Abstract

We conduct a pair of experiments to identify the value that parents place on equality of opportunity when investing in their children. The first is a real-stakes, lab-in-the-field experiment in a lower-income country and the second a hypothetical, online survey experiment in multiple higher-income countries. The experiments exogenously vary the short-run returns to educational investments to identify how much parents care about equalizing “opportunity” (the investment in each child) relative to maximizing “returns” (total household earnings) or to equalizing “outcomes” (child-level expected earnings). We show that while parents in both experiments place some weight on maximizing returns, they also display a strong preference for equalizing opportunities and are willing to forgo roughly 15-50% of their potential earnings to do so. We find that parents in the higher-income countries also care about equalizing outcomes, while parents in the lower-income country do not.
1 Introduction

Equality of opportunity is a widely held ideal. For example, 90% of Americans endorse the idea that it is important to “make sure everyone has an equal opportunity to succeed” (Pew Research Center, 2012). But, if achieving this ideal decreases the size of the pie, what price will people pay to uphold it? And how do people trade off the ideal of equal opportunity against more outcomes-centered notions of equality?

Consider the case of human capital investments made by parents in their children – some of the most important opportunities that people receive growing up, and therefore of great interest to economists (e.g., Adhvaryu and Nyshadham, 2016; Barrera-Osorio et al., 2011, 2020; Behrman et al., 1982; Bergman, 2021; Bursztyn and Coffman, 2012; Lichand and Thibaud, 2020; Soh and Tan, 2020). Some children have higher returns to education than others. Do parents prefer to give more investment and opportunities to children with higher returns in order to maximize aggregate income, with the potential option to redistribute income \textit{ex post} to achieve greater equality in the outcome of consumption? Or do they believe opportunities have intrinsic value and prefer to give equal opportunities to all children, even if it decreases the size of the pie? Despite equality of opportunity being a central idea in the broader discourse, the literature in economics on parental preferences for educational investment has largely neglected this possibility. Instead, that literature (e.g., Adhvaryu and Nyshadham, 2016) has considered a third possibility: that, instead of caring about equality in the opportunities distributed, parents care about equality in the level of human capital or expected future earnings, for example because they believe that “self-respect depends on earnings” (Behrman et al., 1982). In this case, parents may prefer to give more opportunities to children with lower ability in order to compensate for their lower endowment.

This paper estimates preferences for the distribution of opportunities, quantifying the value parents place on equalizing “opportunities” (i.e., investment), equalizing expected “outcomes” (i.e., expected future earnings conditional on ability and investment), and maximizing returns (i.e., total expected earnings). We designed an experimental paradigm that allows us to estimate these preferences and implemented the paradigm in two experiments that identified parents’ preferences for allocating an educational opportunity across two of their children. We conducted one of the experiments in a lower-income country, Malawi. This experiment (hereafter: the lower-income experiment) was an in-person, lab-in-the-field
experiment that used high monetary stakes and sampled parents of children in grades 5-7. We conducted the other experiment in multiple higher-income countries, primarily the US and the UK. This experiment (hereafter: the higher-income experiment) was a hypothetical online survey experiment that sampled parents of children in kindergarten through grade 12. We paired both experiments with a survey assessing stated preferences for equality.

In both experiments, we find that while parents did care about the returns to investment, they also displayed a quantitatively important preference for equal opportunity and were willing to forgo roughly 15-50% of their earnings to satisfy that preference. In contrast, we find mixed evidence regarding whether parents also preferred to equalize expected outcomes. Finally, our survey evidence suggests that the preferences we estimate experimentally may be relatively robust to the exact opportunity considered, such as whether the opportunities are distributed by a parent across her own children or by a government across society.

Our experimental paradigm works as follows. Each child takes a test and receives a monetary payment that increases with their test score. Payments are made directly to the child. The parent’s expectation of each child’s monetary payment is our measure of the child’s expected outcome, following the literature (e.g., Behrman et al., 1982) in which the outcome of interest is the child’s expected earnings. To identify the weights parents assign to different types of preferences, we vary the payment functions that map test scores to payments via multiple exogenous shocks. In the lower-income experiment, we implemented this setup in full, performing a test with the children and delivering real monetary payments. In the higher-income experiment, the implementation was hypothetical; parents were asked to imagine that their children were to take tests and receive payments based on their scores.

Before the test, parents received opportunities to divide between their children. These comprised 10 lottery tickets, where the prize was one hour of tutoring before the test. Tutoring is a commonly used input in both lower- and higher-income countries, and hence familiar to parents.¹ The tutoring was real in the lower-income experiment and hypothetical in the higher-income experiment. Parents were told that exactly one of their 10 tickets would be randomly chosen, and the child to whom that ticket was allocated would receive the tutoring. Since only one child per household received tutoring, each parent could choose which of her

¹For instance, in Malawi, in 2005, the share of 6th graders using tutoring services was 50% (Paviot et al., 2008); in the UK, in 1998, the corresponding figure was 37% (West et al., 1998).
children would be the recipient by allocating all of her tickets to that child.

The lottery setup yields a clean prediction about parents’ behavior: a returns-maximizing parent will give all of her tickets to the child whom she thinks will provide greater returns to the investment, and none to the other. She should only deviate from an “all-or-nothing” allocation if she is averse to unequally distributing opportunities among her children or to inequality in her children’s expected outcomes.

Crucially, this setup allows us to distinguish preferences for equality from risk aversion, which would otherwise be confounded because risk aversion may also cause parents to invest more equally, but as a hedge in the face of uncertainty. In contrast, in our lottery, any returns-maximizing parent should only choose all-or-nothing allocations no matter their risk aversion or uncertainty. Thus, we can test whether parents value equality (of either opportunities or outcomes) by testing whether they deviate from all-or-nothing allocations.

To identify preferences, we varied the child-specific payment functions in ways that have qualitatively different predictions depending on parents’ preference parameters. For example, certain payment functions provided the same payment per test score point to both children, whereas others delivered a higher payment to the higher-performer. If parents are returns-maximizers, increasing the payment-per-test-score-point for the higher-performing sibling would cause parents to give that child more tutoring opportunities (lottery tickets) because their expected payment gains from tutoring have increased. In contrast, if parents value equality of expected outcomes, they would do the opposite: since the lower-performing sibling’s expected earnings have decreased relative to their sibling’s, parents who want to equalize their children’s expected earnings would reallocate tutoring opportunities to the perceived lower-performer to help her earn as much as her sibling.

We establish our results as follows. We first tested and rejected the hypothesis that parents care only about maximizing returns. Only around 50% of allocations were “all-or-nothing,” which is the prediction for returns-maximizers in our lottery environment. However, we do find that parents placed positive weight on maximizing returns, as they generally invested more in a child when the returns to tutoring for that child increased.

We next establish that parents value equality of opportunity and that this preference

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2To maximize our statistical power, we vary the payment functions within parent (as opposed to across parent) by eliciting each parent’s choices under every pair of payment functions used in the experiment. To make the elicitation real stakes in our lower-income experiment, we used the “strategy method.” After eliciting each parent’s choices, we randomly selected the payment functions to implement for each parent.
is the primary reason they deviated from returns maximization. Parents chose to split the tutoring opportunities equally in over 30% of their choices. Even when we substantially increased the expected payment gains from maximizing returns—offering 10 times higher returns per point to one child—at least 25% of parents still equalized. This preference for equalizing opportunities meaningfully decreases earnings. Using a simple mixed logit model to identify parents’ preferences, we find that parents are willing to give up an average of 15-50% of total household experimental earnings to equalize opportunities. In level terms, this means that parents in the real-stakes lower-income experiment were willing to pay an average of 90-110% of the average daily wage (1,300 MWK - 1,600 MWK) to split the opportunity equally. In the higher-income experiment, parents were willing to pay 20-60 USD.

Finally, we find that parents in our higher-income sample also had a preference to equalize expected outcomes, but that parents in our lower-income sample did not. The most direct evidence comes from payment functions that exogenously delivered a lump sum payment to one child only. If parents value equality of outcomes (i.e., expected child-level earnings), they should respond to the lump sum by giving more tickets to the sibling of the child who received the lump sum to help close the gap in expected earnings. We find that parents in the higher-income experiment responded in this way, but those in the lower-income experiment did not. This difference does not appear to reflect differential understanding of the experiment, as we find a similar difference in stated preferences for equality. Instead, heterogeneity analysis suggests that one reason for the disparity is that the preference for equality of expected outcomes relative to equality of opportunity increases with household income.

Our experimental design identifies a preference for equality in the outcome of expected earnings, not the ultimate outcome of ex post consumption, as expected earnings are directly determined by the distribution of opportunities. In contrast, consumption is only more indirectly related as it can also be affected by ex post redistribution. That said, we also assess preferences for equality in consumption by measuring the extent of ex post redistribution in the lower-income sample. The vast majority of parents did not redistribute ex post. This finding is consistent with a literature that shows that people engage in less ex post redistribution when they believe that the process that generated the earnings was fair, for example because the ex ante opportunities were equal (Alesina and La Ferrara, 2005).

3At the time of the study, the exchange rate was 715 MWK : 1 USD.
Collectively, our findings suggest that people place a high value on equality of opportunity and, in higher-income contexts, on equality of expected outcomes as well. They appear to value these ideals in and of themselves, not simply as means to create a fairer distribution of consumption, and are willing to meaningfully shrink the size of the pie to uphold them. These findings have important policy implications. For example, consider a government distributing resources to households with the objectives of maximizing take-up and crowding-in household investment. Our finding that parents are averse to unequal opportunity implies that the government’s choice of whether to distribute the resource equally or unequally within households may be highly impactful, with the impact dependent on how much wealth households have. If parents have the capacity to increase their spending, then the government may prefer unequal distribution, as it could encourage parents to spend more on their less-resourced children to mitigate the inequality in opportunity. But, if parents do not have the capacity to increase their spending, their aversion to unequal opportunity could instead make them forgo the government’s free resource for one child if the other cannot have it. Indeed, in the conclusion, we provide survey evidence that many parents would forgo a free and valuable opportunity if only one of their children could access it. In cases where this is true, the government may prefer equal distribution within households.

Related Literature and Contributions

This paper contributes to two distinct literatures in economics: on parental investment in children and on preferences for fairness and equality.

Parental Investments. This paper makes two contributions to a classical economics literature that examines parents’ preferences for investing in their children (e.g., Behrman et al., 1982, 1986; Griliches, 1979). First, the previous papers rely primarily on functional form assumptions for identification. In contrast, we present the first experimental evidence on parents’ preferences for investing in their own children; we use exogenous shocks to the returns to investment in each of the parent’s children to uncover preferences. Second, the previous work exclusively examines parents’ preferences for equality of expected outcomes relative to efficiency. We also examine the preference for equality of opportunity and show

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4A more recent paper, Giannola (2021), elicits parents’ hypothetical investment allocations across hypothetical children (i.e., not the parent’s own children) with varying ability. That paper is thus an exception on the identification front, but does not estimate preferences for equality of opportunity.
that it is the dominant preference across both lower- and higher-income countries.\(^5\)

Second, our work relates to papers that examine how parents’ investments depend on their children’s endowments, and whether parents prefer to reinforce these endowments by investing more in their higher-endowment children, or to compensate by investing less. The findings are mixed, with responses ranging from reinforcing (e.g., Adhvaryu and Nyshadham, 2016), to zero (e.g., Royer, 2009), to compensatory (e.g., Bharadwaj et al., 2018). See Almond and Mazumder (2013) for a review. These papers, however, generally do not identify parents’ preferences themselves, as investments reflect the interaction between preferences and the (unobserved) perceived production function. It is important to note that our result that parents value equality of opportunity is not inconsistent with studies that show that parents invest differently in their children. We also find differential investment across children, but that parents balance their desire to maximize returns with their desire to equalize, causing their investments to be more equal, not necessarily fully equal.

**Fairness preferences.** We also contribute to a large literature on fairness preferences and inequality aversion.\(^6\) The vast majority of this literature examines preferences over the distribution and redistribution of *ex post* outcomes (i.e., realized earnings and consumption).\(^7\) Our key contribution is to instead identify preferences over the distribution of the *ex ante* opportunities themselves, thus allowing us to identify the price people will pay to achieve the popular ideal of equality of opportunity. Indeed, it is important to understand preferences over the distribution of *ex ante* opportunities even if one already understands preferences over the distribution of *ex post* outcomes since the two preferences are conceptually distinct; one cannot back out one preference from the other.\(^8\)

\(^5\)There also exists a classical economics literature showing that parents often equally divide bequests among their children (e.g., Bernheim and Severinov, 2003; Menchik, 1980; Wilhelm, 1996). However, bequests differ importantly from investments. Unlike investments, equally dividing bequests may have no efficiency cost. The distinction between opportunity and outcomes inequality also is not relevant for bequests.

\(^6\)Key contributions include Andreoni et al., 2018; Bolton and Ockenfels, 2000; Brock et al., 2013; Cappelen et al., 2013; Charness and Rabin, 2002; Fehr and Schmidt, 1999; Forsythe et al., 1994.

\(^7\)Some of these papers use different terminology than us, referring to equality in *ex ante* expected payments as equality of opportunity and equality in *ex post* realized payments as equality of outcomes. In contrast, both of our equality types (of opportunity and expected outcomes) are *ex ante* notions.

\(^8\)That is, it is very possible for individuals to, *ex post*, prefer to redistribute income towards people who experienced less opportunity *ex ante*, but have no preference for distributing opportunities more equally *ex ante*. For example, the individuals may primarily value equality in consumption and think that *ex post* redistribution is a more efficient way to achieve equality in consumption than equal division of opportunity. The opposite (preferring to equalize *ex ante* but not *ex post*) is also very possible.
Andreoni et al. (2018) is a notable exception that also examines how people distribute opportunities, in particular, lottery tickets for a monetary prize. Their goal is to show that notions of fairness change as uncertainty about the lottery tickets’ expected payments is resolved. Their paper differs from ours in two key ways: in Andreoni et al. (2018), there is neither an efficiency cost of dividing the tickets one way or another, nor a distinction between our two notions of \textit{ex ante} equality (expected outcomes or opportunities). In contrast, we test whether people are willing to pay an efficiency cost to equalize, and we distinguish between the two notions of \textit{ex ante} fairness.\footnote{Both differences stem from the fact that the returns to the opportunities used in Andreoni et al. (2018) do not depend on who receives them. In contrast, in our and most human capital settings, the expected returns to opportunities and investments depend on the ability and characteristics of those who receive them.}

2 Conceptual Framework

We propose a simple framework for analyzing preferences over the distribution of opportunities. We focus on the case of a parent’s preference for distributing opportunities between her two children, since this is both an important application and the case we study experimentally. However, the same framework applies more generally, to preferences over governmental allocation of opportunity across individuals, for example.

Consider a utility-maximizing parent with two children indexed by $i \in [1, 2]$. The parent chooses the level of opportunity to give each child and, specifically, the level of educational investment in each child, $x_1$ and $x_2$. Investments in child $i$ weakly increase his or her present discounted lifetime earnings, with the relationship determined by the earnings function: $R_i(x_i) \equiv R(x_i, a_i, \varepsilon_i)$. Child $i$’s earnings also depend on her “endowment” or ability, $a_i$, which we define as her earnings when $x_i = 0$, and on $\varepsilon_i$, a mean-zero noise term capturing uncertainty in the production of earnings. When choosing $x_1$ and $x_2$, parents know $a_1$ and $a_2$. They also know the joint distribution of $\varepsilon_1$ and $\varepsilon_2$, but not their realizations. After earnings are realized, we assume initially that parents can easily redistribute earnings across their two children and then discuss the implications of relaxing this assumption below.\footnote{“Easily” means both that it is not costly to implement and entails limited moral hazard cost.}

We aim to identify four main dimensions of preferences:

1. \textit{Equality of Opportunity}, represented by the negative of the inequality in the opportunities allocated to each child, $-|x_1 - x_2|$. Parents may value equality of opportunity because they believe opportunities have intrinsic value or confer self-determination, for example.
2. **Equality of Outcomes**, represented by the negative of inequality in the children’s expected earnings, $-|ER_1(x_1) - ER_2(x_2)|$.\(^{11}\) One can think of expected earnings $ER_i(x_i)$ as a measure of human capital that encompasses both the individual’s ability and the investments made in her. Parents may care about this if, for example, they think that human capital has intrinsic value or that the act of earning confers self-respect.

3. **Returns Maximization**, captured by the expected utility of total household earnings, $Eu(R_1(x_1) + R_2(x_2))$. $u(\cdot)$ is increasing and weakly concave in order to allow for risk aversion. This represents the desire to maximize the size of the pie, which could then be redistributed *ex post* if desired.

4. **Child-Specific Preferences**, which represent the preference for investing more in one child or the other (e.g., a preference for a son over a daughter).

We focus on the four preferences above because we are interested in the distribution of opportunities, and these four preferences fundamentally affect how people distribute opportunities. A preference for equality of consumption is not included. Unlike the other preferences for equality, this preference does not require the distribution of opportunities to depart from the returns-maximizing distribution, as it can be satisfied via *ex post* redistribution instead.\(^{12}\)

The utility function is a weighted sum of these preferences,\(^ {13}\)

$$U(x_1, x_2) = -\alpha |x_1 - x_2| - \beta |ER_1(x_1) - ER_2(x_2)| + \lambda Eu (R_1(x_1) + R_2(x_2)) + \gamma (z_1, z_2) (x_1 - x_2)$$

The weights on equality of opportunity ($\alpha$), equality of outcomes ($\beta$), and returns maxim-

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\(^{11}\)This formulation, in which parents care about $|ER_1(x_1) - ER_2(x_2)|$, captures a preference for equality in *ex ante* expected earnings. An alternate formulation might stipulate that parents care only about equality in the final *ex post* distribution of outcomes ($E|R_1(x_1) - R_2(x_2)|$). We adopt the *ex ante* formulation since we study the decisions parents make from the *ex ante* perspective, and the literature generally finds that people care about *ex ante* fairness from an *ex ante* perspective (e.g., Andreoni et al., 2018).

