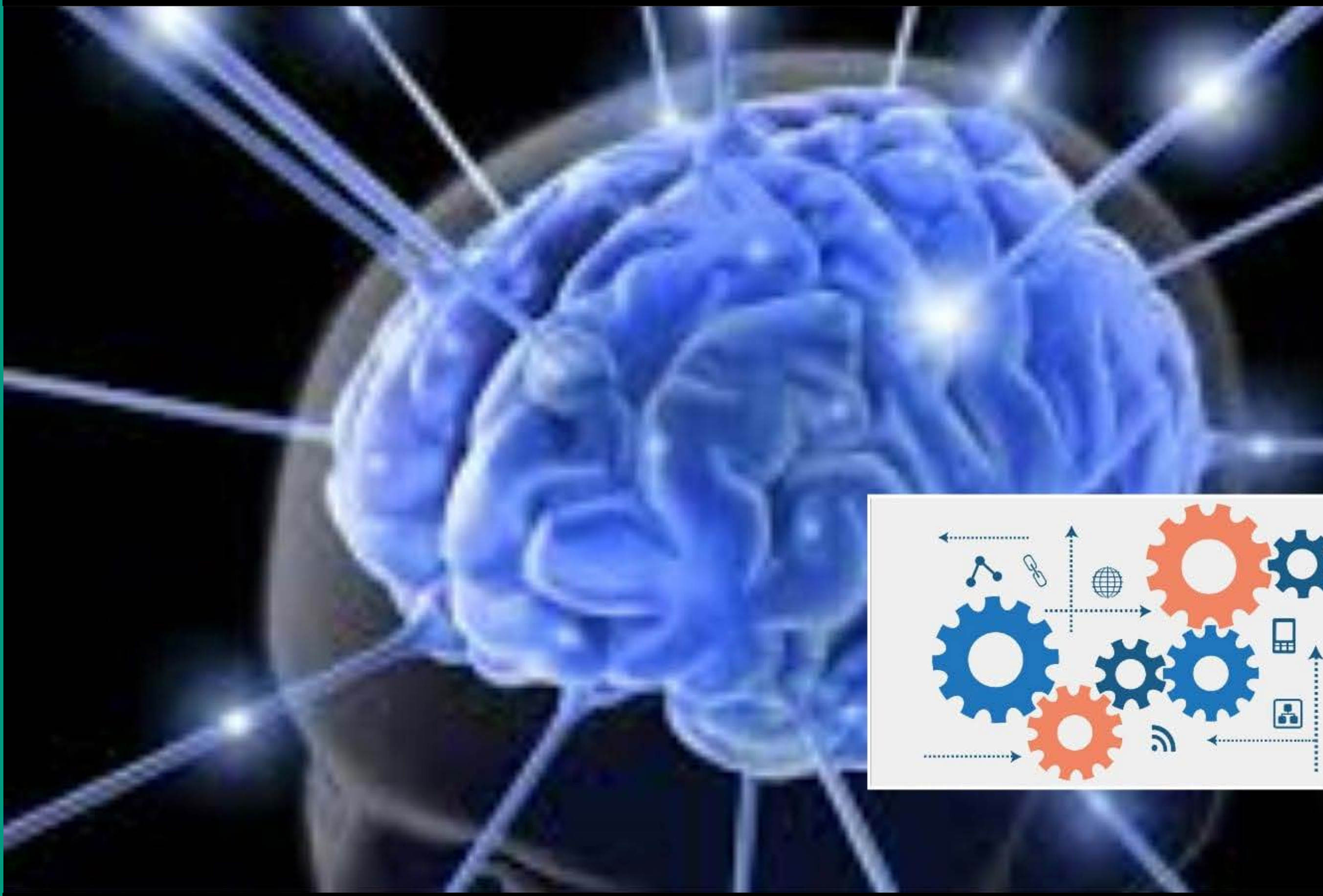
Norwegian University of Life Sciences (NMBU)

# Mental Zooming as Variable Asset Integration in Inter-temporal Choice

Stein T. Holden, Dag Einar Sommervoll and Mesfin Tilahun



Norwegian University of Life Sciences
Centre for Land Tenure Studies

Centre for Land Tenure Studies Working Paper 07/20
ISBN: 978-82-7490-290-9

# Mental Zooming as Variable Asset Integration in Inter-temporal Choice

Stein T. Holden[1], Dag Einar Sommervoll[2], and Mesfin Tilahun[3]

**Abstract**

Our time preferences deviate systematically from that of Homo economicus. They seem to be driven by a form of mental zooming, where higher and more distant payouts induce a more holistic perspective in contrast to smaller and near future payouts. We model zooming as variable asset integration and ask whether this can explain the observed variation in discount rates in experiments. It can. Equally important, the zooming for both time and magnitude is similar across two countries (Ethiopia and Malawi), and within a country (Ethiopia). An intriguing empirical regularity is that the dimensionless degree of zooming in time is roughly twice the zooming degree in magnitude. We offer no explanation for this asymmetry between time and magnitude.

