55	1.82	0.52	0.79	0.84	1.11
0.5	0.3	1.55	1.85	0.52	0.82	0.84	1.14
0.7	0.32	1.55	1.87	0.52	0.84	0.84	1.16
1	0.36	1.55	1.91	0.52	0.88	0.84	1.2
1.3	0.41	1.55	1.96	0.52	0.93	0.84	1.25
2	0.63	1.55	2.18	0.52	1.15	0.84	1.47
Panel B: Estimates based on 6.6 % increase in lifetime wages from learning-adjusted Mincerian estimates (GLSS-7)							
0	0.27	5.53	5.8	1.84	2.11	2.99	3.26
0.5	0.3	5.53	5.83	1.84	2.14	2.99	3.29
0.7	0.32	5.53	5.85	1.84	2.16	2.99	3.31
1	0.36	5.53	5.89	1.84	2.2	2.99	3.35
1.3	0.41	5.53	5.94	1.84	2.25	2.99	3.40
2	0.63	5.53	6.16	1.84	2.47	2.99	3.62

Notes: this table computes net present values (NPVs) in millions of 2018 Ghanaian cedis of the total benefits from school feeding. Total benefits are the sum of distributional and productivity benefits for the sample of endline recipients (N = 931) at different values of the inequality aversion parameter ε, discount rates d*, and with/without economic growth. Distributional benefits are calculated by using an additive social welfare function that adds the weighted transfer value at the power of different ε. Panel A uses an estimate of a 2.3 % increase in lifetime wages based on the STEP dataset to calculate the NPV of human capital returns to program exposure, while Panel B uses an estimate of 6.6 % based on Mincerian returns to education adjusted for quality of education using the GLSS-7 and RCT data.*

approach (Step 1.B) is to combine RCT treatment effect converted in LAYS with the estimates of returns of schooling from the Mincer equation, leading to a lifetime increase in wages of 6.6 %. The estimate from the second method is much larger than the one implied by the direct application of the STEM data in the first approach, a point we return to in the discussion.

Step 2 now focuses on applying the estimates of returns obtained from Step 1.A and 1.B to the lifetime flow of wages, following the assumptions and sensitivity scenarios outlined in Section 2.2. We first aggregate the lifetime flow of wages at the individual level, and then we compute total NPVs from productivity gains for the whole intervention sample.

4.3. Step 3: estimating current (non-discounted) distributional benefits and total NPVs from program exposure

To compute total NPV, the remaining piece is to compute distributional benefits from current poverty reduction stemming from the program. As illustrated in Section 2.3, we do so by using an additive social welfare function that adds the weighted transfer value at the power of different inequality aversion parameters ε. To operationalize Eq. (7), values of per capita consumption for the students' families from the baseline impact evaluation data provide the values of x_i while the mean from the GLSS7 provides the value for x_j.

Table 4 presents the results from steps 2 (NPVs of human capital investments) and 3 (distributional benefits) at different inequality aversion preferences. Panels A and B present results using two alternative approaches to estimating wages as outlined earlier, namely Panel A reports estimates based on the wages derived from the STEP data (Step 1.A) while Panel B uses wage estimated from the combination of LAYS and GLSS data (Step 1.B). Within each panel, we calculate NPV of lifetime human capital returns to school feeding under three scenarios. As noted, the first and the second scenarios relate to income levels that are fixed at the average wage of primary school leavers[17] under the assumption of no national real income growth and a discount rate of 3 % and 8 %, respectively.