\(^{12}\)This statement depends not just on our assumption of easy *ex post* redistribution but also on an assumption that the preference for equality of consumption is separable from the preferences for equality of opportunities and equality of outcomes (expected earnings) in the utility function.

\(^{13}\)This formulation assumes linearity in the equality and child-specific preference terms. We assume this for expositional simplicity and (later) ease of estimation, but show below that our results are robust to relaxing this assumption. This formulation also assumes separability in the different terms, as is common in the fairness literature (e.g., Charness and Rabin, 2002).
mization (λ) are all weakly positive. γ(z₁, z₂) is the parents’ relative preference for investing in child 1 relative to child 2; it depends on a vector of each child’s characteristics zᵢ (e.g., gender, age) and can be positive or negative.

The parent’s problem is to choose x₁ and x₂ to maximize U(x₁, x₂) subject to the budget constraint x₁ + x₂ ≤ y, with y denoting the total educational budget.

**Comparative Statics.** To develop the basic predictions from the model, we consider cases in which the parent places full weight on one of the components and zero on the others. In these cases, the first-order conditions yield the following intuitive predictions:

- **Returns Maximization** (α = 0, β = 0, λ > 0, γ = 0). The parent invests to equalize the expected utility returns to investment across children: 
  \[ E \left[ \frac{\partial R₁(x₁)}{\partial x₁} u' (R₁(x₁) + R₂(x₂)) \right] = E \left[ \frac{\partial R₂(x₂)}{\partial x₂} u' (R₁(x₁) + R₂(x₂)) \right]. \]
  Since u(·) may be concave, the parent may be risk averse and choose more equal investments than the investments that maximize expected household earnings.¹⁴ Depending on the complementarity between x and the children’s endowments, she could invest more in the higher- or lower-endowment child.

- **Equality of Opportunities** (α > 0, β = 0, λ = 0, γ = 0). The parent seeks to equalize x₁ and x₂, regardless of the shape of R₁(x₁) and R₂(x₂).

- **Equality of Outcomes** (α = 0, β > 0, λ = 0, γ = 0). The parent invests first in the lower-endowment child so that that child’s earnings “catch up” with their sibling’s. At a sufficiently high y, she may begin to invest a positive amount in the higher-endowment child to keep their earnings from falling behind their sibling’s. If investments and endowments are complements, she always invests more in the lower-endowment child.

- **Child-specific Preferences** (α = 0, β = 0, λ = 0, γ ≠ 0). The parent gives all opportunities to child 1 if γ(z₁, z₂) > 0 and to child 2 if γ(z₁, z₂) < 0.

**Identification of Equation (1).** We aim to estimate α, β, λ, and γ from observed data on x₁ and x₂. To do so, we need to know the perceived earnings functions, R₁(x₁) and R₂(x₂), and observe how x₁ and x₂ respond to multiple exogenous shocks to these functions.

A key identification challenge is parents’ risk-aversion: to determine deviations from returns-maximization, one needs to know the returns-maximizing allocation. However, with risk aversion, knowing the returns-maximizing allocation entails knowing not just the returns functions R₁(x₁) and R₂(x₂) but also knowing parents’ utility function u(·) – which is

¹⁴The investments that maximize household earnings would instead satisfy \( \frac{\partial E R₁(x₁)}{\partial x₁} = \frac{\partial E R₂(x₂)}{\partial x₂} \).
generally not possible.

We sidestep this identification challenge by considering how parents allocate “probabilistic investments.” Consider a parent choosing how to allocate a single free binary investment (in our case, one indivisible tutoring class) between her children. To determine who receives the investment, the parent receives \( n \) lottery tickets to allocate between her children, where each ticket has the same chance of being chosen, exactly one ticket is chosen per household, and the child whose ticket is chosen receives the binary investment.

The lottery setup yields a sharp prediction for returns-maximization: parents who care only about maximizing returns (\( \alpha = 0, \beta = 0, \gamma = 0 \)) will choose “all-or-nothing” allocations of lottery tickets, giving all tickets to one child or the other. This choice ensures that the binary investment goes to the child the parent would prefer to receive it. If parents choose “split” allocations, giving positive allocations to both children, it must be that either \( \alpha \neq 0 \) or \( \beta \neq 0 \), or that it is the knife’s edge (and hence empirically unlikely) case that the parent is exactly indifferent about which child receives the binary investment.\(^{15}\) The prediction that split allocations imply a preference for equality follows from the fact that, with \( \alpha = 0 \) and \( \beta = 0 \), expected utility is linear in lottery tickets, but also holds with many preferences that are non-linear in probability (e.g., prospect theory preferences). See Appendix C for additional discussion.

The all-or-nothing test for inequality aversion is a test for caring about a specific type of inequality: inequality in \textit{ex ante} or \textit{expected} opportunities and outcomes.\(^{16}\) If some parents are only averse to inequalities in the final \textit{ex post} distribution of opportunities or outcomes (i.e., if a parent cares only about whether her children ultimately receive unequal opportunities) but not about the \textit{ex ante} expected opportunities distribution, our test will categorize them as not averse to inequality. In that way, the test for preferences for equality is conservative or shaded towards the “null” of the standard returns-maximizing model.\(^{17}\)

Thus far we have assumed that utility is linear in all preferences except for returns-

\(^{15}\) \( \gamma \neq 0 \) will not produce split allocations; it also implies all-or-nothing allocation, to the preferred child.

\(^{16}\) On the opportunities side, this means that the third term of equation (1) becomes \(-\alpha|Ex_1 - Ex_2|\).

\(^{17}\) That said, it is not always the case that a parent who chooses to split expected investments or opportunities would also choose to split actual investments. For example, if the investments have convex returns (and therefore lower total returns when split across children), the parent might split expected investment but not actual investment. Our notion of aversion to inequality in \textit{ex ante} expected opportunities can be seen as a form of “procedural” fairness (Krawczyk and Le Lec, 2010), wherein people care not just about the \textit{ex post} outcome but also the procedure that produced it. A classic example of similar preferences comes from Machina (1989), who observes that a parent may be indifferent about which of her children receive an indivisible good but may strictly prefer to randomize which receives it.
maximization, where we permit non-linearity to allow for risk aversion. Allowing for non-linearity in all preference terms only changes one prediction: split allocations do not always imply that \( \alpha \neq 0 \) or \( \beta \neq 0 \). In particular, a parent who does not care about equality \((\alpha = 0, \beta = 0)\) could choose a split ticket allocation to balance returns-maximization and a child-specific preference, if the children preferred by the returns-maximization preference and the child-specific preference are different. As discussed later, we rule out this behavior as important in practice by inducing random variation in the child a returns-maximizing parent would prefer.

**Earnings, Consumption, and Redistribution** Above, we assumed that *ex post* redistribution was easy. This implied that people with a preference for equality in consumption (but not equality in earnings or opportunities) would maximize returns when distributing opportunities and redistribute income *ex post*. But what would happen if *ex post* redistribution were not viable?\(^{19}\)

In that case, those people might equalize earnings or opportunities not because they value those objects per se, but rather as a means to create a more equitable distribution of consumption. The preference for equality of consumption would then load on \( \alpha \) and/or \( \beta \), and we would not be able to separate the primitive desires for equality of opportunity or earnings from the desire for equality of consumption. Thus, to separately identify those primitive preferences, one should evaluate preferences in a setting with easy *ex post* redistribution. In such a setting, one can also identify the preference for equality in consumption by simply measuring the extent of *ex post* redistribution.

To separately identify preferences for equality of opportunity, earnings, and consumption, we do two things. First, we make it easy for parents to redistribute earnings *ex post* by delivering earnings in cash to children who live in the same homes as their parents.\(^{20}\) Second, we measure the extent of *ex post* redistribution in our lower-income experiment.

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\(^{18}\)That is, allowing for \( U(x_1, x_2) = -\alpha g_1(|x_1 - x_2|) - \beta g_2(|ER_1(x_1) - ER_2(x_2)|) + \lambda Eu(R_1(x_1) + R_2(x_2)) + \gamma(z_1, z_2)g_3(x_1 - x_2) \) with \( g_1(\cdot), g_2(\cdot) \), and \( g_3(\cdot) \) increasing functions.

\(^{19}\)For example, people may not trust the government to redistribute effectively (Kuziemko et al., 2015). Alternatively, the efficiency cost of *ex post* redistribution (moral hazard) could loom large.

\(^{20}\)The scope for moral hazard due to *ex post* redistribution is also limited, since children do not know any specifics about whether or how they will be paid when they take the test. While there still could remain some barriers to redistribution – for example, parents may not want to take earnings away from their children – those barriers are arguably fundamental to any situation where someone might redistribute earnings.
3 Experimental Design and Procedures

Our goal is to identify the preference weights $\alpha$, $\beta$, $\lambda$, and $\gamma$. To do so, we designed an experimental paradigm that shocks children’s short-run earnings for identification. We begin with a brief overview of the experimental paradigm, followed by a description of our experimental procedures and how we implemented our two separate experiments. We then explain how we identify the preference parameters. Finally, we provide evidence to validate our experimental method and present summary statistics.

3.1 Overview of Experimental Paradigm

We sample households with two children enrolled in school. Children take a math test and receive a payment, delivered directly to the children based on their test scores. The monetary payment is our measure of the outcome (the $R(\cdot)$ function), which in the literature generally represents the earnings from education (e.g., Behrman et al. (1982)). Before the test, parents are endowed with opportunities: 10 lottery tickets to be allocated across their children, where one ticket would be chosen per household. The lottery winner in each household receives one hour of tutoring covering the test material.

We use lottery tickets as our opportunities for two reasons. First, it enables a clean test for returns-maximization in the presence of risk-aversion. Second, it facilitates measurement of the perceived production function, which one needs to understand to test for deviations from returns-maximization. Expected test scores (and hence the production function) are linear in probability and therefore in lottery tickets. Thus, with a lottery, to understand the full production function, we can simply elicit parents’ beliefs about each of their children’s test scores without tutoring and about how much each child’s test score would increase if they received tutoring. If instead we had used, say, hours of tutoring as our opportunities, we would have needed to elicit beliefs about how the returns to tutoring diminish with additional hours – beliefs which appeared in piloting to be highly uncertain and unreliable.

Our primary source of identification comes from exogenously varying the payment functions that map child-specific scores to payments. For example, one payment function awards both children the same payment per test-score-point, whereas another gives one child a higher payment than their sibling. We describe the payment functions in Section 3.3.

In order to maximize statistical power, we use the “strategy method”: we present each

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21 We focus on math to increase the reliability of the test.
parent with five scenarios for what the payment functions might be, and each parent chooses allocations under each of the five scenarios, with one scenario randomly chosen to be implemented for each parent. Lottery tickets are then assigned to both children based on their parents’ allocation for that scenario.\textsuperscript{22}

Payments are delivered directly to the children in cash. Cash, like most earnings, are potentially transferable across people. This is appropriate since, as described in Section 2, we aim to use the experiment to identify a preference for equality in earnings, not consumption. To identify preferences for equality in consumption, we then conducted a follow-up survey after the lower-income experiment to assess the amount of redistribution.

### 3.2 Implementation and Procedures

We implemented our experimental paradigm in two different experiments. Our first \textit{lower-income experiment}, conducted in southern Malawi, was an in-person, lab-in-the-field experiment. This experiment used real (high) stakes. The sample consists of 289 parents with two children enrolled in grades 5 - 7 in government schools. Our second \textit{higher-income experiment}, conducted in multiple higher-income countries, was a hypothetical, online survey experiment. The sample represents 285 parents, sampled through the online platform Prolific, who had two children enrolled in kindergarten through 12 grade.\textsuperscript{23} Forty-six percent resided in the UK, 33% in the US, 13% in continental Europe, and 8% elsewhere.

Figure 1 presents a visual representation of the experimental process for both the lower- and higher-income experiments. See Appendix D.1 for more detail on the procedures for each experiment.\textsuperscript{24} While the processes were similar, the higher-income experiment omitted some steps due to feasibility and/or brevity because it was conducted online.

In both experiments, we first gathered baseline data. To measure the perceived production function translating tutoring to outcomes, we then measured parents’ beliefs of each

\textsuperscript{22}This adds a second layer of uncertainty: in addition to there being a lottery within scenarios about which ticket would be chosen, there is also a lottery across scenarios determining which scenario would be chosen. Since inequality aversion in our model is over expected opportunities and outcomes, this raises a question regarding the level at which parents evaluate the expectation. We assume that parents “narrowly bracket” and try to equalize expected opportunities and outcomes \textit{within} scenarios; Exley and Kessler (2019) show that people generally narrowly bracket equity concerns. This assumption is conservative for estimating inequality aversion: if instead parents try to equalize expected opportunities and outcomes \textit{across} scenarios, that would bias us away from detecting inequality aversion.

\textsuperscript{23}Thirteen-percent of older and 60% of younger children were below grade 5, 38% of older and 26% of younger children were between grades 5-7, and 49% of older and 14% of younger were above grade 7.

\textsuperscript{24}The full scripts for both experiments are available online in the \textit{Supplementary Materials}. 

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Figure 1: Experimental Procedures

1. **Baseline Survey**: Collect demographics and other data

2. **Elicit Beliefs**: Ask parents for beliefs about each child’s performance on the math test with and without 1 hour tutoring.\(^1\)

3. **Explain Design**: Give overview of entire experiment and assess understanding

4. **Practice and Validation**: Parents allocate tickets across kids in 2 practice scenarios; placebo lottery

5. **Parents Allocate Tickets**: Parents allocate tickets across kids in 5 experimental scenarios.\(^1\)

6. **Lottery for Tutoring**: Scenario randomly selected; tickets assigned based on parents’ choices; one ticket selected per household

7. **Confounds Survey**: Short parent survey to gauge understanding and confounds

8. **Tutoring**: “Winning child” in each household gets an hour of tutoring

9. **Test**: Both children take the math test

10. **Payment**: Reward money handed to children in individual envelopes

Notes: The experimental script for steps 3-5 of both experiments are available online in the Supplementary Materials.

\(^1\) Since the survey in the higher-income experiment was administered online, we included three ‘attention checks’ to gauge whether participants were paying attention. The first two occurred during belief elicitation (step 2) and the third occurred during the experimental ticket allocation (step 5).
child’s test score and of the amount each child’s test score would increase with tutoring.

Next, we explained the design of the experiment to the parents. Because the experiment involved multiple steps, our explanation was very detailed, including visual aids, demonstrations, and questions assessing understanding. For example, in the lower-income experiment, surveyors walked parents through two “practice” scenarios before the actual experiment. In the higher-income experiment, we included five questions that tested respondent understanding of the experimental design.

During the explanations of both experiments, we emphasized that children would not be told that their parents’ choices influenced which child received tutoring. Instead the children were told that the winner was decided by lottery. This allays concerns that unequal opportunity might be more visible to children in our experiment than elsewhere, and means we measure parents’ preferences ignoring social pressure from their children to equalize.\footnote{In the real world, parents might equate to placate their children. By shutting down this channel, our experiment is conservative in estimating preferences for equality.}

After the explanation of the design, parents made their ticket allocations for each of the five experimental scenarios. For each scenario, we described the payment functions before parents made their allocations. We also told parents how to allocate their tickets if they wanted to maximize returns (total expected experimental earnings), equalize outcomes (child-specific experimental earnings), equalize opportunities, or simply choose which child they wanted to receive tutoring. To avoid leading parents towards one particular response, we told them how to distribute tickets for each of the main potential allocation preferences.

We included these explanations to ensure that parents understood the setup and that departures from returns maximization did not reflect poor understanding. Section 4.5 presents evidence that these instructions did not lead parents towards certain responses.

Next, in the lower-income experiment only, we implemented the lottery to select the tutoring winner. One scenario was randomly selected, tickets were assigned based on the parents’ choices for that scenario, and then one ticket was selected for tutoring. The “winning child” in each household then received one hour of tutoring, after which all children took the test. Immediately after the test, cash earnings were delivered directly to each individual child in an envelope.

In both experiments, we implemented a short “confounds survey” after ticket allocations were made to gauge understanding of the experiment and assess potential confounds.
Additionally, to investigate whether parents or children had reallocated earnings after the experiment, we conducted a follow-up survey of households from our lower-income experiment from August to October 2019. We interviewed the parents and children separately.\footnote{We located and surveyed 259 (out of 289) parents and 392 (out of 578) children.}

### 3.3 Identification Approach and Scenarios

We now describe the payment functions for each scenario in the experiment and how they allow us to identify the parameters of the parental utility function specified in Section 2. In each household, we denote Child L and Child H as the child who the parent believed would have lower and higher test scores without tutoring, respectively.\footnote{If parents’ beliefs about both their children’s test scores were equal, we arbitrarily defined Child L as the child whose first name came first alphabetically in the lower-income experiment (29 cases) and as the younger child in the higher-income experiment, where name data were not available (33 cases).} The payment function scenarios differ from each other in two regards: whether they feature lump sum transfers (and, if so, the amounts), and the reward they deliver per point on the test. The payment function for child $i \in \{L, H\}$ from household $k$ in scenario $j$ can be expressed as the sum of a lump-sum transfer $B_{ij}$ plus a reward of $C_{ij}$ per point on the test:

$$P_{ijk} = B_{ij} + C_{ij}(TestScore_{ik} - \text{Threshold}_k)$$

where $P_{ijk}$ is the child’s payment if she receives a score of $TestScore_{ik}$ on the test (test scores are percentage scores out of 100). $\text{Threshold}_k$ equals parent $k$’s belief about child $L$’s test score without tutoring, rounded down to the nearest 10; the threshold thus varies across households but, within household, is the same for both children. We reward performance above the threshold only in order to implement steep payment functions while keeping total payments reasonable.\footnote{We did not tell parents when eliciting beliefs that the payment functions would depend on their beliefs.} We suppress the $k$ index going forward.

**Generating Predictions** Figure 2 presents the five payment function scenarios used in the experiment as well as the predictions for how parents would allocate their tickets in each scenario if their utility functions only weighted (a) returns-maximization, (b) equality of outcomes, or (c) equality of opportunities. We generate the predictions as follows: In each scenario, the returns-maximizing strategy is to give all tickets to the child with larger expected payment gains from tutoring, calculated as the parent’s belief about each child’s test score gains from tutoring (denoted $R_i$) multiplied by the child’s scenario-specific payment.
## Figure 2: Payment Function Scenarios and Predictions

**Payment Function Scenarios**  
Child $i$'s payment = $B_i + C_i(\text{TestScore}_i - \text{Threshold})^1$  

<table>
<thead>
<tr>
<th>Scenario</th>
<th>Child L $^2$</th>
<th>Child H</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$B_L$</td>
<td>$C_L$</td>
<td>$B_H$</td>
<td>$C_H$</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Higher</td>
<td>Lower$^3$</td>
<td>Higher</td>
<td>Lower$^3$</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1. Base Case</td>
<td>0</td>
<td>1</td>
<td>10</td>
<td>0</td>
<td>10</td>
<td>0</td>
<td>10</td>
<td>0</td>
<td>10</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2. Higher Returns to Child H</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>10</td>
<td>0</td>
<td>10</td>
<td>10</td>
<td>H$^7$</td>
<td>L</td>
<td></td>
<td></td>
</tr>
<tr>
<td>3. Higher Returns to Child L</td>
<td>0</td>
<td>10</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>10</td>
<td>10</td>
<td>L$^8$</td>
<td>H</td>
<td></td>
<td></td>
</tr>
<tr>
<td>4. Lump Sum to Child L</td>
<td>80</td>
<td>1</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>10</td>
<td>10</td>
<td>H</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>5. Higher Returns to Child L &amp; Lump Sum to Child H</td>
<td>0</td>
<td>10</td>
<td>500</td>
<td>1</td>
<td>0</td>
<td>10</td>
<td>10</td>
<td>L$^8$</td>
<td>L</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Returns Maximization$^4$</th>
<th>Equality of Outcomes</th>
<th>Equality of Opportunity</th>
</tr>
</thead>
<tbody>
<tr>
<td>If $R_L &lt; R_H$: H</td>
<td>If $R_L = R_H$: L</td>
<td>Equal</td>
</tr>
<tr>
<td>If $R_L = R_H$: No prediction</td>
<td>If $R_L &gt; R_H$: L$^5$</td>
<td>Equal</td>
</tr>
<tr>
<td>If $R_L &gt; R_H$: Depends on parameters$^6$</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes:  
1. Threshold is the parent’s belief about Child L’s test score without tutoring, rounded down to the nearest 10.  
2. Child L (lower-performing child) defined as the one whom the parent perceived would have a (weakly) lower test score without tutoring.  
3. The top value listed in the cell is the parameter value from the higher-income experiment and is listed in USD for participants outside the UK and in GBP for participants from the UK (we did not use a currency conversion, so, for example, if a US participant saw a value of 1 USD, a UK participant would see a value of 1 GBP). The bottom value is the parameter value from the lower-income experiment and is listed in MWK.  
4. $R_i = \text{parent’s belief about child } i\text{’s score with tutoring} - \text{belief about child } i\text{’s score without tutoring}$. 66% of parents in the lower- and 10% of parents in the higher-income experiment believed $R_L < R_H$. The percentages were 19% and 29% for $R_L = R_H$, and 14% and 61% for $R_L > R_H$.  
5. Assumes test score for Child L without tutoring is strictly less than for Child H without tutoring. If equal, no prediction.  
6. In the lower-income experiment: L for 83% of parents; H for 12% of parents; No prediction for 5% of parents. For the higher-income experiment: L for 97% of parents; H for 2% of parents; No prediction for 1% of parents.  
7. For 96% of parents in the lower-income experiment and 96% in the higher-income experiment.  
8. For 95% of parents in the lower-income experiment and 95% in the higher-income experiment.
function slope $C_{ij}$. Thus, of the payment function parameters, only $C_{ij}$ matters, not the lump sum amount $B_{ij}$. In contrast, the strategy to equalize outcomes is to minimize the cross-child expected payment gap, which depends on both $B_{ij}$ and $C_{ij}$ because both determine expected payments. Finally, the strategy to equalize opportunities is to equate tickets across children regardless of $B_{ij}$ or $C_{ij}$.

**Identification Approach**  Figure 2 shows the predictions for each scenario separately, but only for ease of illustration: our analysis primarily uses cross-scenario variation for identification. The cross-scenario (i.e., within-parent) variation originates from the experimental variation in the payment functions. It controls for the endogenous child- and parent-level factors affecting choices, such as parents’ beliefs about the benefits for tutoring for each of their children, $R_i$. It also controls for child-specific preferences, which are constant across scenario. In addition, although the predictions shown in Figure 2 assume that the only value of the tutoring to parents is the short-run monetary payments their children will receive (and thus that parents ignore any potential long-run or non-monetary benefits of tutoring), this assumption is again only for expositional purposes. The cross-scenario predictions we take to the data are robust to parents believing that tutoring has additional value, as that belief is also constant across scenarios and hence controlled for in cross-scenario comparisons.

There is one important hypothesis test we perform that does not depend on cross-scenario variation: the test of whether parents only choose all-or-nothing allocations (a test that both $\alpha = 0$ and $\beta = 0$). The test does not require cross-scenario variation because it holds regardless of parents’ beliefs about their children’s test scores or the additional value to tutoring, obviating the need for cross-scenario variation to control for beliefs.

**Scenarios and Predictions** In our Base Case scenario (Scenario 1), the payment functions for both children are the same: both receive the same positive reward for each test score point above the threshold ($C_L = C_H > 0$), with no lump-sum transfers ($B_L = B_H = 0$). For example, in the higher-income experiment, $C_L = C_H = 1$ USD. The other four scenarios are variations on the base case designed to identify the utility function parameters.

We designed two scenarios to yield opposite predictions for returns-maximization and

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29 The assumption is also consistent with survey data: 93% parents in the lower-income experiment and 70% in the higher-income experiment said they were only thinking about the short-run monetary payment returns to tutoring when making their experimental ticket allocations.

30 As discussed in Section 2, the one exception would be if parents split to balance returns-maximization and child-specific preferences - a possibility we test below with Scenario 5.
equality of outcomes, thus letting us test which preference parents weigh more heavily on average. Scenario 2, *Higher Returns to Child H*, gives Child H a ten times higher per-point reward than Child L while leaving Child L’s per-point reward the same as in the Base Case. Neither child receives a lump sum. Increasing $C_H$ has two effects. First, it increases the returns to receiving tutoring for Child H relative to Child L enough that, for 96% of parents, the returns-maximizing strategy is to give all tickets to Child H. Second, it increases Child H’s expected payments. As a result, the outcome-equalizing choice is to give all tickets to Child L to mitigate the gap in expected payments. Scenario 3, *Higher Returns to Child L*, exchanges the payment functions used in *Higher Returns to Child H* between Child L and Child H. Relative to *Higher Returns to Child H*, returns-maximizing parents would now reallocate to Child L, whereas outcomes-equalizing parents would do the opposite.

Our next scenario, *Lump Sum to Child L* (Scenario 4), was designed specifically to test for a preference for equality of outcomes. Relative to the Base Case, *Lump Sum to Child L* delivers a lump sum transfer, $B_L$, to Child L while delivering no lump sum transfer to Child H. For both children, the per-point rewards $C_i$ remain the same as in the Base Case. Increasing Child L’s lump sum transfer, $B_L$, should not change his or her expected cash rewards from receiving tutoring, $R_L C_L$, and thus does not affect parents’ returns-maximizing choices, nor does it affect their opportunity-equalizing choices. However, lump sum transfers do affect the outcomes-equalizing choice: since giving a lump sum transfer to one child increases his or her expected payments, an outcomes-equalizing parent would respond by reallocating tickets to that child’s sibling to increase the sibling’s expected payments and decrease the expected payment inequality. We made the lump sum transfers large to ensure that parents who care about equality of outcomes would respond: 1,000 MWK, or around 70% of the adult daily wage, in the lower-income experiment and 80 USD in the higher-income experiment.31

In both the *Higher Returns to Child H* and *Higher Returns to Child L* scenarios, both equality of outcomes and returns maximization dictate that the parent should give all tickets to only one child. However, in both cases, the child dictated by each strategy is different. Thus, if parents care about both returns maximization and equality of outcomes, they could in theory choose split allocations to balance those two opposing forces.

To shed light on whether split allocations reflect a positive preference for equality of

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31 The daily wage for adults in the lower-experiment was estimated to be around 1,400 MWK by field staff at the time of the study. Appendix D.2 explains how we chose the payment function parameters.
opportunities, we introduce a final scenario, Higher Returns to Child L & Lump Sum to Child H (Scenario 5), that adjusts the payment functions so that the child a returns-maximizing parent would choose is the same as the child an outcomes-inequality-minimizing parent would choose (both are Child L). As a result, if we see similar numbers of parents choosing split allocations in that scenario, it is unlikely that the splitting reflects a balance between equality of outcomes and returns maximization. Instead, it suggests parents care directly about minimizing inequality in opportunities. Note that this prediction depends on parents not having important child-specific preferences for Child H. To assess this possibility, we also test the prediction excluding parents who appear to have preferences for Child H in Appendix B.1.

Another distinctive prediction of equality of opportunity—testable using any of our scenarios—is that there should be excess mass (relative to a smooth distribution) in the density of choices at the equal allocation point. No other theories should produce this excess mass, since all other factors (e.g., outcomes inequality or expected returns) are smooth through the equal-allocation point.

**Identifying Variation**  
Figure 3 depicts the scenarios and identifying variation graphically. For each scenario, there are two bars, with the left bar showing the case where parents give all 10 tickets to Child H and the right bar showing the case where parents give all 10 tickets to Child L. The height of each bar represents expected household earnings in that case (i.e., the sum of Child L’s and Child H’s expected earnings), averaged across all households.

There are a few takeaways from Figure 3. First, in Scenarios 2, 3, and 5, where the payment per test score point $C_i$ varies substantially across children, there are large within-scenario differences in expected household earnings depending on whom the parent gives tickets to. For example, in Scenario 2 (Higher Returns to Child H), expected household earnings in the lower-income experiment would decrease by 1,340 MWK, or 96% of an adult’s daily wage, if parents allocated all of their tickets to Child L instead of Child H. In the higher-income experiment, expected household earnings would decrease by 54 USD. Thus, departures from returns-maximization are costly. Second, there are meaningful differences across scenarios in terms of the total earnings, earnings distribution, and which child is the higher-return child. Third, although in several scenarios the average parent cannot perfectly equalize Child L’s and Child H’s expected payments, parents’ allocations can still make meaningful headway towards equality. For example, in Scenario 1, the average parent in
Notes: This figure presents, for each scenario in each experiment, the mean perceived expected experimental earnings for Child H and Child L in the cases when all ten tickets are given to Child H (bar “H”) and when all ten tickets are given to Child L (bar “L”). The bars representing Child H and Child L earnings are additive, so that the total height represents the total earnings for that allocation.

both experiments will decrease payment inequality between Child L and H by roughly 80% if she chooses the ticket allocation that minimizes payment inequality rather than the ticket allocation that maximizes payment inequality.

Beliefs Uncertainty Our experiment identifies preferences based on parents’ beliefs about their children’s scores, which are often inaccurate (Banerji et al., 2017; Dizon-Ross, 2019). Fortunately, our identification does not require parents’ beliefs to be accurate: we are identifying parents’ preferences conditional on their beliefs. Potential inaccuracies should only affect our estimation if parents’ beliefs distributions are uncertain and if that uncertainty affects their choices. However, uncertainty does not seem to play a large role here. Parents in both experiments express a very high degree of certainty in their beliefs about their children’s test scores, with and without tutoring (see Table 1). In addition, as we discuss in Appendix B.1, two additional analyses – heterogeneity analysis based on uncertainty measures, and directly asking parents whether they would change their allocations if they knew their children’s scores with certainty – suggest that the effect of uncertainty is limited.

3.4 Validation of Method and Respondent Understanding

In this section, we present evidence suggesting that the vast majority of parents understood the experiment and how to maximize returns. In Section 4.5, we show our results are
robust to excluding the few parents who did not fully understand.

First, during the explanation of the design, we asked parents questions about how to allocate tickets to accomplish different goals, such as how to maximize returns or equalize outcomes. Appendix Table A.1 shows that the vast majority of parents answered the questions correctly. In the higher-income experiment, we also asked several “attention check” questions to make sure respondents were paying attention; Appendix Table A.1 shows that the vast majority of parents also answered these questions correctly.

Our next two pieces of evidence are available from the lower-income experiment only. First, before the experiment, we conducted a placebo lottery that asked parents to allocate 10 lottery tickets between two (hypothetical) prizes just like they did with real stakes in the main experiment. However, here the two prizes were monetary prizes to be given directly to the parent: 50 MWK (0.07 USD) or 100 MWK. As shown in Figure 4, 97% (280/289) of parents allocated 100% of the tickets to the larger 100 MWK prize, suggesting that the vast majority of those parents understand how to maximize returns in a lottery.

Second, after we elicited allocations for each scenario in the lower-income experiment, we asked parents about the rationale behind their chosen allocation. Appendix Table A.2 shows that parents’ stated rationales line up well with their actual choices.

3.5 Summary Statistics

Table 1 reports selected summary statistics from our sample. Roughly 85% of respondents in the lower-income experiment were female compared to 62% in the higher-income experiment. On average, parents in both experiments believed that Child H’s score on the
test without tutoring would be 15 points higher than Child L’s.

Our approach depends on parents perceiving that the test score returns to tutoring are positive. Critically, 99% of parents in the lower-income experiment and 97% of parents in the higher-income experiment thought it would have positive returns for at least one of their children, while 93% in the lower-income experiment and 83% in the higher-income experiment thought it would have positive returns for both. In the lower-income experiment, parents generally believed that tutoring would have higher absolute score returns for Child H than Child L, increasing Child L’s score by 11 points and Child H’s by 15 points, on average. However, the opposite was true for parents in the higher-income experiment, with tutoring expected to increase Child L’s score by 10 and Child H’s score by 6, on average.
## Table 1: Summary Statistics

### A. Respondent Characteristics:

<table>
<thead>
<tr>
<th></th>
<th>Lower-income</th>
<th>Higher-income</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Completed Grade 8</strong></td>
<td>0.33 (0.47)</td>
<td>.</td>
</tr>
<tr>
<td><strong>Completed High School</strong></td>
<td>. 0.93 (0.25)</td>
<td>. (0.49)</td>
</tr>
<tr>
<td><strong>Female</strong></td>
<td>0.85 (0.36)</td>
<td>0.62 (0.49)</td>
</tr>
<tr>
<td><strong>Household income (USD)</strong></td>
<td>. 76,649 (0.00)</td>
<td>. (51,158)</td>
</tr>
<tr>
<td><strong>United States</strong></td>
<td>0.00 (0.00)</td>
<td>0.33 (0.47)</td>
</tr>
<tr>
<td><strong>United Kingdom</strong></td>
<td>0.00 (0.00)</td>
<td>0.46 (0.50)</td>
</tr>
<tr>
<td><strong>Malawi</strong></td>
<td>1.00 (0.00)</td>
<td>0.00 (0.00)</td>
</tr>
<tr>
<td><strong>Other location</strong></td>
<td>0.00 (0.00)</td>
<td>0.21 (0.41)</td>
</tr>
</tbody>
</table>

### B. Child Characteristics:

<table>
<thead>
<tr>
<th></th>
<th>Lower-income</th>
<th>Higher-income</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Grade</strong></td>
<td>5.87 (0.81)</td>
<td>4.92 (3.17)</td>
</tr>
<tr>
<td><strong>Female</strong></td>
<td>0.55 (0.50)</td>
<td>0.55 (0.50)</td>
</tr>
<tr>
<td><strong>Annual household education expenditure (MWK) on child</strong></td>
<td>8412 (9316)</td>
<td>8372 (9020)</td>
</tr>
<tr>
<td><strong>Parent believed child had positive returns to tutoring</strong></td>
<td>0.96 (0.20)</td>
<td>0.96 (0.20)</td>
</tr>
<tr>
<td><strong>Parent’s belief of score without tutoring (out of 100)</strong></td>
<td>53.14 (13.26)</td>
<td>72.61 (17.36)</td>
</tr>
<tr>
<td><strong>Parent’s belief of score with tutoring (out of 100)</strong></td>
<td>64.43 (14.02)</td>
<td>82.55 (14.71)</td>
</tr>
<tr>
<td><strong>Parent believed child had strictly higher returns to tutoring than sibling</strong></td>
<td>0.14 (0.35)</td>
<td>0.10 (0.30)</td>
</tr>
<tr>
<td><strong>Certainty of belief about score with tutoring (out of 10)</strong></td>
<td>9.42 (5.92)</td>
<td>9.55 (3.46)</td>
</tr>
<tr>
<td><strong>Certainty of belief about score without tutoring (out of 10)</strong></td>
<td>9.42 (6.81)</td>
<td>7.54 (2.10)</td>
</tr>
<tr>
<td><strong>Math test score (out of 100)</strong></td>
<td>41.91 (25.16)</td>
<td>44.14 (24.23)</td>
</tr>
<tr>
<td><strong>Received tutoring</strong></td>
<td>0.43 (0.50)</td>
<td>0.57 (0.50)</td>
</tr>
</tbody>
</table>

Total Households: 289

Notes: This table presents summary statistics for the lower-income experimental sample (left columns) and the higher-income experimental sample (right columns), separately. All statistics are proportions unless otherwise indicated. Standard deviations are in parentheses. Any measures only displayed for the lower-income or higher-income experimental samples were not collected in the other sample.