Under the first scenario, in which the low discount rate implies a relatively high weight assigned to future productivity, the individual NPV amounts to GHS 1666 (equivalent to current USD 362) in Panel A and GHS 5942 (equivalent to current USD 1292) in Panel B. These, in turn, lead to a total NPV of GHS 1.82 million (USD 395,652) over the sample using the lifetime wage increase based on the STEP dataset, and GHS 5.8 million (USD 1260,879) based on the Mincerian results with the GLSS-7 data for the sample of school feeding beneficiaries in the impact evaluation (N = 931). When the discount rate rises to 8 %, the individual NPV decreases to GHS 555 (USD 121) in Panel A and GHS 1979 (USD 430) in Panel B, for a total of GHS 0.5 million (USD 108,696) and GHS 1.84 million (USD 400,000), respectively. Finally, in the third scenario, we add real income growth over time based on middle-of-the-road projections (Riahi et al. 2017). Keeping the discount rate of 8 %, the individual NPV of school feeding returns from enhanced human capital over an individual working lifetime is GHS 901 (USD 196) in Panel A, and GHS 3214 (USD 699) in Panel B. For the full sample benefits amount to GHS 0.84 million (USD 182,608) and GHS 2.99 million (USD 650, 000), respectively, for the estimates based on the STEP dataset, and for the ones based on the learning-adjusted Mincerian results from the GLSS-7, respectively.

Appendix 2 provides a visualization of the distributional benefits relative to total benefits under the three different scenarios. As with Table 4, Panel A is based on estimates of lifetime wage increases based on the STEP data (derived from Step 1.A) and Panel B on the learning-adjusted Mincerian estimates with GLSS7 data (derived from Step 1. B). The figure highlights the role of current increased consumption of the households of children participating in the program. Even when societies do not value redistribution (ε =0) and the lifetime productivity gains from program exposure are relatively high (as in Panel B) the transfer contributes to 5–11 % of total school feeding benefits, depending on the different assumptions related to preferences for future productivity and whether projected real growth is accounted for. Since higher discount rates place greater weight on current redistribution at all values of ε, the share of benefits from redistribution is higher under scenario 2 than under scenario 1. With higher lifetime earnings but also a higher discount rate, scenario 3 falls between the other two.

When the LAYs estimate of productivity gains are lower, as in Panel A, the distributional benefits range between 15 and 34 % in the absence of inequality aversion. When ε =0.7, the distributional benefits are appreciable at both estimated future productivity gains on lifetime wages, ranging between 5 and 15 % in the high productivity scenario (Panel B) and 17–38 % in the low productivity scenario (Panel A). At very high levels of inequality aversion preferences, the relative

[17] This is a conservative assumption as most children in Ghana usually transition to junior secondary schools after primary school.

Table 5
BCR of school feeding including distributional and productivity benefits at different scenarios.

	Scenario 1 d=3%, no growth		Scenario 2 d=8%, no growth		Scenario 3 d=8%, income growth	
ε	2.3 % increase in lifetime wages	6.6 % increase in lifetime wages	2.3 % increase in lifetime wages	6.6 % increase in lifetime wages	2.3 % increase in lifetime wages	6.6 % increase in lifetime wages
0	3.5	11.1	1.5	4.0	2.1	6.2
0.5	3.5	11.2	1.6	4.1	2.2	6.3
0.7	3.6	11.2	1.6	4.1	2.2	6.3
1	3.7	11.3	1.7	4.2	2.3	6.4
1.3	3.7	11.4	1.8	4.3	2.4	6.5
2	4.2	11.8	2.2	4.7	2.8	6.9

Notes: this table presents BCRs at different inequality aversion parameters ε, scenarios based on different combination of discount rates d *and income growth (at 8 %), and the two methods to estimate lifetime increases in wages due to school feeding exposure. Total costs include implementation costs and deadweight loss at 25 %. Total program benefits include both distributional benefits and productivity returns, and are estimated on a transfer valued at GHS 1.5.*

contribution of current equity gains ranges between 10 % and 29 % (at a discount rate of 3 % with no growth) and 26 % and 55 % (discount rate =8 %, with no growth), depending on whether the estimated returns to lifetime wages are 6.6 % (Panel B) or 2.3 % (Panel A).