1. Household income was collected by Prolific and reported in GBP for all participants regardless of their location. The exchange rate was 1 GBP:1.4 USD at the beginning of the higher-income experiment.
2. Grade values range from 0-12, with Kindergarten coded as 0.
3. “Returns” to tutoring is defined as the difference between beliefs of the test scores with and without tutoring.
4. Not baseline statistics but rather statistics describing what happened during the experimental tutoring and testing.
4 Experimental Results

This section presents the main results of the experiment. First, we analyze the raw data and compare the results across our experimental scenarios to provide qualitative evidence on preferences. Second, we estimate the magnitudes of the preference parameters using structural estimation. Third, we compare our experimental results with the results of survey questions assessing stated preferences. Finally, we present evidence against possible confounds and rule out alternative explanations for our experimental findings.

4.1 Qualitative Evidence for Each Type of Preference

**Returns Maximization.** We first test whether parents only value maximizing returns. If so, they should allocate their tickets in an all-or-nothing fashion. Instead, Figure 5 shows that only 44% and 51% of allocations were all-or-nothing in the lower-income and higher-income experiments, respectively. This means that, in roughly half of the allocations in each experiment, both children received tickets. Thus, parents appear not to be pure returns-maximizers, but rather care about equality and fairness as well.

Figure 5: Parents Often Did Not Maximize Returns in the Experiment

![Figure 5](image)

Notes: This figure presents the number of tickets given to Child L, pooled within each experiment across scenarios 1-5, and 95% confidence intervals. The prediction of returns maximization is that all allocations would give 0 or 10 tickets to Child L. Since there were 10 tickets total, the number of tickets given to Child H is 10 - (Tickets to Child L).

Although parents are not *pure* returns-maximizers, Figure 6 presents evidence that parents do still place positive weight on returns maximization. Figures 6(a) and 6(b) compare Scenario 2 (*Higher Returns to Child H*) and Scenario 3 (*Higher Returns to Child L*) for the
Figure 6: Returns Maximization Always Dominated Outcomes Equalization in the Lower-Income Experiment, and Often Dominated in the Higher-Income Experiment.

Notes: Each of these figures present the allocation of lottery tickets for two separate scenarios in each experiment. The arrow labeled “Returns Max.” shows the direction that returns maximization predicts allocations would move when going from the solid scenario to the outlined scenario, and the arrow labeled “Equalize Outcomes” shows the direction that equality of outcomes predicts allocations would move when going from the solid to the outlined scenario.
lower- and higher-income experiments, respectively. Switching from Scenario 2 to Scenario 3 increases the payment per test score point for Child L relative to Child H. As a result, returns maximization suggests parents should reallocate tickets to Child L, as indicated by the “Returns Max.” arrow. In contrast, increasing the payment per test score point for Child L also effectively makes Child L richer. If parents were inequality averse over outcomes, they would reallocate in the opposite direction, as indicated by the “Equalize Outcomes” arrow. Consistent with returns-maximization, we find that parents in both experiments increase their allocations to Child L. In the lower-income experiment, the tickets allocated to Child L increase by a statistically significant 1.6 tickets (Figure 7), with over 20 percentage points (pp) fewer parents giving all of their tickets to Child H, and 10 pp more parents giving all tickets to Child L (Figure 6(a)). In the higher-income experiment, the magnitudes of the shifts are smaller but still meaningful: the tickets allocated to Child L increase by a statistically significant 0.5 tickets, with over 5 pp fewer parents giving all tickets to Child H, and 4 pp more parents giving all tickets to Child L.

Figure 7: The Tickets Allocated to Child L Varied Across Scenarios

![Figure 7](image)

(a) Lower-income experiment  
(b) Higher-income experiment

Notes: This figure presents average lottery tickets allocated (out of 10) to Child L in each experiment. The 95% confidence intervals on the bars for Scenarios 2-5 represent tests of equality between each scenario and the Base Case. The dotted line represents the average for the base case which implies there are significant differences when the confidence intervals do not intersect with this line. The p-value for the comparison between Scenario 2 and 3 is 0.01 in the lower- and 0.08 in the higher-income experiment. The p-value for the comparison between Scenario 3 and 5 is 0.07 in the lower- and 0.01 in the higher-income experiment.

Although these shifts in allocations are meaningful, it is important to note that they are still quite muted relative to a pure returns-maximizing response. For example, if parents were maximizing experimental earnings, the share of parents giving all tickets to Child L
in Figure 6(a) should go from 0 to 100%, not 15% to 25%. The fact that parents deviate from returns maximization may be particularly surprising given that the potential costs are large. For example, in Scenario 2 of the lower-income experiment, Child L’s average expected experimental gains from tutoring are a whole 983 MWK—70% of a day’s wage—higher than Child H’s, and the differences are even larger in Scenario 3. This suggests that parents also place high weight on preference components other than returns maximization.

Figures 6(c) and 6(d) present a similar comparison between Scenarios 2 (Higher Returns to Child H) and 1 (Base Case). The exact magnitudes of the shifts are different, but the takeaway is the same: parents place positive weight on maximizing returns, and their desire to maximize returns can dominate their potential desire to equalize outcomes, on average.

Figures 6(e) and 6(f) presents the final pairwise comparison of scenarios varying the payment-per-score-point, between Scenarios 3 (Higher Returns to Child L) and 1 (Base Case). For the lower-income experiment, the takeaway is the same as in the previous comparisons. However, in the higher-income experiment, parents on average reallocate in the direction predicted by outcomes equalization, not by returns-maximization. This suggests that the higher-income experimental sample may value equality of outcomes positively and more so than the lower-income experimental sample. We now implement a test to isolate this preference for equality of outcomes absent concerns for returns-maximization.

**Equality of Outcomes.** Relative to Scenario 1 (Base Case), Scenario 4 (Lump Sum to Child L) delivers a lump sum to Child L without changing the per-point rewards for either child. The only theory that predicts parents will react to this change is equality of outcomes, which predicts that parents would reallocate towards Child H.

In the lower-income experiment, in aggregate, we fail to find evidence for reallocation towards Child H (Figure 8(a)). If anything, parents reallocate slightly in the opposite direction. When we examine individual-level changes, however, equal numbers of parents reallocate to and away from Child L (roughly 20% in either direction), suggesting that these reallocations likely represent noise. However, in the higher-income experiment, parents on average reallocate towards Child H, consistent with a desire to equalize outcomes. Figure 8(b) shows that 15 pp more parents give all of their tickets to Child H, and over 10 pp fewer parents give all tickets to Child L. This increases the tickets allocated to Child H by a statistically significant 1.4 tickets (Figure 7). In Section 4.2, we present heterogeneity analysis suggesting that

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32 We find consistent results in both experiments when comparing Higher Returns to Child L (Scenario 3)
one explanation for the difference in the preference for equality of outcomes between the two experiments is the difference in income between the two experimental samples.

Figure 8: When One Child Received a Lump Sum, Only Parents In the Higher-Income Experiment Reallocated to His or Her Sibling.

![Graphs showing allocation of lottery tickets for two separate scenarios: Base Case and Lump Sum to Child L in both experiments.](image)

Notes: Each of these figures present the allocation of lottery tickets for two separate scenarios: Scenario 1 (Base Case) and Scenario 4 (Lump Sum to Child L) in both experiments. The arrow “Equalize Outcomes” shows the direction that equality of outcomes predicts allocations would move when going from the solid to the outlined scenario.

**Equality of Opportunities.** Taken together, the evidence presented so far suggests that, in the higher-income experiment, parents value both returns-maximization and equality in outcomes. In contrast, in the lower-income experiment, parents appear to value both returns-maximization and some form of equality, but do not appear to value equality in outcomes. This suggests that they likely value equality of opportunity.

We substantiate this conclusion with additional analysis, and provide evidence that parents in the higher-income experiment care about equality of opportunity as well. In particular, a concern for equality of opportunity is the only theory that predicts excess mass at 50%. Consistent with this, visual inspection of the data in Figure 5 shows a notable spike at equal allocation, with parents choosing exactly equal tickets in roughly 37% of the scenarios in the lower-income experiment and 31% of the scenarios in the higher-income experiment. Moreover, we can easily reject that the ticket distribution is smooth around the 5/5 point. Appendix Table A.3 also shows that a substantial share of parents in both the lower- and higher-income experiments chose exactly-equal opportunities in each scenario, and Higher Returns to Child L & Lump Sum to Child H (Scenario 5). These scenarios were included in the design for other reasons but the only difference between them is again the lump sum.
even the scenarios where there are large differences in expected earnings across children. Specifically, in each scenario, at least 30% of parents in the lower-income experiment and 24% of parents in the higher-income experiment chose the 5/5 allocations. The desire to equate also appears to be widespread across parents, with 58% of parents in each experiment equalizing at least once. Section 4.5 discusses and rules out other potential explanations for equal splitting.

**Child-Specific Preferences.** Interestingly, many of parents’ choices diverge from either the returns-maximizing, outcome-equalizing, or opportunity-equalizing choices, suggesting that parents may also care about something else. Appendix Figure A.1 provides evidence that this behavior represents child-specific preferences that vary across parents. Parents who allocated all tickets to Child H or Child L in the *Base Case* scenario (which featured symmetric payment functions) were substantially more likely to allocate all tickets to that same child in all the other scenarios, explaining most of the deviations we described above from the other strategies. Note that the “child-specific preferences” that we are identifying here incorporate both actual child-specific preferences (e.g., preferences for a certain gender) and beliefs and preferences regarding the cross-child differences in the non-experimental benefits of tutoring (e.g., non-monetary or long-run benefits). Both of these factors could lead parents to prefer one child to the other and do not vary across scenarios.

While child-specific preferences appear to be important forces for both the lower-income and higher-income experimental samples, the preferred child appears to differ across the two samples. Parents in the higher-income experiment are more likely to prefer their lower-performing child, whereas parents in the lower-income experiment are more likely to prefer their higher-performing child. One can see this preference suggestively in Figure 5, and the structural estimation, which we present below in Section 4.2, confirms it. Table A.4 also shows that the difference holds controlling for the characteristics of the lower and higher-performing children in each experiment (e.g., age, gender). Although there are many potential explanations, the higher-income sample’s greater preference for investing in their lower-performing children could stem from their greater preference for equality of outcomes. While our experiment identifies the value placed on equality of experimental outcomes (i.e., payments), a value for equality of *non-experimental* outcomes (e.g., long-run earnings) would manifest as child-specific preferences for the lower-performing child.
Parent “Types.” Thus far, we have mainly examined average behavior across the population, but the within-person variation induced by our experiment also enables us to examine individual behavior. Figure 9 shows the share of the sample that always, across all scenarios, equalized outcomes, maximized returns, etc. Roughly half of the parents in each experiment adopted the same strategy in all scenarios, and the other half mixed their strategies across the scenarios (not depicted in the figure). In both experiments, between 15 and 20 percent of parents always equalized opportunities ($p$-val for equality across experiments: 0.214). The mix of other types varies significantly across experiments, with more parents in the higher-income experiment always equalizing outcomes, giving all tickets to Child L, or maximizing returns, and more in the lower-income experiment always giving all tickets to Child H.

Figure 9: Parents Have Heterogeneous Behavioral Patterns

Notes: These figures present the proportion of each sample of parents who always, in each of the 5 scenarios, equalized outcomes (bar 1), maximized returns (bar 2), allocated all tickets to Child L or Child H (bars 3 and 4, respectively), or split tickets equally (bar 5). The proportion of parents who have choice patterns that do not fit into the previous categories is 0.54 in both experiments. We tested whether the proportion of parents in each category was equal across experiments and report the following $p$-values: Outcome Equalizers $p$-val < 0.01, Return Maximizers $p$-val < 0.01, Child L $p$-val < 0.01, Child H $p$-val < 0.01, Opportunity Equalizers $p$-val = 0.214.

Forgone Earnings from Not Maximizing Returns. Finally, we show that parents’ deviations from returns maximization have significant earnings implications. For each family × scenario, we calculate the sum of the children’s expected earnings under their parents’ chosen ticket allocations (“chosen earnings”) and compare those with expected earnings if parents had instead chosen to maximize the sum of their children’s earnings (“returns-maximizing
earnings”) or minimize the sum of their children’s earnings (“returns-minimizing”). Figures 10(a) and 10(b) plot forgone earnings (returns-maximizing earnings minus chosen earnings) for each experiment. The experimental stakes are substantial — in the scenarios where we exogenously varied the returns to tutoring across children (Scenarios 2, 3, and 5), the forgone amounts are correspondingly large. Figures 10(c) and 10(d) plot forgone earnings as a percent of “potential earnings” (returns-maximizing minus returns-minimizing earnings). On average, across scenarios, parents forwent roughly 40% of their potential (non-inframarginal) earnings in the lower-income experiment, and 30% in the higher-income experiment.

Figure 10: Parents Forgo Substantial Experimental Earnings

(a) Forgone Earnings in MWK, Lower-income experiment

(b) Forgone Earnings in USD, Higher-income experiment

(c) Forgone Earnings as % Potential Earnings, Lower-income experiment

(d) Forgone Earnings as % Potential Earnings, Higher-income experiment

These forgone-earnings estimates combine the impacts of preferences for equality of opportunity, equality of outcomes, and child-specific preferences. To disentangle the role of each specific preference, we adopt a simple structural approach in the next section.
4.2 Quantifying Preferences

This section uses our experimental results to estimate parents’ average preference weights from Section 2: $\lambda$, $\alpha$, $\beta$, and $\gamma$. We use a mixed logit regression model to estimate these parameters. The parameters are identified from variation across parents’ potential choices in opportunities and expected payments. The mixed logit allows the preference parameters to vary across the population and avoids the independence of irrelevant alternatives assumption (IIA) entailed by a simpler conditional logit approach. Following equation (1), we assume that parent $i$ has the following utility in scenario $j$ from choosing ticket allocation $k$:

$$u_{ijk} = \lambda_i \text{HouseholdEarnings}_{ijk} - \alpha_i \text{OpportunityInequality}_{ijk} - \beta_i \text{OutcomeInequality}_{ijk} + \gamma_i \text{OpportunitiesToChildL}_{ijk} + \epsilon_{ijk}. \quad (2)$$

$\text{HouseholdEarnings}_{ijk}$ is the total expected combined earnings across both children under allocation $k$, measured in either USD or 100’s of MWK (the returns-maximization term). $\text{OpportunityInequality}_{ijk}$ is the absolute difference in lottery tickets between parent $i$’s children under allocation $k$ (the equality of opportunity term). $\text{OutcomeInequality}_{ijk}$ is the absolute difference between parent $i$’s children’s expected earnings under allocation $k$, measured in either USD or 100’s of MWK (the equality of outcomes term). $\text{OpportunitiesToChildL}_{ijk}$ is the number of lottery tickets given to Child L in allocation $k$ (the child-specific preference term). Since $\gamma_i$ can be positive or negative, parents can prefer either of their children. In each scenario $j$, parent $i$ is assumed to choose the allocation $k$ with the highest utility. We allow for the preference parameters ($\lambda$, $\alpha$, $\beta$, $\gamma$) to vary for each parent $i$. Appendix E.1 describes the assumptions we make to identify the model (e.g., the parameter distributions).

We also estimate variations of this specification. First, because the earlier reduced form evidence suggests that the primary utility cost from not giving equal opportunities is binary instead of continuous, we estimate a specification where the utility cost associated with unequal opportunities is binary. In that specification, $\text{OpportunityInequality}_{ijk}$ is a dummy that equals 1 if allocation $k$ does not give the two children an equal number of tickets and 0 otherwise. Second, to facilitate interpretation, we model $\text{HouseholdEarnings}_{ijk}$ as the log of the total expected earnings combined across both children under allocation $k$.

33We thus assume that the $u(\cdot)$ function from equation (1) is linear. This is for feasibility and to make our estimates easy to interpret. Estimating a non-linear $\text{E}(\text{HouseholdEarnings}_{ijk})$ would require knowing the full distribution of beliefs about test scores (not just expected test scores), which we do not know.
While the primary variation in $\text{HouseholdEarnings}_{ijk}$ and $\text{OutcomeInequality}_{ijk}$ is generated by the experiment itself, parents' beliefs about the returns to investment can also endogenously influence these measures. For example, in Scenario 1, $\text{HouseholdEarnings}_{ijk}$ will only vary with the parent’s ticket allocation if the parent perceives that her two children have different returns to tutoring, but not if the parent perceives that both children have the same returns to tutoring. To correct for potential endogeneity in these regressors, we also adopt the control function approach of Petrin and Train (2010) to isolate the experimental variation. Appendix E.2 describes how we implement the approach.

Table 2 presents results from the real-stakes lower-income experiment in Panel A, and the hypothetical higher-income experiment in Panel B.

**Lower-Income Experiment.** Column 1 displays the mixed logit estimates of the means of the parameter distributions from equation (2). The results are consistent with our Section 4.1 results. First, parents have a strong preference for equalizing opportunities. The coefficient on “Absolute difference in opportunities (tickets)” is significant and large in magnitude, indicating that parents are more likely to pick choices that have smaller ticket differences between children.

Second, we find no evidence of a preference for equality in outcomes, as the coefficient on “Absolute difference in outcomes” is small, wrong-signed, and not statistically significant. Finally, parents are more likely to choose a ticket allocation when the total expected payments associated with that allocation increase (see the coefficient on “Household Earnings”), consistent with them placing a positive weight on returns maximization.

To interpret the magnitude of the preference for equality, it is useful to scale the coefficient relative to the $\text{HouseholdEarnings}$ coefficient, $\lambda$. $\frac{\alpha}{\lambda}$ then gives the amount of household earnings a parent would be willing to forgo to decrease the gap in lottery tickets (i.e. opportunities) between her children by 1. We display these estimates in the table row “WTP for 1 ticket lower opportunity inequality.” Our estimate of $\frac{\alpha}{\lambda}$ is large, implying that, on average, parents in the lower-income experiment are willing to give up 148 MWK (0.20 USD) in expected household earnings for each 1-ticket decrease in opportunity inequality.

Including control functions to address endogeneity does not change the conclusion that parents have a high willingness to pay (WTP) for equality of opportunities. Column 2 of Table 2 shows the estimates of equation (2) when we include the control functions. The estimates suggest that parents are willing to give up 399 MWK or roughly 0.55 USD in
Table 2: Mixed Logit Estimates Suggest Parents Place High Weight on Equalizing Opportunities

<table>
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<tr>
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<th>Dependent Variable: Chose Ticket Allocation</th>
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<td>opportunity inequality (MWK 100s)</td>
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<td>(MWK 100s)</td>
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<td>(percent)</td>
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Notes: This table presents estimates of the predictors of parents’ choice of ticket allocations. Panel A displays results from the lower-income experiment and Panel B from the higher-income experiment. Each observation is a parent × scenario × ticket allocation. “Absolute difference in outcomes” measures the absolute difference in child-level earnings. Willingness to pay (WTP) is calculated by dividing the “Absolute difference in tickets” (Columns 1 and 2) and “Tickets not equally split” (Columns 3, 4 and 5) coefficients by the “Household earnings” (Columns 1, 2, 3 and 4) and “Log Household earnings” (Column 5) coefficients. Columns 2, 4, and 5 are estimated using a control function approach where control functions are used for “Household earnings” and “Absolute difference in outcomes” and the instruments are dummies for the scenario × ticket allocation. Standard errors are in parentheses, clustered at the household level. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.
expected household earnings for each 1-ticket decrease in opportunity inequality, somewhat larger than our baseline willingness to pay estimate. The similarity suggests that the primary variation identifying the mixed logit model is the experimental variation.

To fit the trends visible in the raw data more closely, in column 3 we estimate a variant of equation (2) where we replace the continuous $\text{OpportunityInequality}$ term with a binary term for whether the allocation equally split tickets. We find that parents’ mean WTP to ensure equal opportunities is 1,296 MWK or roughly 1.81 USD, a substantial amount equal to roughly 92% of the daily adult wage in our lower-income setting and 15% of per-child annual education spending. As before, our coefficients of interest remain qualitatively similar after we include the control functions (column 4, Table 2): Parents’ mean WTP to ensure equal opportunities is 1,594 MWK or roughly 2.22 USD.

For ease of interpretation, in column 5, we estimate another version of equation (5) where we model $\text{HouseholdEarnings}_{ijk}$ as the log of the total expected combined earnings across both children under allocation $k$. We find that parents’ mean WTP to ensure equal opportunities is a substantial 50% of total expected combined earnings.

Our mixed logit estimation procedure also allows us to extract parent-level coefficient estimates, which we can then correlate with other parent-level data to validate our estimates. Reassuringly, Table 3 shows that the parent-level preference to equalize opportunities correlates with survey measures capturing inequality in non-experimental investments between children. Parents who have higher WTP to equalize opportunities, as measured by our experiment, have more equality across their children in educational expenditures, time spent by the mother on each child, and time spent by the father on each child.

**Higher-Income Experiment.** The mixed logit estimates from the hypothetical higher-income experiment are directionally consistent with estimates from the lower-income experiment, save three differences. First, the coefficient on the absolute difference in child-level outcomes is negative and significant. This is consistent with the Section 4.1 results suggesting that parents in the higher-income experiment care about equality of outcomes. The magnitude of this preference is also meaningful, with parents willing to give up roughly 0.22-0.62 USD in total household earnings in order to decrease the gap between their children’s earnings by 1 USD. (See the statistics in columns (1)-(4) in the “WTP for 1 USD lower outcome

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34 We can only conduct this analysis in the lower-income experiment, not the higher-income experiment in which, to keep the online survey short, we did not collect survey data on non-experimental investments.
Table 3: Estimated Preference for Equal Opportunities Correlates with More Equal Allocations of Spending and Time in the Lower-income Experiment

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<tr>
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<td>Coefficient on Absolute</td>
<td>Coefficient on Tickets</td>
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<td></td>
<td>Difference in Tickets</td>
<td>Not Equally Split</td>
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<td>A. Household Expenditure</td>
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<td>Above-median abs. gap in</td>
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<td>0.61</td>
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<td>expenditures</td>
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<td>(0.37)</td>
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<td>B. Mother’s Time Use</td>
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<td>Mother’s time not equally</td>
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<td>0.90**</td>
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<td>split</td>
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<td>(0.40)</td>
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<td>Dependent variable mean</td>
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<td>C. Father’s Time Use</td>
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<td>Father’s time not equally</td>
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<td>0.99**</td>
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<td>-2.97</td>
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Notes: This table displays data from the lower-income experiment only. (We did not collect similar data in the higher-income experiment.) Panel A presents heterogeneity in the logit preference coefficients by the difference in children’s share of household expenditure, which is measured by a binary variable that takes value 1 if the absolute gap in expenditure between children is above the median. Panel B presents heterogeneity in the logit preference coefficients by the difference in children’s share of mother’s time, which is measured by a binary variable that takes value 1 if mother’s time is not equally split. Panel C presents heterogeneity in the logit preference coefficients by the difference in children’s share of father’s time, which is measured by a binary variable that takes value 1 if father’s time is not equally split. Standard errors are in parentheses, clustered at the household level. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

inequality (USD)$^{n}$ row at the bottom of Panel B.) Second, while the preference for equality of opportunity is still significant and substantial in magnitude, its magnitude may be somewhat smaller than in the lower-income experiment. For example, column 5 suggests that parents’ mean WTP in percent terms to ensure equal opportunities is 14% of total expected earnings—an estimate which is very substantial, but which is smaller than the 50% estimate from the lower-income experiment. Below, we discuss one potential reason for differences one and two. The third difference is that, consistent with our discussion of child-specific preferences in Section 4.1, the direction of child-specific preferences in the higher-income experiment is the opposite of what it is in the lower-income experiment. Parents on average prefer their lower performing child.
Parent Types. To give a sense of how widespread the preferences for equal opportunity are, Appendix Figures A.2(a) and (b) show the distribution of the parent-level coefficients on \textit{OpportunityInequality} from Table 2, column 3. Eighty percent of parents in the lower-income experiment and 62% of parents in the higher-income experiment place positive weight on equality of opportunities. In the higher-income experiment, 64% of parents place positive weight on equality of outcomes (Figure A.2(d)).

Heterogeneity in Preferences by Income. A large literature documents heterogeneity in how parents allocate investments based on income (e.g., Fan and Porter, 2020; Restrepo, 2016), suggesting that preferences may also vary by income. Could heterogeneity in preferences by income account for the differences in preferences we see across experimental samples and, in particular, the qualitatively different levels of aversion to inequality in outcomes? To shed suggestive light on this hypothesis, we conduct within-sample heterogeneity analysis by household income, regressing the logit preference coefficients on a household income measure.\footnote{Given the difficulties of measuring income in developing countries, we only collected income data from our higher-income experiment and so this analysis is only conducted within that sample. While the displayed results include no controls, the results are robust to including controls such as country fixed effects, respondent age, and respondent gender.} Appendix Table A.5 shows that, within sample, parents from higher-income households care more about equality of outcomes and less about equality of opportunities than lower-income households, as their logit coefficients on outcomes inequality are more negative (i.e., larger in magnitude), on average, and their logit coefficients on opportunity inequality are less negative. These correlations are consistent with income heterogeneity explaining our cross-sample differences in results. Of course, this evidence is only suggestive, and there are many other possible explanations.

4.3 Qualitative Survey Responses

We now summarize parents’ responses to survey questions we asked after each experiment about preferences for allocating opportunity. These questions had two purposes.

The first purpose was to check consistency with our experimental results. Reassuringly, we find a high degree of consistency. For example, we asked parents how they would allocate their spending in the scenario that they had a higher-ability and lower-ability child and thought that spending more on the higher-ability child would yield higher test-score returns. They could choose between spending more on the higher-ability child to maximize the test
score gains, spending more on the low-ability child to try to equalize test scores, or spending equally on both. Figure 11 plots their responses.

There are two main takeaways, which align with our primary experimental findings. First, the modal preference in both experimental samples is to equalize opportunities. Second, there are substantially more parents who prefer to equalize outcomes in the higher-income experimental sample than the lower-income experimental sample.

Figure 11: Survey Responses Show High Preference for Equalizing Opportunities in Both Samples, and Greater Preference for Equalizing Outcomes Among Higher-Income Sample

Notes: This figure displays parents’ responses to survey questions asking them whether they would want to allocate short-run investments in their children in order to equalize opportunities, equalize outcomes, or maximize returns. The height of each bar represents the proportion of parents who indicated each preference for the lower- and higher-income experiments separately. P-values represent tests of equality between the lower- and higher-income samples. Fifty percent of the sample in both experiments were randomly selected to answer this question, which assessed preferences regarding short-run investments (i.e., investment to increase test scores); the other fifty percent answered a question about long-run investments (i.e., investment to increase long-run earnings). The answers to that question are similar and are displayed in Appendix Figures A.3(a) and A.3(b).

The second purpose of our qualitative questions was to explore how preferences regarding equality of opportunity and of outcomes vary across different domains, and we find a high degree of consistency. Within the domain of educational investments, parents maintain a strong preference for equality of opportunity across different investment circumstances. Appendix Figures A.3(a) and A.3(b) show that the preference for equality of opportunity is similarly strong for short-run investments to aid achievement and long-run investments to promote ultimate earnings. Appendix Figures A.3(c) and A.3(d) show the preference is also similarly strong when thinking about how parents would personally allocate opportunities across their own children (relative to hypothetical high- and low-ability children), how
parents would personally allocate opportunities across other peoples’ children,\textsuperscript{36} and, importantly, how the government should allocate educational investment across its constituents. Across all of these questions, the higher-income sample also maintains a larger preference for equalizing outcomes than the lower-income sample (Appendix Figure A.4). We also see high levels of interest in equalizing opportunities instead of outcomes when thinking about the allocation of non-educational opportunities, specifically allocating government aid to business and (re)distributing wealth in society (Appendix Figures A.5 and A.6, respectively).\textsuperscript{37}

### 4.4 Redistribution and Inequality in Consumption

As described in Section 2, our experiment identifies preferences over the outcome of earnings, not consumption, since a preference for equality of earnings is the preference that necessarily affects the distribution of opportunities. That said, one may independently want to understand preferences regarding equality of consumption and redistribution of income. One may also wonder if the absence of aversion to unequal earnings in our lower-income experiment is because those parents care about equality of consumption rather than equality of earnings.

Appendix Table A.6 presents data from the follow-up survey we conducted with parents and children from the lower-income experiment. The data in the table suggest that, even though there were substantial between-sibling earnings gaps, most parents did not redistribute earnings to decrease those gaps. Seventy-seven percent of children with positive earnings indicated that they kept their full earnings. Importantly, only 2\% of children said that their parents gave their earnings to their sibling, and only 5\% of parents said that they took earnings from one of their children because they wanted to equalize outcomes \textit{ex post}. We also find no evidence that parents took earnings from their higher-earning children more often than from their lower-earning children. Parents report that they allowed similar shares of high and low earners to keep their earnings (74\% of high earners and 79\% of low earners), and the difference is not statistically significant (p-value = 0.19). We also do not find any evidence that parents used non-monetary transfers to equalize consumption.\textsuperscript{38}

\textsuperscript{36}In the lower-income experiment there is somewhat less - but still substantial - levels of preferences for equality of opportunities when thinking about other peoples’ children.

\textsuperscript{37}The text for the qualitative survey questions are available online in the Supplementary Materials.

\textsuperscript{38}We asked parents if after the experiment they treated their children differently than usual, by giving them more or less of something than usual (e.g., other money or gifts). Only 9 (8\%) out of the 110 parents we posed this question to replied in the affirmative. (Note that this question was added partway through the survey and so only 110 parents were asked the question.) We posed a similar question to children, and
The finding that parents in our experiment do not redistribute to equalize consumption is consistent with many studies that find that people think it is fair for consumption to vary with differences in effort and/or talent (see Cappelen et al., 2020 for a review). It is also consistent with the majority of parents in our sample agreeing with the statement “the fairest way of distributing wealth and income in society would be to allow everyone to keep what they have earned, as long as everyone has equal opportunities” (App. Figure A.6). The finding also suggests that a plan to redistribute earnings \textit{ex post} does not explain the absence of outcomes equalization upfront in the lower-income experiment.

4.5 Robustness Checks

We now summarize evidence against possible confounds or alternative explanations for our primary experimental results.

Lack of Understanding. If parents did not understand the experiment, it could potentially influence our results. For example, if parents did not understand how to maximize returns, they might perhaps choose to split their tickets equally because it is easier. Reassuringly, as explained in section 3.4, most parents in both experiments did in fact understand the setup. Our design decision to tell people how to accomplish various goals (e.g., how to maximize returns) also helps mitigate this concern. Here, we present evidence that the small subset of parents who may not have fully understood or been fully attentive does not drive our results. Appendix Table A.7 presents estimates of the mixed logit specification and the raw percentage of scenarios equalized within various subsamples of parents who correctly answered understanding questions and/or attention checks. Our main findings are qualitatively robust to the sample of estimation. While the magnitudes of the logit coefficients vary somewhat across specifications, if anything, our estimates suggest that parents who correctly understood various questions place a \textit{higher} value on equality of opportunity and equality of outcomes (columns 3 onward). This suggests that a lack of understanding does not explain why people appear to value equality in the experiment.

The consistency of our experimental results with the survey responses presented in Section 4.3 also suggests that the experimental results capture parents’ true preferences.

Explanations to Parents. Our experiment explained to parents how to allocate their tickets in order to accomplish four different goals: maximize returns, equalize outcomes, equalize only 10% indicated that parents gave them more or less money or gifts than usual after the experiment.
opportunities, or guarantee that one child got the tutoring. While we did this to ensure that parents understood the experiment and that departures from returns-maximization represented true choices as opposed to lack of understanding, it does raise a potential concern that perhaps parents had some other goal in mind that they would have tried to satisfy without our explanations. In Appendix B.3, we present evidence that this is not the case. The evidence comes from two sources: our piloting, and a different lottery in which parents were dividing lottery tickets across their children for an educational prize (in that case, a scholarship to secondary school). In that lottery, no explanations were given to parents about how to allocate tickets to achieve different goals, but the high-level takeaway is consistent with what we find here.

**Alternative Explanations for Equalization of Opportunities.** In Appendix B.1, we consider whether any other forces besides aversion to unequal opportunity are responsible for the equal ticket allocations we observe in the data. We conclude that none are.

First, we show that *belief uncertainty* is unlikely to drive equal allocations through a combination of heterogeneity analysis based on uncertainty measures, and by directly asking parents whether they would change their allocations if they had greater certainty. We also demonstrate why *indifference* is an unlikely explanation for equal splitting. Third, we provide evidence that equal ticket allocations are not driven by an attempt to *balance opposing desires* for returns maximization, equality of outcomes, and/or child-specific preferences, since parents equalize nearly as often when the predictions of those other preferences align as when they differ.

**Absence of Outcomes Equalization in the Lower-Income Experiment.** We find that, although many parents in the higher-income experiment care about equalizing outcomes, parents in the lower-income experiment largely do not. The fact that parents in the higher-income experiment state a stronger preference for equalizing outcomes in survey questions than parents in the lower-income experiment (e.g., Figure 11) suggests that this difference reflects true heterogeneity in preferences, as does the fact that, within the higher-income experiment sample, higher-income parents care more about equalizing outcomes than lower-income parents.

Appendix B.2 discusses several potential alternative explanations (aside from parents simply not valuing equality of outcomes) that could explain the limited preference for equality
of outcomes in the lower-income experiment, and concludes that none do. Of note, the result is not caused by parents in the lower-income experiment reallocating earnings after the experiment, nor by them equalizing outcomes across scenarios, nor by the fact that they often could not perfectly equalize within scenarios.

**Demand Effects.** To try to mitigate demand effects (response bias), we only gave participants a relatively vague description regarding the purpose of our study.39 However, there is still a potential concern that parents inferred that we wanted them to equalize. We believe this concern is not responsible for the effects we see for two main reasons. First, our lower-income experiment uses high stakes, which is the traditional approach to address demand effects. The fact that we estimate substantial preferences for equalization even when the stakes are high suggests that demand effects are not responsible. Second, recent evidence suggests that, in general, demand effects are not large in these types of experiments. Mummolo and Petersen (2018) finds that demand effects are relatively minimal in hypothetical survey experiments on online platforms like mTurk or Prolific,40 and de Quidt et al. (2018) provides evidence that demand effects are modest with both incentivized and hypothetical choices.