4.4 Step 4. Estimates of program costs and calculation of benefit:cost ratios

Finally, Table 5 Panel A summarizes BCRs at different values for ε, discount rates, and growth scenarios. We measure program costs as outlined in Section 3.4. Total benefits include distributional gains and human capital benefits that are based on both methods to estimate increases in lifetime wages from program exposure (Steps 1.A and 1.B). Total costs include implementation costs and deadweight loss at 25 %. Once the present value from increased consumption is considered, BCRs range from 1.5 (lifetime returns =2.3 %) and 4 (lifetime returns =6.6 %) in the absence of societal inequality aversion and the stringent discount and growth assumptions (scenario 2), to 4.2 (lifetime returns =2.3 %) and 11.8 (lifetime returns =6.6 %), where discount rates are low and inequality aversion is high (scenario 1).

5. Extension: retargeting simulation

As indicated in Section 3.1, the RCT was designed to retarget and scale the GSFP to 58 priority districts (out of the country's 170 at the time of this exercise) that had high rates of poverty and food insecurity (see Gelli et al., 2016, for details). Analysis of headcount poverty levels after the retargeting show consistency between our baseline sample (23 %) and the 2012–13 national headcounts (24 %) (Aurino et al., 2023). While this speaks to the potential external validity of the findings from the RCT to the whole of Ghana, it also highlights that poverty targeting remained limited even after the retargeting exercise.

So far, total benefits calculated so far reflect the program as implemented. However, the trial's design included subgroup analysis, which showed greater impacts for poorer households as well as within the more deprived northern regions. We thus use this information to explore a scenario under which the program's pro-poor targeting is improved. We simulate benefits under the assumption that the program is retargeted so that 50 % of the population is under the poverty line (N = 465). Under this scenario there can be both efficiency improvements as well as equity gains, as the learning and nutrition impacts of the GSFP were larger for poorest households (Section 3.1.1).

We start by estimating human capital returns for children from households under the poverty line. Following the first approach proposed by Evans and Yuan (2019) (Step 1.A), the estimated increase in lifetime earnings for children from households under the poverty line is 4.1 %, based on estimated treatment effects of 0.23 from the literacy score for this group (Aurino et al. 2023) and the increase in wages with each standard deviation of literacy of 17.8 % from the STEP data. By including the impact on the share of non-poor (with a treatment effect on literacy of 0.9), the average increase in wages in a retargeted scenario is 2.7 %.

Alternatively, in keeping with the results using the Mincerian equations (Step 1.B), based on learning among control children from poor households estimated from the interaction between grade and being from a household below the poverty line we find that every additional year of school increases the composite score by 0.15 SD. This is estimated as the difference between the coefficient for grade attained (0.23, $p < 0.01$) and the one of the interactions between grade attained and household poverty (−0.083, $p < 0.1$). This estimate translates into an effect of 6.6 EYOS for children in households below the poverty line. The learning-adjusted treatment effect of school feeding for children from households below the poverty line participating in the intervention is thus 2.18 additional years of school [0.33*6.6]. For children from households above the poverty line, the treatment effect of school feeding was much lower, at 0.48 LAYS. We compute individual and total NPV from productivity growth within each method for poor and non-poor households and present these results in Online Appendix 3.

The updated benefits and BCRs from this retargeting exercise are presented in Table 6, whereby in Panel A and B are reported the results from the STEP dataset and learning adjusted Mincerian estimates, respectively. These incorporate the additional schooling as well as the distribution of increased transfers to low-income families at their current values. When societies do not value redistribution, program's distributional benefits do not change based on a different poverty headcount. As soon as ε increases, distributional benefits rise, together with the BCRs from the program. But as schooling benefits also increase with retargeting, the share – but not the absolute value - of total distributional benefits is lower under retargeting than in the current program.