5 Conclusion

This paper shows that parents value equality of opportunity when distributing opportunities across their children. To identify this preference, we measured how parents allocate probabilistic (i.e., lottery ticket) opportunities, which generate clean predictions and allows us to sidestep uncertainty as a potential confound to the identification of preferences for equality. To identify the degree to which parents care about (a) maximizing total returns, (b) creating equality of opportunity, and (c) creating equality of outcomes or earnings, we experimentally shocked the (real or hypothetical) short-run returns to the opportunities.

We find that parents care about both maximizing returns and equalizing opportunity. The value they place on equality of opportunity is quantitatively important, and they are willing to forgo an average of roughly 15-50% of their potential experimental earnings to equalize opportunity. In contrast, our findings regarding the value placed on equality of

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39 For example, in the lower-income experiment, we said we were “conducting a study about how parents allocate resources towards their children’s education.”

40 For example, they randomly assign participants information about experimenter intent and show that providing this information does not alter the treatment effects.
outcomes or earnings are mixed. We find evidence that parents in the higher-income experiment care about equalizing outcomes but find no evidence that parents in the lower-income experiment do. We provide suggestive evidence that the reason for this discrepancy is that the preference for equality in outcomes increases as household income rises.

Our findings open up new directions for future work. First, because the effects of policies depend on individuals’ behavioral responses, our estimates have important implications for policy design. If a government is distributing opportunities across households, parents’ substantial aversion to unequal opportunity means that the decision of whether to distribute the opportunities equally or unequally within households could be very consequential. However, the direction is theoretically ambiguous. On the one hand, if parents are resource-constrained and unable to increase the resources to the non-targeted child, they may respond to unequal distribution by refusing to take up the opportunity for the targeted child. In this case, the government may prefer to distribute the opportunity equally within households to ensure high take-up. Indeed, we asked parents in the higher-income experiment whether they would accept a scholarship to a high-quality private high school, or a seat in an in-person school, if only one of their children could have it. For each question, over 35% of parents said they would turn the resource down because of fairness concerns. This suggests that households, especially poorer ones, may be leaving important opportunities on the table due to their equality preferences. On the other hand, if parents are not resource-constrained, they may respond to policies that deliver opportunities to one child in the household by providing more opportunities to non-targeted children to try to mitigate the inequality. In that case, if governments want to crowd-in household spending on education, they may wish to target opportunities unequally across children in households, instead of equally.

A second direction is to determine the bracket within which people normally equalize. Do they equalize within short or long time horizons? Within their own investments or across all investments, inclusive of government investments? Within specific investment domains (e.g., health vs. education) or across all investments? In our experiment, parents narrowly bracket, and Exley and Kessler (2019) suggest that narrow bracketing of equity concerns is a widespread phenomenon. The more narrowly people bracket outside of experimental settings, the higher the efficiency cost of equalizing opportunities is likely to be.

A third direction is to understand the channels behind the preference heterogeneity we observe between our lower-income and higher-income samples, and to rigorously test our
suggestive evidence that household income itself drives the heterogeneity.

A final area for future work is to examine the extent to which the preferences we identify here over parental investments with short-run returns extend to parental investments with larger, longer-term returns, and to investments made by the government.\(^{41}\) Our survey results suggest the preferences are similar but it would be useful to provide experimental evidence on those preferences as well. The estimates could then be important guides for policymakers in designing policy that honors citizen preferences.

References


\(^{41}\)One specific issue to investigate is whether parents who are averse to inequality in *expected* opportunities also prefer to equalize actual opportunities when opportunities are divisible. Another issue to explore is how the cost of choosing unequal opportunities scales with the stakes of the decision. If the cost is fixed, then a preference for equality would be more important for small-stakes than large-stakes decisions. In contrast, if the cost scales in proportion with the returns, then equality preferences would be important for large-stakes decisions as well. In that case, our structural estimates imply that parents are willing to give up 15-50% of total investment returns to equalize, a huge cost for large-stakes decisions.


Appendix A.1: Consistent with Child-Specific Preferences, Parents Who Give More Tickets to Child H (L) in Scenario 1 Continue to Allocate More to Child H (L) in Scenarios 2-5

Notes: This figure presents allocation of lottery tickets across children for Scenarios 2-5 in both experiments. The panels present allocations separately for parents who gave strictly more tickets to Child H in Scenario 1 (“Prefer High”), and those who gave strictly more tickets to Child L (“Prefer Low”) in Scenario 1. The proportion of respondents in the “Prefer High” group was 0.25 in the lower-income experiment and 0.01 in the higher-income experiment. For the “Prefer Low” group the proportions were 0.13 and 0.41 respectively.
Appendix Figure A.2: Distribution of Parents’ Preferences for Equalizing Opportunities

Notes: Panels (a)-(b) present the distribution of the coefficient on “Opportunities (tickets) not equally split” and Panels (c)-(d) present the distribution of the coefficient on “Absolute difference in outcomes” from Column 3 of Table 2 separately for the lower- and higher-income experiments. We use an Epanechnikov kernel function. Recall that the prediction of equality of opportunity and equality of outcomes is that the coefficients would be negative. 80% of lower- and 62% of higher-income experiment participants have a coefficient on tickets not equally split less than 0. 40% of lower- and 64% of higher-income experiment participants have a coefficient on absolute difference in outcomes less than 0.
Appendix Figure A.3: Parents State Similar Preferences Across Various Educational Investment Circumstances

Preferences over long-run (i.e. to increase future wages) and short-run (i.e. to increase test scores) investments

Notes: Parents were asked about their preferences over different investment circumstances. Panels (a)-(b) display preferences over investments to increase their children’s test scores (short-run) and future wages (long-run). In both experiments, half the sample was randomly asked about the short-run and the other half asked about the long-run. Panels (c)-(d) display preferences in investments in education they would make for their own children, for other peoples’ children, and that they would want the government to make in children.

Appendix Figure A.4: Parents in the Higher-income Experiment Consistently Express a Higher Preference to Equalize Outcomes When Distributing Educational Investments

Notes: These figures display preferences for equalizing outcomes (Panel (a)) and opportunities (Panel (b)) in investments made for children’s education. The excluded option is to maximize returns. “Short-run” and “long-run” refer to investments to increase children’s test scores and future earnings, respectively. “Own kids” refers to investment in one’s own children and “other kids” refers to investment in other children. “Gov investment” refers to investment made by the government.
Appendix Figure A.5: Preferences Over Government Investment

(a) Lower-income experiment
(b) Higher-income experiment

Notes: This figure displays preferences over investments made by the government in education and in small business in both experiments.

Appendix Figure A.6: Preferences Over Distributing Initial Opportunities Versus Redistributing Wealth

(a) Lower-income experiment
(b) Higher-income experiment

Notes: For the “Wealth in society” question, parents were asked to choose which of the following two statements they agree with more: A. The fairest way of distributing wealth and income in society would be to give everyone an equal amount, or, B. The fairest way of distributing wealth and income in society would be to allow everyone to keep what they have earned, as long as everyone has equal opportunities. We label choice A “Equal Outcomes” and choice B “Equal Opportunities.” For the “Kids’ test scores” question, parents were given a scenario where one child had higher ability and higher returns to test scores, and asked them to choose whether they wished to spend the “same amount on each child to give both children the same opportunities” or to “spend more on child W (the lower-ability child) to help them achieve as high a test score as their sibling.” We label the first answer “Equal Outcomes” and the latter “Equal Opportunities.”
Appendix Table A.1: The Majority of Parents Answered Each Understanding Question Correctly

<table>
<thead>
<tr>
<th>Experiment understanding questions</th>
<th>Lower-income (1)</th>
<th>Higher-income (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Understood ticket allocation (one question)</td>
<td>0.95 (0.21)</td>
<td>0.93 (0.25)</td>
</tr>
<tr>
<td>2. Understood ticket allocation (both questions)</td>
<td>0.95 (0.21)</td>
<td>0.85 (0.36)</td>
</tr>
<tr>
<td>3. Understood returns maximization</td>
<td>0.84 (0.37)</td>
<td>-</td>
</tr>
<tr>
<td>4. Understood outcome equalization</td>
<td>0.84 (0.37)</td>
<td>-</td>
</tr>
<tr>
<td>5. Understood opportunity equalization</td>
<td>0.98 (0.13)</td>
<td>-</td>
</tr>
<tr>
<td>6. Attention check: what will your child receive if they win the lottery?</td>
<td>-</td>
<td>0.82 (0.38)</td>
</tr>
<tr>
<td>7. Attention check: what type of test is being administered?</td>
<td>-</td>
<td>0.90 (0.31)</td>
</tr>
<tr>
<td>8. Attention check: strictly dominant money game</td>
<td>-</td>
<td>0.94 (0.24)</td>
</tr>
</tbody>
</table>

Total Households: 63 285

Notes: Questions 1-5 were only asked to 63 parents in the lower-income experiment because they were added after surveying had commenced. Questions 3-5 were only asked in the lower-income experiment while questions 6-8 were only asked in the higher-income experiment. In question 1, parents were asked whether they understood how to definitely allocate the tutoring to one of their specific children; in question 2, they were asked whether they understood how to allocate the tutoring to the other child. In questions 3-5 parents were asked if they understood how to allocate tickets in order to maximize returns, equalize outcomes, or equalize opportunities. Questions 6-8 were simple questions designed to gauge whether participants were paying attention, as is standard when implementing online experiments. For example, question 8 was added after the lottery questions in a part of the survey where we asked parents a hypothetical question about how they would split money between two people whom they did not know. In one of these scenarios (there were 5), one choice implied that both people were getting strictly more money than the other choice.

Appendix Table A.2: Parents’ Stated Reasons Predict Their Actual Choices

<table>
<thead>
<tr>
<th>Equalize Opportunities</th>
<th>Maximize Returns</th>
<th>Equalize Outcomes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Prefer equalizing opportunities</td>
<td>0.79*** (0.05)</td>
<td>-0.14*** (0.04)</td>
</tr>
<tr>
<td>Prefer maximizing returns</td>
<td>-0.13*** (0.05)</td>
<td>0.36*** (0.06)</td>
</tr>
<tr>
<td>Prefer equalizing outcomes</td>
<td>0.08 (0.09)</td>
<td>-0.03 (0.06)</td>
</tr>
<tr>
<td>Prefer Child L</td>
<td>0.02 (0.05)</td>
<td>-0.02 (0.05)</td>
</tr>
<tr>
<td>Prefer Child H</td>
<td>0.02 (0.05)</td>
<td>-0.02 (0.05)</td>
</tr>
<tr>
<td>Scenario FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Household FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>1445</td>
<td>1445</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.886</td>
<td>0.468</td>
</tr>
</tbody>
</table>

Notes: This table displays data from the lower-income experiment only. (We did not collect similar data in the higher-income experiment.) The dependent variables represent dummies for the actual choices parents made in a given scenario. The independent variables represent the parents’ stated reasons for making those choices. These reasons were not collected in the higher-income experiment. Standard errors are in parentheses, clustered at the household level. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.
### Appendix Table A.3: Parents Equalized Opportunities in All Scenarios

<table>
<thead>
<tr>
<th>Scenario</th>
<th>Lower-income</th>
<th>Higher-income</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Base Case</td>
<td>0.42</td>
<td>0.36</td>
</tr>
<tr>
<td>2. Higher Returns to Child H</td>
<td>0.30</td>
<td>0.33</td>
</tr>
<tr>
<td>3. Higher Returns to Child L</td>
<td>0.42</td>
<td>0.28</td>
</tr>
<tr>
<td>4. Lump Sum to Child L</td>
<td>0.36</td>
<td>0.36</td>
</tr>
<tr>
<td>5. Higher Returns to Child L &amp; Lump Sum to Child H</td>
<td>0.33</td>
<td>0.24</td>
</tr>
</tbody>
</table>

Notes: This table reports the proportion of parents in the lower-income (column 1) and higher-income (column 2) experiments who equalized opportunities (i.e. chose the 5-5 ticket allocation) in each scenario.

### Appendix Table A.4: Higher-Income Parents’ Greater Preference for the Lower-Performing Child Is Robust to Conditioning on Child Characteristics

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Child L (0/1)</td>
<td>3.38***</td>
<td>3.37***</td>
<td>3.28***</td>
<td>3.04***</td>
<td>3.38***</td>
<td>3.53***</td>
</tr>
<tr>
<td></td>
<td>(0.27)</td>
<td>(0.28)</td>
<td>(0.29)</td>
<td>(0.27)</td>
<td>(0.27)</td>
<td>(0.32)</td>
</tr>
<tr>
<td>Child L (0/1) × Lower-income experiment (0/1)</td>
<td>-3.96***</td>
<td>-3.95***</td>
<td>-3.83***</td>
<td>-3.72***</td>
<td>-3.96***</td>
<td>-3.70***</td>
</tr>
<tr>
<td></td>
<td>(0.42)</td>
<td>(0.43)</td>
<td>(0.47)</td>
<td>(0.42)</td>
<td>(0.42)</td>
<td>(0.51)</td>
</tr>
<tr>
<td>Grade controls</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Older child controls</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Child-level returns to tutoring controls</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Gender controls</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>5740</td>
<td>5740</td>
<td>5740</td>
<td>5740</td>
<td>5740</td>
<td>5740</td>
</tr>
<tr>
<td>P-val: Child L + Child L × Lower-inc. exp. = 0</td>
<td>0.07</td>
<td>0.07</td>
<td>0.13</td>
<td>0.03</td>
<td>0.07</td>
<td>0.70</td>
</tr>
</tbody>
</table>

Notes: This table pools data from both the lower- and higher-income experiments. Observations are at the child × scenario level. The outcome variable is the number of tickets allocated to the child in each scenario. Specifications controlling for “older child” include the interaction between a dummy for whether the child is older and the lower-income experiment dummy. Specifications controlling for “child-level returns to tutoring” include the interaction between the the expected test score returns to tutoring for that child child in the given scenario lower-income experiment dummy. Specifications controlling for “grade” include the interaction between the lower-income experiment dummy and the school grade of the child. Specifications controlling for “gender” include the interaction between the lower-income experiment dummy a dummy for the child being female. The reported p-value was obtained by testing Child L (0/1) + Child L (0/1) × Lower-income experiment (0/1) = 0. All specifications include scenario and household fixed effects. Standard errors are in parentheses and clustered at the parent-level. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

### Appendix Table A.5: Within the Higher-Income Sample, Richer Parents Care Relatively More about Equality of Outcomes, and Less About Equality of Opportunities

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Coefficient on Absolute Difference in Tickets</td>
<td>0.31***</td>
<td>1.40***</td>
<td>-0.03**</td>
</tr>
<tr>
<td></td>
<td>(0.10)</td>
<td>(0.35)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Observations</td>
<td>275</td>
<td>275</td>
<td>275</td>
</tr>
<tr>
<td>Dependent variable mean</td>
<td>-0.17</td>
<td>-2.08</td>
<td>-0.07</td>
</tr>
</tbody>
</table>

Notes: This table displays data from the higher-income experiment only. (We did not collect household income data in the lower-income experiment.) It presents heterogeneity in the logit preference coefficients by the level of household income, which is measured in units of 100,000 USD. Recall that the prediction of equality preferences is that the logit coefficients are negative. Hence a negative heterogeneity coefficient means that richer parents have stronger preferences for equality (for opportunity or for outcomes) and a positive heterogeneity coefficient means the opposite. All columns control for country fixed effects. Standard errors are in parentheses, clustered at the household level. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.
Appendix Table A.6: After the Lower-income Experiment, Most Children Kept Their Earnings

<table>
<thead>
<tr>
<th>Total Households</th>
<th>289</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total Parents Surveys</td>
<td>259</td>
</tr>
<tr>
<td>Total Children Surveys</td>
<td>392</td>
</tr>
</tbody>
</table>

### A. Parent Survey

<table>
<thead>
<tr>
<th>Children with non-zero earnings</th>
<th>Child L</th>
<th>Child H</th>
<th>High Earner</th>
<th>Low Earner</th>
<th>Equal Earner</th>
</tr>
</thead>
<tbody>
<tr>
<td>Children who kept earnings</td>
<td>135</td>
<td>163</td>
<td>210</td>
<td>84</td>
<td>4</td>
</tr>
<tr>
<td>Children who did not keep earnings</td>
<td>82%</td>
<td>69%</td>
<td>73%</td>
<td>79%</td>
<td>100%</td>
</tr>
</tbody>
</table>

#### Reasons children did not keep earnings

- Parents didn’t trust child with money: 6% 10% 10% 5%
- Parents wanted to equalize outcomes: 4% 2% 2% 5%
- Other reasons: 8% 19% 15% 11%

Treated children differently beyond earnings after experiment: 8%

### B. Child Survey

<table>
<thead>
<tr>
<th>Children Surveyed</th>
<th>Child L</th>
<th>Child H</th>
<th>High Earners</th>
<th>Low Earner</th>
<th>Equal Earner</th>
</tr>
</thead>
<tbody>
<tr>
<td>Children with non-zero earnings</td>
<td>190</td>
<td>202</td>
<td>193</td>
<td>196</td>
<td>3</td>
</tr>
<tr>
<td>Children who kept earnings</td>
<td>125</td>
<td>150</td>
<td>193</td>
<td>79</td>
<td>3</td>
</tr>
<tr>
<td>Children who did not get to keep earnings</td>
<td>75%</td>
<td>77%</td>
<td>74%</td>
<td>81%</td>
<td>100%</td>
</tr>
</tbody>
</table>