6. Discussion and conclusions

This paper presents and applies a method to fully account for the total benefits of interventions that impact multiple policy dimensions, including both distributional and productivity gains. Aggregation across multiple dimensions is a key practical hindrance for evaluating the cost-effectiveness of educational programs that have effects on different domains, which in turn hampers their financing (Watkins et al., 2024). Yet, the inclusion of distributional benefits in the BCA framework is not only relevant to educational programs but can be applied to a range of cash or in-kind transfers or health programs (e.g. health insurance).

To illustrate, we use the example of a government-led school meal program at scale in Ghana which has multidimensional impacts across learning, nutrition, and poverty outcomes. Our method converts learning impacts and distributional benefits into a common monetary unit. We accomplish this by first mapping the appreciable effect size of learning stemming from program evaluation to a more conventional metric, increments to human capital, and in turn link these with expected increases in wages. Two related but distinct approaches are used for this step inspired by Evans and Yuan (2019). We then add distributional benefits applying a social welfare function introduced by Atkinson (1970). Transfer values experienced by beneficiary households are weighted according to their relative income, and societal aversion to inequality. This second step is straightforwardly calculated using estimates of the transfer value implicit in school meals, the mean income of Ghanaian households, individual beneficiary household incomes and plausible ranges for the inequality aversion parameter, ε. While the methodological elements have been adopted previously, our primary contribution is to combine and illustrate the approaches to a practical and policy-relevant example, providing both step-by-step calculation guidance and a transparent accounting of data sources and results.

Our calculations show that under a range of assumptions, the gains attributed to a government-led school meal program at scale in Ghana greatly exceed program costs. The NPV of income gains from the program appear to be typical of modelled income gains from programs that improve learning in LMICs Evans and Yuan (2019). Yet, the current (non-discounted) distributional gains constitute up to half of total benefits from the transfer, depending on the scenario considered. This is consistent with a recent study, which shows that 77 % of Ghanaian households preferred school meals over the equivalent in cash, suggesting that: a) households value school meals more than the equivalent cash as per our assumptions, and b) the presence of the school meal program increases consumption relative to the absence of the intervention (Abreh et al., 2024).

In the light of our findings, comparisons of intervention cost-benefit that consider only learning gains may lead to sub-optimal investment decisions because they do not include the potentially important welfare gains of increased equity. Additionally, the distributional benefits alone exceed costs with a inequality aversion parameter, ε, of 1.3 or above. With plausible improved targeting (and assuming that the retargeting is a fixed and not a variable cost) the BCR will exceed 1 from increased equity alone when ε is slightly below 1, which is within the range of distributional studies (Skoufias et al. 2010). These types of threshold analyses are only possible with a formal accounting of distributional benefits.

There are several limitations of our analysis, many of which apply broadly to education BCAs and economic evaluations seeking to use impacts from RCTs to estimate benefits. First, neither of our benefits consider dynamic, general equilibrium effects. The distributional benefits modeled only pertain to current consumption. There are plausible dynamic distributional benefits to the degree that education leads to economic mobility. It is, however, a complex exercise to predict income distribution when the current students will reach maturity. A related dimension of this complexity revolves around general equilibrium effects of increased education (Khanna 2023). Such effects might lower the education premium on wages, but might also stimulate economic growth. Similarly, it is plausible that increased equity might contribute to growth. Thus, again, it is hard to predict their overall effects on future incomes. As the RCT studied does not add data on such effects we acknowledge that this is an area for further modeling, but one that is beyond the current study.

A second issue common to education BCAs is that the (general) absence of a longitudinal studies with data well past the primary schooling years, necessitates additional assumptions to convert learning to wages. The logic of converting improved learning to expected increases in wages is clear. We have employed two plausible empirical approaches to applying this logic, by using STEM data available for Ghana as well as using Mincerian estimates that are far more readily obtainable. Both methods rely on the assumption that improvements occurred in primary school result in similar benefits when the student enters the labor force. We show results from both approaches, although our preferred one is the Mincerian one, due to the large potential of applicability to RCTs and school surveys. Our choice of presenting both is also related to the wide differences of the two methods in yielding estimates of lifetime returns, with the Mincerian approach resulting in higher estimates of future wages. Nevertheless, it should be readily apparent from Table 3 that even if one halves our estimates of the NPV from the contribution to learning attributed to the school feeding program, the program still results in favorable BCRs under each scenario.