#### Reasons children did not keep earnings

- Parents took earnings: 5% 5% 5% 5%
- Parents gave it sibling: 2% 1% 2% 0%
- Other reasons: 18% 16% 19% 14%

Children treated differently beyond earnings after experiment: 7% 13% 9% 13%

#### How children were treated differently

- Parent gave extra money: 6% 5% 5% 6%
- Parent gave sibling extra money: 4% 1% 2% 3%
- Parent gave some other gifts: 1% 0% 1% 0%
- Parent gave sibling some other gifts: 0% 1% 1% 0%
- Parent gave sibling less money: 2% 0% 1% 0%
- Other reasons: 6% 1% 3% 5%

Notes: This table displays data from the lower-income experiment only. It presents parents’ and children’s responses from the follow-up survey where they were asked questions about (i) reallocation of children’s earnings after the experiment and (ii) treatment of children by parents after the experiment on dimensions other than experimental earnings (e.g., other money or gifts). This data is not available in the higher-income experiment because children did not earn real payments.
### Appendix Table A.7: Mixed Logit Robustness Checks

<table>
<thead>
<tr>
<th></th>
<th></th>
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<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>A. Lower-income experiment</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Tickets not equally split (0/1)</td>
<td>-2.976***</td>
<td>-2.729***</td>
<td>-5.438***</td>
<td>-5.269***</td>
<td>-3.446***</td>
<td>-4.749***</td>
<td>-2.801***</td>
<td>-3.479***</td>
</tr>
<tr>
<td></td>
<td>(0.292)</td>
<td>(0.259)</td>
<td>(0.959)</td>
<td>(0.890)</td>
<td>(0.595)</td>
<td>(0.761)</td>
<td>(0.251)</td>
<td>(0.563)</td>
</tr>
<tr>
<td>Absolute difference in outcomes (MWK 100s)</td>
<td>0.011</td>
<td>0.020</td>
<td>-0.016</td>
<td>-0.041</td>
<td>0.139</td>
<td>0.024</td>
<td>0.024</td>
<td>0.161*</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.028)</td>
<td>(0.099)</td>
<td>(0.064)</td>
<td>(0.099)</td>
<td>(0.063)</td>
<td>(0.024)</td>
<td>(0.094)</td>
</tr>
<tr>
<td>Household earnings (MWK 100s)</td>
<td>0.230***</td>
<td>0.217***</td>
<td>0.229**</td>
<td>0.286**</td>
<td>0.113</td>
<td>0.109*</td>
<td>0.221***</td>
<td>0.200**</td>
</tr>
<tr>
<td></td>
<td>(0.045)</td>
<td>(0.039)</td>
<td>(0.100)</td>
<td>(0.121)</td>
<td>(0.100)</td>
<td>(0.060)</td>
<td>(0.044)</td>
<td>(0.100)</td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>15895</td>
<td>14740</td>
<td>3300</td>
<td>2915</td>
<td>2915</td>
<td>3410</td>
<td>15400</td>
<td>2640</td>
</tr>
<tr>
<td><strong>Individuals</strong></td>
<td>289</td>
<td>268</td>
<td>60</td>
<td>53</td>
<td>53</td>
<td>62</td>
<td>280</td>
<td>48</td>
</tr>
<tr>
<td>% Scenarios Equalized</td>
<td>37</td>
<td>35</td>
<td>39</td>
<td>40</td>
<td>40</td>
<td>40</td>
<td>37</td>
<td>40</td>
</tr>
</tbody>
</table>

| **B. Higher-income experiment** |             |                  |                       |                         |                        |                        |                 |               |
| Tickets not equally split (0/1) | -1.945***  | -2.880***        | -2.467***            | -2.580***               | -3.064***              |                        |                 |               |
|                       | (0.280)     | (0.292)          | (0.290)               | (0.278)                 | (0.316)                |                        |                 |               |
| Absolute difference in outcomes (USD) | -0.067***  | -0.038***        | -0.052***            | -0.086***               | -0.104***             |                        |                 |               |
|                       | (0.009)     | (0.007)          | (0.011)               | (0.014)                 | (0.014)                |                        |                 |               |
| Household earnings (USD) | 0.108***    | 0.060***         | 0.134***             | 0.123***                | 0.157***               |                        |                 |               |
|                       | (0.010)     | (0.008)          | (0.014)               | (0.015)                 | (0.017)                |                        |                 |               |
| **N**                | 15675       | 12980            | 12705                 | 12375                   | 10395                  |                        |                 |               |
| **Individuals**      | 285         | 236              | 231                   | 225                     | 189                    |                        |                 |               |
| % Scenarios Equalized | 31          | 33               | 30                    | 28                      | 26                     |                        |                 |               |

Notes: Panel A presents results from the lower-income experiment and Panel B presents results from the higher-income experiment. In both Panel A and Panel B, column 1 includes all parents for reference. Column 2 includes all parents who believed both children’s test scores would increase after tutoring, and column 3 includes parents who correctly understood how to definitely allocate (i.e allocate 10 tickets) tutoring to each child. In Panel A, column 4 includes parents who understood a question about how to allocate tickets to maximize returns, column 5 includes those who understood how to equalize outcomes and column 6 includes those who understood how to equalize opportunities. Column 7 includes parents who allocated their tickets to the larger prize in the placebo lottery (see Figure 4) and column 8 includes parents who were in all of columns 3-8. In Panel B, column 4 includes parents who answered all attention checks correctly and column 5 includes all parents included in both columns 3 and 4.
B Robustness Checks

In this section, we first discuss and provide evidence against a set of possible confounds for our findings regarding the value of equality of opportunity and, in particular, for our finding that parents in the lower-income experiment do not appear to value equality of outcomes. Finally, we provide evidence that the explanations we gave to parents are not responsible for our experimental results.

B.1 Equality of Opportunities

Parents did not choose to equalize opportunities due to beliefs uncertainty. Neoclassical risk aversion through concave utility should not cause splitting in our experiment. However, there could still be behavioral channels through which risk aversion might cause parents to choose split allocations. To address this, we perform a heterogeneity analysis based on baseline measures of uncertainty in parents’ beliefs. We fail to find evidence that more uncertain parents equalize more in either experiment; see Appendix Figure B.1.

We can also provide more direct evidence: After the lower-income experiment, we asked parents whether they would have allocated differently if they were certain about the scores their children would receive with and without tutoring. Only 2 out of 289 parents replied in the affirmative. Thus, uncertainty does not seem to be causing split allocations here.

Indifference does not explain why parents evenly split their tickets. If parents are completely indifferent about which of their children receives tutoring, they could split their tickets evenly, even if they are pure returns-maximizers. However, indifference should create an even distribution on allocations 1/9 through 9/1, whereas there is a clear spike at 5/5. In addition, we find that 19% of lower-income parents and 15% of higher-income parents equate in all scenarios, which should not reflect indifference. If a given parent is indifferent between her children in the base case, then when we offer 10x higher returns per point to Child H (scenario 2) or Child L (scenario 3), the parent should no longer be indifferent, but many parents continue to equalize. Moreover, indifference is a knife’s edge and hence an empirically improbable case.

Parents did not split their opportunities to balance a preference for returns-maximization with other desires (e.g., equality of outcomes). The spike at equal allocation makes it unlikely that parents are splitting in order to balance competing desires, as all desires should be smooth through the equal allocation point. To further rule out the possibility that parents split opportunities in order to balance competing desires, we show that parents still choose a substantial share of split allocations in scenarios where the returns-maximizing and outcomes-equalizing allocations are the same. In the Higher Returns to Child L & Lump Sum to Child H scenario, both returns maximization and equality of outcomes dictate that almost all parents should allocate all tickets to Child L. The fact that Table A.3 shows that 33% of parents in the lower-income experiment and 24% of parents in the higher-income experiment still choose equal opportunities in that scenario provides further evidence that equalizing tickets reflects an aversion to opportunity inequality, not a desire to balance...

---

42 We did not ask this question in the higher-income experiment to keep the survey brief.
Appendix Figure B.1: More Uncertain Parents Did Not Equalize Opportunities More

**Higher-income experiment: Heterogeneity by parents’ baseline measure of uncertainty**

(a) Parents who are uncertain about their beliefs (n = 1507)  
(b) Parents who are certain about their beliefs (n = 1628)  
(c) Difference in means (uncertain - certain) (n = 3135)

**Lower-income experiment: Heterogeneity by parents’ baseline measure of uncertainty**

(d) Parents who are uncertain about their beliefs (n = 671)  
(e) Parents who are certain about their beliefs (n = 1650)  
(f) Difference in means (uncertain - certain) (n = 2321)

**Lower-income experiment: Heterogeneity by whether parents changed beliefs after baseline**

(g) Parents who changed their beliefs (n = 2035)  
(h) Parents who did not change their beliefs (n = 1144)  
(i) Difference in means (changed - did not change) (n = 3179)

**Lower-income experiment: Heterogeneity by difference between parents’ beliefs and actual scores**

(j) Difference between beliefs and actual scores above median (n = 1617)  
(k) Difference between beliefs and actual scores below median (n = 1562)  
(l) Difference in means (Difference in means (above-median - below-median) (n = 3179)

Notes: This figure presents allocation of lottery tickets across children pooled across Scenarios 1-5, separately by beliefs uncertainty. Whiskers represent 95% confidence intervals for each allocation or difference in allocations. We use three measures of uncertainty. Observations are at the parent × scenario level. Panels (a)-(f) use parents’ response to the question “How certain are you about each of these scores?” after they were asked to state their beliefs about their children’s scores during the baseline survey. Panels (a)-(c) present data from the higher-income experiment and panels (d)-(l) present data from the lower-income experiment. Parents had to pick a score between 0 and 10, with 0 indicating ‘no idea’ and 10 indicating ‘very certain’. We define parents who are certain about their beliefs as those who chose a number above the median. Seventy-three percent of parents answered this question in the lower-income experiment, and all parents answered this question in the higher-income experiment. Panels (g)-(l) present data from the lower-income experiment only. Panels (g)-(l) use a variable that takes a value of 1 if parents changed their beliefs between the baseline survey and experimental visit, and 0 if they did not change their beliefs. Panels (j)-(l) use a variable that takes value of 1 if the absolute value of the gap between the parents’ beliefs and their children’s true scores was above the median and 0 if it is below the median.
returns-maximization against a preference for equality of outcomes.

To address the possibility that parents are balancing child-specific preferences with other desires, Appendix Figure B.2 shows that we get similar results in Scenario 5 when excluding parents who appear to have a child-specific preference for Child H. For such parents, child-specific preferences, equality of outcomes, and returns-maximization should all lead them to the same child (Child L), and so splitting should not represent a balance of competing desires. However, those parents still split their tickets equally in a high share of cases.

Appendix Figures B.3(a) and B.3(b) present additional evidence from other scenarios, limiting the sample to people with $R_L < R_H$ in order to have only one ticket allocation prediction per strategy per scenario. The left bar pools all parent × scenario observations where equality of outcomes and returns maximization have different predictions (and thus splitting tickets could represent a balance between the two forces). The right bar shows all parent × scenario observations where inequality aversion in outcomes and returns maximization have the same prediction (and thus splitting tickets would not represent a balance between the two). Parents equalize in nearly as many scenarios in the right subfigures as the left subfigures, suggesting that the vast majority of “splitting” represents a preference for equality of opportunity.43

B.2 Equality of Outcomes

Ex post redistribution does not explain the absence of outcomes equalization in the lower-income experiment. If the potential to redistribute cash rewards ex post was more salient to parents in our lower-income experiment than the (hypothetical) higher-income experiment, this could explain why we see less equalizing of outcomes ex ante in our lower-income experiment. However, the evidence presented in Section 4.4 suggested that parents largely did not redistribute unequal earnings in the lower-income experiments, making this explanation unlikely. Of course, what really matters is the lower-income parents’ perceptions at the time they allocated opportunities about their likelihood of engaging in ex post reallocation, which could be different from the realized level of ex post reallocation. The fact that the realized level of ex post redistribution was very low does suggest, however, that it was not a factor parents were considering upfront, and thus that it does not explain why outcomes were not equalized initially.

Cross-scenario equalization does not explain the absence of outcomes equalization in the lower-income experiment. It is possible that the real stakes lottery in the lower-income experiment made parents realize that it was possible to equalize outcomes across scenarios instead of within. If so, this could contribute to the absence of equality of outcomes we observe within-scenario if parents were able to satisfy their desire for equality in expected outcomes by equalizing across, rather than within, scenario. To test this possibility, we asked parents if they had equalized outcomes across scenarios after we had elicited all ticket allocations in the lower-income experiment. Reassuringly, only 19% of parents said they equalized outcomes across scenarios, and our results look very similar when we exclude these respondents.

43 We find similar results when we include the full sample of households but limit the scenarios to those in which the predictions are uniform across nearly all households (Scenarios 2, 3, and 5) (Appendix Figures B.3(c) and B.3(d)).
Appendix Figure B.2: Even Parents Who Do Not Have a Child-specific Preference for Child H Equalized in Scenario 5 (Higher Returns to Child L & Lump Sum to Child H)

Notes: This figure presents the allocation of experimental lottery tickets across children for Scenario 5, and 95% confidence intervals, after excluding parents who allocated > 5 tickets to Child H in Scenario 1 (that is, those who may have a child-specific preference for Child H) in both experiments.

Appendix Figure B.3: Many Parents Equalized Opportunities Even When Returns Maximization and Equality of Outcomes Had the Same Prediction

Sample: Parents who believed tutoring yields higher test score gains for Child H ($R_L < R_H$)

Notes: This figure shows the percentage of parents who equalized tickets across their children, with the left bars depicting the scenarios where equality of outcomes (EO) and returns maximization (RM) have opposite predictions and the right bars depicting the scenarios where EO and RM have the same prediction. Observations are at the parent × scenario level. Panels (a)-(b) restrict the sample to the 66% of parents who believed tutoring yields higher test score gains for Child H ($R_L < R_H$) and includes observations from scenarios 1-5 since the predictions for EO and RM are the same for everyone in that sample for all scenarios. Panels (c)-(d) includes all parents but only observations from scenarios 2, 3, and 5 since those are the scenarios for which predictions are the same for the full sample. The bracket on each of the right bars depicts the 95% confidence interval for a test that the difference between the bars is 0.
An inability to perfectly equalize outcomes does not explain the absence of outcomes equalization in the lower-income experiment. In many of our scenarios, it was not possible for parents to perfectly equalize expected outcomes. If parents only care about equalizing outcomes when they can perfectly do so (i.e., if their utility term governing equality of outcomes is \(-\beta 1 \{ER(x_1) \neq ER(x_2)\}\)), it could bias us away from finding evidence of equality of outcomes. However, this is unlikely to explain the fact that we do not see outcomes equalization in the lower-income experiment in particular, as the percent of parent \(\times\) scenario instances where parents could perfectly equalize is similarly low in the higher-income experiment (8% in the lower-income and 7% in the higher-income).

To further investigate this idea, Table B.1 presents summary statistics from Scenario 1 (which had the majority of cases in which parents could perfectly equalize outcomes), separately by whether the parent had the option to perfectly equalize experimental earnings. Although the option to equalize perfectly depends on potentially endogenous variation in parents’ beliefs about their children’s scores, we view the analysis as suggestive.

Table B.1 shows that the percentage of choices in which parents minimize outcomes inequality is not significantly different across parent types in the lower-income experiment. In the higher-income experiment, we find that there is a significant difference, however it is actually smaller among parents who had the option to perfectly equalize. Thus, not being able to equalize perfectly does not appear to explain our findings.

B.3 Explanations to Parents

We discuss two pieces of evidence that our explanations to parents about how to allocate their tickets in order to accomplish various goals are not responsible for the effects we see. First, we engaged in extensive qualitative piloting with parents before our lower-income experiment. We asked parents both about how they choose to divide their real-world investments between their children and about what factors, within our experimental paradigm, would affect their decisions of how to allocate the tickets between their children. Across the piloting, there were no other goals parents mentioned besides the four goals we focused on in our explanations: returns-maximization, equality of outcomes, equality of opportunity, and child-specific factors. This suggests there is not some “fifth goal” that parents would have tried to achieve had we not implemented our explanations.

To further address the concern, we bring in data from a lottery with a different set of parents in Malawi who were also asked to choose how to allocate lottery tickets across two of their children. The prize was again an educational investment, in that case a scholarship to secondary school. However, a critical difference from our setting here is that the parents were given no explanations about how to allocate the tickets between their children. Across the piloting, there were no other goals parents mentioned besides the four goals we focused on in our explanations: returns-maximization, equality of outcomes, equality of opportunity, and child-specific factors. This suggests there is not some “fifth goal” that parents would have tried to achieve had we not implemented our explanations.