A third issue is that the GLSS7 data does not allow us to clearly distinguish the impact of schooling on self-employment in the Mincerian estimates. Both the magnitude and even the sign of any bias from the assumption that the gain in earnings from the program for self-employed is the same as for low wage earners is unknown. Further, while a growing literature has documented evidence of nontrivial human capital depreciation arising from under/non-employment (e.g., Dinerstein et al. 2022), we model returns of individuals not in the labor force like the returns for a wage-earning individual with a similar education. This is because returns may include home enterprises but may also reflect non-monetary returns. For example, there is extensive evidence showing that child nutrition improves with the education of the caregiver and that this is mediated by maternal learning (Glewwe, 1999). Thus, we assume that the gains to education for women engaged in home production and childcare are equivalent to the gains estimated in the labor force. Furthermore, we assume, along with Schultz (2002), that women and men receive the same percentage increase in their wage rates with additional schooling even if women earn less on average.

Another issue is that, as each additional year in school increases socio-emotional or non-cognitive skills in addition to learning (Ajayi et al., 2022; Jackson, 2018), LAYS only offer an incomplete and a possible lower-bound estimate of total skills acquired through schooling. The STEP data employed in this study has been utilized to study the significant contribution of non-cognitive skills to earning (see, for example, King & Gunewardena 2022) but there are few examples of RCs that measure the contribution of schooling in general, and school meals in particular, to such skills. The study in Ghana was not designed to

Table 6
Benefit-cost ratios for a retargeted sample.

		Scenario 1 d=3%, no growth			Scenario 2 d=8%, no growth			Scenario 3 d=8%, income growth		
ε	Distributional benefits	NPV of human capital investment	Total benefits	BCR	NPV of human capital investment	Total benefits	BCR	NPV of human capital investment	Total benefits	BCR
	Panel A: Estimates based on 2.3 % increase in lifetime wages (Step 1.A)									
0	0.27	3.99	4.26	8.1	1.33	1.60	3.1	2.16	2.43	4.6
0.5	0.35	3.99	4.34	8.3	1.33	1.68	3.2	2.16	2.51	4.8
0.7	0.38	3.99	4.37	8.4	1.33	1.71	3.3	2.16	2.54	4.9
1	0.46	3.99	4.45	8.5	1.33	1.79	3.4	2.16	2.62	5.0
1.3	0.56	3.99	4.55	8.7	1.33	1.89	3.6	2.16	2.72	5.2
2	0.99	3.99	4.98	9.5	1.33	2.32	4.4	2.16	3.15	6.0
	Panel B: Estimates based on 6.6 % increase in lifetime wages from learning adjusted Mincerian equation (Step 1.B)									
0	0.27	9.08	9.35	17.9	3.02	3.29	6.3	4.91	5.18	9.9
0.5	0.35	9.08	9.43	18.0	3.02	3.37	6.4	4.91	5.26	10.1
0.7	0.38	9.08	9.46	18.1	3.02	3.40	6.5	4.91	5.29	10.1
1	0.46	9.08	9.54	18.2	3.02	3.48	6.7	4.91	5.37	10.3
1.3	0.56	9.08	9.64	18.4	3.02	3.58	6.9	4.91	5.47	10.5
2	0.99	9.08	10.07	19.3	3.02	4.01	7.7	4.91	5.90	11.3