Because that lottery does not have the same exogenous shocks to returns that we have here, the data from that lottery are not able to distinguish the preferences we investigate in our experiment. However, we can still use the data to provide suggestive evidence that our headline finding from this experiment – that parents have a substantial preference for equal
Appendix Table B.1: We Fail to Find Evidence that Parents Who Can Perfectly Equalize Outcomes Do So More Than Parents Who Cannot

Scenario 1 (Base Case) choices by whether parents could perfectly equalize outcomes

<table>
<thead>
<tr>
<th>Whether Parents’ Outcome-Equalizing Choice Perfectly Equalized Outcomes</th>
<th>Yes</th>
<th>No</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td><strong>A. Lower-income experiment</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Equalized opportunities (% of parents)</td>
<td>0.47</td>
<td>0.39</td>
<td>0.20</td>
</tr>
<tr>
<td></td>
<td>(0.50)</td>
<td>(0.49)</td>
<td></td>
</tr>
<tr>
<td>Returns maximization (% of parents)</td>
<td>0.25</td>
<td>0.21</td>
<td>0.46</td>
</tr>
<tr>
<td></td>
<td>(0.43)</td>
<td>(0.41)</td>
<td></td>
</tr>
<tr>
<td>Equalized outcomes (% of parents)</td>
<td>0.08</td>
<td>0.11</td>
<td>0.29</td>
</tr>
<tr>
<td></td>
<td>(0.27)</td>
<td>(0.32)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>106</td>
<td>183</td>
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</tr>
<tr>
<td><strong>B. Higher-income experiment</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Equalized opportunities (% of parents)</td>
<td>0.44</td>
<td>0.30</td>
<td>0.01</td>
</tr>
<tr>
<td></td>
<td>(0.50)</td>
<td>(0.46)</td>
<td></td>
</tr>
<tr>
<td>Returns maximization (% of parents)</td>
<td>0.02</td>
<td>0.02</td>
<td>0.89</td>
</tr>
<tr>
<td></td>
<td>(0.13)</td>
<td>(0.14)</td>
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<tr>
<td>Equalized outcomes (% of parents)</td>
<td>0.21</td>
<td>0.46</td>
<td>0.00</td>
</tr>
<tr>
<td></td>
<td>(0.41)</td>
<td>(0.50)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>123</td>
<td>162</td>
<td></td>
</tr>
</tbody>
</table>

Notes: This table presents the proportion of parents who equalized opportunities, maximized returns, and attempted to equalize outcomes for Scenario 1, summarized separately in columns 1 and 2 by whether the parent had the option to perfectly equalize outcomes. The P-value reported in column 3 tests for a difference in means between columns 1 and 2. Panel A presents data from the lower-income experiment and Panel B presents data from the higher-income experiment. There were 2 parents in the lower- and 82 parents in the higher-income experiment for which the returns-maximizing and outcomes-equalizing allocations were the same — these parents were categorized in the “equalized outcomes” category.

opportunity – holds in that data as well.

Figure B.4(a) shows the absolute value of the gap in tickets between children from the other lottery. Seventy-five percent of parents chose as equal an allocation as possible, whereas only 12% chose an all-or-nothing allocation. Panel (b) shows the number of tickets that were given to the child perceived as lower-performing. Among those who chose the most equal allocation, 4 times as many chose to allocate 1 more ticket to their higher-performing child than lower-performing child, suggesting that, again, in that setting, parents were not truly indifferent or did not misunderstand the returns.\textsuperscript{44} Rather, it is likely that they were simply averse to splitting inputs unequally.

\textsuperscript{44}In that setting, survey evidence suggests that the vast majority of parents believed the earnings return to the lottery prize would be higher for higher-performing children.
Appendix Figure B.4: Many Parents Also Equalize Inputs As Much As Possible In Another Setting

Notes: Data from the control group from the Dizon-Ross (2019) experiment. Panel A shows the distribution of the absolute gap between the number of tickets allocated to a parents’ two children in a setting where parents were asked to allocate 9 tickets between their two children. Panel B shows the number of tickets allocated to the child the parent perceived was lower-performing child. Here, one out of every 100 households was randomly selected and the child whose name was on the selected ticket received a scholarship for four years of government school fees. In the cases where parents believed both children performed equally, we randomly select which child is designated as the “lower-performing child.”
C Section 2 Proofs

In Section 2, we posit that, if parents do not care about equality (i.e., if $\alpha = 0$ and $\beta = 0$), then parents should only choose all-or-nothing allocations of tickets. In this section, we first show this idea more rigorously when parents have expected-utility preferences. We then explore the robustness of the prediction even when parents have other types of preferences such as prospect theory preferences.

Expected Utility

To see that parents with expected-utility preferences and no equality preferences ($\alpha = 0, \beta = 0$) would choose all-or-nothing allocations, we first rewrite equation (1) in the case where $\alpha = 0, \beta = 0$, and the first term is a nonlinear expected-utility term:

$$U(x_1, x_2) = \lambda [Eu(R_1(x_1)) + Eu(R_2(x_2))] + \gamma(z_1, z_2)(x_1 - x_2) \quad (3)$$

Now define $t_i$ as the fraction of tickets allocated by the parent to child $i$ and rewrite equation (3) as a function of the new choice variable $t_i$:

$$U(t_1, t_2) = \lambda [t_1(Eu(R_1(1)) + Eu(R_2(0))] + t_2[Eu(R_1(0)) + Eu(R_2(1))]$$

$$+ \gamma(z_1, z_2)(t_1 - t_2) \quad (4)$$

The first two returns-maximization terms represent the weighted sum of the parent’s expected utility if child 1 received the binary investment and her expected utility if child 2 received the binary investment, weighted by the probability that each event occurs, $t_1$ and $t_2$. Utility is thus linear in $t_1$ and $t_2$, and hence parents will in general choose “all-or-nothing” corner solutions, setting either $t_1$ or $t_2$ to 0. The only case in which parents with utility function (4) would not choose corner solutions is if they are exactly indifferent between which child receives the tutoring (i.e., $Eu(R_1(1)) + Eu(R_2(0)) + \gamma(z_1, z_2) = Eu(R_1(0)) + Eu(R_2(1)) - \gamma(z_1, z_2)$) which is a knife’s edge case that is unlikely to hold in practice.

Non-Expected-Utility Preferences (e.g., Prospect Theory)

Define $\pi(p)$ as the “probability weighting” function or the actual probability weight a parent places on an event whose true probability of occurrence is $p$. A sufficient condition for the prediction that parents who are not averse to inequality will not choose split allocations to hold is that $\pi(p) + \pi(1-p) \leq \pi(0) + \pi(1)$. Intuitively, this condition means that parents understand that choosing split allocations does not increase their chance of winning the lottery.

Most non-expected-utility preferences that the literature has posited will satisfy this condition. For example, if people prefer certainty (as prospect theory suggests), which means that $\pi(1)$ is particularly large, then the prediction should go through. If people have probability weighting functions that are concave at the bottom and convex at the top (as again suggested by prospect theory) then that is also fine for the prediction as long as the function is rotationally symmetric. In our specific setting, we show that the $\pi(p) + \pi(1-p) \leq \pi(0) + \pi(1)$ condition likely holds by conducting a “placebo lottery” allocation, which suggests that parents likely understand that choosing split allocations does not increase their chances of winning the lottery.
D Additional Detail on Experimental Design

The full experimental scripts and visual aids for both the lower- and higher-income experiments are available in our Supplementary Materials, posted online at
https://faculty.chicagobooth.edu/-/media/faculty/rebecca-dizon-ross/research/bdj_app.pdf

D.1 Implementation and Procedures

D.1.1 Lower-Income Experiment

We recruited roughly 300 parents with two children enrolled in grade 5 through grade 7 in government schools in southern Malawi during December of 2017. Roughly a week prior to the experiment, participating parents completed a baseline survey that elicited demographic information, investments, and attitudes regarding their children’s education. At this time, surveyors also described the math test that the children would take and measured parents’ beliefs about each of their children’s expected test score without tutoring, expected test score gains from tutoring, and the certainty of their beliefs. On the day of the experiment, parents were reminded of their beliefs and given a chance to change their responses if they wished. Surveyors then described the experimental design and conducted the experiment. Both the elicitation of parents’ beliefs and their lottery ticket allocations in the experiment were conducted privately, when children were not present.

Explanation of Design and Understanding Checks

We began with a “placebo” lottery designed to verify whether parents understood how to maximize monetary returns in a lottery environment; for the very few parents who did not understand, we then explained how to do so. Next, we gave parents an overview of the experiment. After describing the test, the payments, and the tutoring, surveyors explained how parents would allocate lottery tickets across their children. To make this explanation concrete, surveyors walked parents through a sample tutoring lottery with fake tickets.

Surveyors then walked parents through two “practice” scenarios. The practice scenarios used different payment functions than those used in the actual experiment but were explained in the same way as the experimental scenarios.

For both the practice scenarios and those used in the experiment, surveyors explained the procedure as follows. They began by describing the payment function for that scenario. They then walked parents through two visual aids, one graphical and one table-based. These displayed the expected payments for each child as well as (in the graphical version) the total expected payments across both children for each ticket allocation the parents could choose. Surveyors drew the graphs based on instructions from their tablets, which used parents’ beliefs and the specific scenario to calculate expected payoffs under each ticket allocation. The graphs clearly displayed the total expected payments as well as child-specific expected payments for each potential allocation (allowing the parent to observe the returns-maximizing and outcomes-inequality-minimizing allocations), but did not display anything about the difference in opportunities. As mentioned earlier, as part of the explanations, surveyors told parents how to allocate their tickets if they wanted to maximize returns, equalize outcomes, equalize opportunities, or guarantee that one child received the tutoring. Finally, before we elicited allocations for the practice scenarios only, we asked parents questions to test their understanding of how to allocate tickets to achieve each of the goals above. No such
questions were asked after the real experimental scenarios.\footnote{We added these questions during the final days of our experiment, leading to a smaller sample.}

After the practice payment function scenarios, surveyors explained to parents that they would now be given 5 real experimental scenarios, and that the actual payment function scenario would be chosen by lottery from the 5 experimental scenarios. As part of the explanation, surveyors conducted a sample “scenario lottery” to concretely illustrate how the strategy method worked in this context.

**The Experiment** After the practice scenarios and explanation of the lottery, parents made their 5 actual experimental lottery ticket allocations. Surveyors explained each scenario following the procedure described above, and parents made their selections.\footnote{To ensure parent understanding, when surveyors explained a new scenario, they would not just explain the payment function but also specify what had changed relative to the payment function from the previous scenario. To keep this explanation uniform and easy for surveyors to master, we did not randomize the order in which scenarios were presented. However, because the payment function parameters do not move monotonically with the order in which they were presented, it is unlikely that order effects drive our results.}

After parents made their ticket allocations, we used the following steps to determine which child in each household would receive tutoring. First, surveyors’ tablets randomly selected one scenario. Second, surveyors assigned the 10 tickets to children based on their parents’ allocation for that scenario. Third, parents blindly selected one of the tickets. For example, if the parent had allocated five tickets to each child for the selected scenario, the surveyor wrote the initials of each child on five out of the ten tickets, and asked the parent to pick a ticket out of a hat to select the winner of the lottery.

We then conducted the real-stakes tutoring, test, and payments. To deliver the cash payments, all the children and the remaining parents gathered in a classroom after the test. Surveyors called each child to the front of the room to receive his or her cash envelope. The amounts were not announced publicly.

**D.1.2 Higher-Income Experiment**

We recruited roughly 300 parents using the online platform Prolific in March - May 2021. Our inclusion criteria were that the parent must have at least two children enrolled in school and that the eldest sibling must be 9-18 years of age.\footnote{In the lower-income experiment, we used a narrower range of ages/grades because we had to implement the tests and so each extra grade included in the sample increased the logistical costs entailed. In the higher-income experiment, we increased the range to be able to ensure we had enough parents in the sample.}\footnote{We instructed parents who had more than one younger child to answer the questions for their two oldest children enrolled in school.}

There are two primary differences between the lower-income and higher-income experiments: the stakes and the mode of delivery. First, the higher-income experiment was hypothetical, not real-stakes. None of the tutoring, test, or payment stages depicted in Figure 1 were conducted in the higher-income experiment. All explanations and scripts made clear that the experiment was hypothetical by, for example, asking parents to imagine that their children were to take a math test and be paid for it. Second, the experiment was conducted online, not in-person. The role of the surveyor from the lower-income experiment was performed almost exactly by detailed “explanation screens”, interactive graphs, tables, and mouseovers.

There are also several smaller differences between experiments. First, to keep the online
experiment short, in the baseline survey, we only collected basic demographic information. The survey was also administered immediately before the experiment (not on separate days). Second, during the explanation of the design, we assessed respondent understanding in different ways. We did not include the “placebo” lottery or “practice” scenarios. Instead, before we elicited ticket allocations for the first scenario, we included five questions that tested respondent understanding of the experimental design (Appendix Table A.1). Two were questions asked in the higher-income experiment during the practice scenarios, and three were “attention check” style questions to test whether participants had been paying attention to the explanations. Third, during the actual experimental scenarios, we did not ask parents their rationales, again to keep the experiment short and focused. Finally, we did not conduct a follow-up survey, and so incorporated the module gathering stated preferences about equality (conducted during the follow-up survey of the lower-income experiment) into the Confounds survey.

D.2 Choosing Payment Function Parameters

The values for parameters $B_L, C_L, B_H$ and $C_H$ differ across the lower-income and higher-income experiments; however, the method used to choose them was consistent. For the multiplicative parameters ($C_L$ and $C_H$) in the lower-income experiment, we chose values that yielded reasonably high-stakes payment functions to have good identification. When designing the higher-income experiment, we then simply scaled values down by a factor of 10 to reflect the fact that payment was made in USD as opposed to MWK. For the lump sum parameters ($B_L$ and $B_H$) relevant for scenarios 4 (Lump Sum to Child L) and 5 (Higher Returns to Child L & Lump Sum to Child H), the values in both experiments were informed by pilot data we collected before the experiments. During piloting, we elicited parent’s beliefs about their children’s test scores and determined their payment thresholds. Using this data, we were able to determine the proportion of parents for whom the outcome-inequality-minimizing prediction was to give all the tickets to Child H in scenario 4 and and all tickets to child Child L in scenario 5. Then, separately for each experiment, we solved for the values of $B_H$ and $B_L$ that made these proportions approximately 1.

D.3 Scenario Order

The experiment presented the scenarios to respondents in the following order:

1. Base Case
2. Lump Sum to Child L
3. Higher Returns to Child H
4. Higher-Returns to Child L & Lump Sum to Child H
5. Higher Returns to Child L

This order was optimized during piloting to facilitate respondent understanding of the payment functions. This order of payment functions changes the lump sum parameter $B$ and the slope parameter $C$ separately before changing them together. For ease of exposition, we changed the order of presentation in the paper.

49Our reasons for excluding these elements were that almost all parents in our lower-income experiment understood the lottery environment; the higher-income experiment was conducted in higher-literacy countries and so we expected understanding to be even higher; and, we wanted to keep the pre-experiment portion short to ensure that parents focused on the actual experimental scenarios.
E  Estimation of the Mixed Logit

E.1 Identification assumptions

We assume that each preference parameter \((\lambda, \alpha, \beta, \gamma)\) is distributed normally across individuals with a standard deviation estimated using the regression model. We also allow for correlations across all preference parameters and again estimate the correlations within the regression procedure. Finally, the error term \(\varepsilon_{ijk}\) is assumed to be Type I extreme value, independent across \(i, j,\) and \(k\).\(^{50}\)

E.2 Control Function Approach

This section describes how we use the control function approach of Petrin and Train (2010) to correct for potential endogeneity in our mixed logit estimation of equation (2).

The two potentially endogenous regressors are \(HouseholdEarnings_{ijk}\) and \(OutcomeInequality_{ijk}\). To isolate the experimental variation in these regressors, we use indicators for the experimental scenario \(\times\) ticket allocation \(\tau_{jk}\) as instruments, thus eliminating the endogenous component of the variation. In the first stage of the estimation, we estimate two regressions, regressing the dependent variables \(HouseholdEarnings_{ijk}\) and \(OutcomeInequality_{ijk}\) on our exogenous instruments (indicators for the scenario \(\times\) ticket allocation, \(\tau_{jk}\)) and on the other endogenous regressors from equation (2). We denote \(\hat{\eta}_{ijk}\) and \(\hat{\mu}_{ijk}\) as the residuals from those regressions. In the second stage, we then include these residuals linearly in the utility function, allowing them to enter with normally-distributed random coefficients \(\rho_i\) and \(\tau_i\):

\[
u_{ijk} = \lambda_i HouseholdEarnings_{ijk} - \alpha_i OpportunityInequality_{ijk} - \beta_i OutcomeInequality_{ijk} + \gamma_i TicketsToChildL_{ijk} + \rho_i \hat{\eta}_{ijk} + \tau_i \hat{\mu}_{ijk} + \varepsilon'_{ijk}
\]

Intuitively, this approach leverages the fact that, conditional on the linear control function \(\rho_i \hat{\eta}_{ijk} + \tau_i \hat{\mu}_{ijk}\) (which captures all of the potentially-endogenous components of \(HouseholdEarnings_{ijk}\) and \(OutcomeInequality_{ijk}\)), \(HouseholdEarnings_{ijk}\) and \(OutcomeInequality_{ijk}\) should no longer be correlated with the unobserved component of the decision \(\varepsilon'_{ijk}\).

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\(^{50}\)Allowing each choice to have a separate logit error may seem strange here given that there is a relatively natural numeric ordering between the allocations. Indeed, if the options were “fully ordered” in the sense that, if a parent ranked her preferences, her first choice would always be adjacent to her second choice in the ordering, then this specification would be very unreasonable. However, although choices take on numeric values here, they are not, in fact, fully-ordered. For example, if a parent’s first choice would be to give all tickets to child 1, it does not mean her second choice is necessarily to give 9 tickets to child 1; her second choice might instead be to split 5/5 because she has a high utility from splitting, or to give all tickets to child 2 because she likes to make all-or-nothing allocations. This means that allowing different choices to have separate logit errors is more plausible here than in settings where the choices are fully-ordered, i.e., where knowing a parent’s first choice means we know her second choice.