Notes: this table computes benefit-cost ratios (BCR) from school feeding by summing distributional and human capital benefits for the sample of endline recipients (N = 931) at different values of ε, d, and with/without economic growth (Panel A). Panel B presents results for a new sample based on a retargeting scenario where 50 % of the children are below the poverty line. Transfer size is GHC 1.5 per child per day at 400 hypothesized school feeding days over two school years. Economic growth projections are based on IASSA estimates. All values, except the BCR, are in million 2018 Ghanaian cedis. The BCR is unitless. Aggregate costs are 0.42 m GHC 6.

address this lacuna. Similarly, we note that the treatment effects we used to compute LAYS were intent-to-treat effects of offering school meals in randomized communities. Given program take-up was around 60 % on the study sample of children aged 5–15 years used for the evaluation, it is likely that program effects on children that received school meals were higher, pointing to another reason why the estimates in Aurino et al. (2023) may be lower bounds.

A further reason why this may be a lower bound is that we did not include estimated returns to nutrition in our estimates of human capital gains. We also have subsumed all gains from the observed improvements in nutrition for poor individuals under education when, in fact, there are additional documented impacts via health, such as through decreased illness due to better quality diets (Hoddinott et al. 2013; Belot & James 2011). We have chosen this approach to avoid double-counting of any direct impacts of better nutrition on learning. There may other potential nutritional benefits from school lunch programs such as improved reproductive fitness or the adoption of healthy dietary habits likely to reduce subsequent risks of noncommunicable diseases, but these are seldom quantified, including in the Ghanaian study.

Additionally, while various trials point to learning gains from school feeding, there is less evidence of persistence of results (Alderman & Fernald, 2017; Bailey et al., 2020). A different, yet related, issue is the appropriateness of using estimates of returns of schooling from older cohorts to estimates of returns for younger cohorts, as changing conditions can decrease the returns to schooling in the long-run. However, these decreases have been rather modest historically. For instance, Patrinos (2024) shows that while the supply of schooling expanded by almost 50 percent globally since 1980, the returns to schooling decreased by only 3.5 percentage points, or 0.1 percent per year. Additionally, our estimate of returns in Ghana is quite consistent with global evidence, showing a global rate of return to schooling of 9.5 %, and of 11 % for Sub-Saharan Africa. Thus, while we do not dismiss the potential gains from narrowing down these uncertainties, we consider the main contribution of this analysis to be the illustration of a straightforward approach to utilize effect sizes from randomized controlled studies in broader economic analyses as well as the inclusion in such analyses of often recognized, but seldom quantified, distributional impacts.

Given this emphasis, we conclude by briefly discussing how this method could be applied to other programs that affect multiple outcomes including equity. Let's imagine, for instance, a poverty-targeted food-based program evaluated through an RCT. If this program would affect human capital through improved child nutrition but does not measure learning, this method could still be applied by using appropriate parameters to estimate the productivity impact of malnutrition over a lifetime (Alderman et al., 2017) and combine these with the analysis of the current distributional benefits of the program. This method – as in any BCA – inevitably builds on a range of assumptions, which must be transparently conveyed to allow for replication of the results of each study, as well as comparability of estimates with other studies. We hope that laying those down as in Table 1 or offering simulations based on different parameters can offer a workable framework for other researchers. The latter allows one to ascertain what drives differences in cost-effectiveness across studies (e.g. differences in assumptions viz. differences in the magnitudes of effects, and whether such differences in assumptions are reasonable given differences in times and places).

CRediT authorship contribution statement

Harold Alderman: Conceptualization, Writing – original draft, Writing – review & editing, Methodology. **Elisabetta Aurino:** Writing – original draft, Writing – review & editing, Methodology, Formal analysis. **Priscilla Twumasi Baffour:** Methodology, Writing – review & editing. **Aulo Gelli:** Writing – review & editing. **Festus Ebo Turkson:** Methodology, Writing – review & editing. **Brad Wong:** Methodology, Writing – review & editing.

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.econedurev.2025.102646.

Data availability

Data will be made available on request.

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