*Keywords:* time discounting, magnitude effects, asset integration, zooming theory
*JEL:* D03, D91, C93

## 1. Introduction

Loewenstein and Prelec (1992) were the first to give a good overview of anomalies in inter-temporal choice. Anomalies are defined to be violations of the discounted utility (DU) model of Samuelson (1937). While Samuelson's ambitions were very modest for this model, it gained widespread popularity as it represented rational inter-temporal choice equivalent to Expected Utility Theory (EUT) in risky decisions. To this day, it serves as a valuable benchmark. The anomalies in inter-temporal choice include hyperbolic discounting (discount rates fall with the length of the time horizon), magnitude effects (small outcomes discounted more than large outcomes), the sign effect (gains are discounted more than losses), preference for improving sequences, and the delay-speedup asymmetry (Loewenstein and Prelec, 1992).

[1]School of Economics and Business, Norwegian University of Life Sciences, stein.holden@nmbu.no.

[2]School of Economics and Business, Norwegian University of Life Sciences and NTNU Trondheim Business School dag.einar.sommervoll@nmbu.no.

[3]Mekelle University and School of Economics and Business, Norwegian University of Life Sciences, mesfin.tilahun.gelaye@gmail.com.

This paper aims to contribute to the literature on hyperbolic discounting and magnitude effects and their possible explanations. Hyperbolic discounting differs from exponential discounting (DU-model) in two ways. It puts higher weight on the present and involves a higher degree of patience for more distant prospects than the DU-model dictates. We refer to the latter as general hyperbolic discounting in contrast to the former, which is impatience in the form of present bias, also called quasi-hyperbolic discounting. First, we assess the extent of population-averaged hyperbolic and magnitude effects in a large new sample from incentivized lab-in-the-field experiments in Ethiopia and compare with an earlier study in Malawi drawing on their original data. We use a unique within-subject 3*3+1 design to separately estimate magnitude, general hyperbolic, and present bias effects at the population level. Next, we test the external validity of the zooming theory of Holden and Quiggin (2017), which was based on the Malawi data, and whether it holds across countries and districts in the Ethiopian sample. Finally, we derive unit-free zooming parameters in time horizon and magnitude and assess their stability across samples.

Different theories have been proposed to explain hyperbolic discounting. First, the most well-known and documented is present bias associated with immediate pleasure, addiction, self-control problems and commitment devices, and liquidity constraints. Present bias is associated with quasi-hyperbolic discounting (Loewenstein and Prelec, 1992; Augenblick et al., 2015; Augenblick and Rabin, 2019; Balakrishnan et al., 2020) - also defined as the $(\beta, \delta)$-formulation and originated from Phelps and Pollak (1968). Second, risk or uncertainty about future payments versus immediate payments is another potential reason for apparent time-inconsistent choices, and that has been studied (Halevy, 2008, 2015; Epper et al., 2011). To control for such differences in risk between immediate and future payments, some studies include delayed up-front points in time, such as introducing a one-week delay. A recent study in Kenya revealed that even very short delays in initial payment eliminated present bias (Balakrishnan et al., 2020). Other studies have provided guarantees related to future payouts. Grijalva et al. (2014) provided such guarantees and found diminishing impatience in a Multiple Price List experiment with time horizons of 5, 10 and up to 20 years into the future. Moreover, a CTB experiment with similar long time horizons and guaranteed future payments (Grijalva et al., 2018) also found diminishing impatience associated with longer time horizons. Their estimated discount rates were an order of magnitude lower than rates found with the CTB approach over much shorter time horizons of 5-14 weeks by Andreoni and Sprenger (2012). These high discount rates was not explained by present bias as Andreoni and Sprenger (2012) found no evidence of present bias. The discount rates gap may be due to general hyperbolic effects but require mores variation in time horizon treatments to be detected. Both studies used relatively small university student samples from universities in the US. It is natural to ask to what extend their results carry over to less select respondent groups. In particular, if general hyperbolic effects are found in broader respondent groups in other parts of the world. Our study provides evidence that they are.

In the seminal paper on magnitude effects (Thaler, 1981), Thaler found that discount rates declined with higher magnitudes. This magnitude effect has, over the years, been confirmed by many researchers, e.g., (Benzion et al., 1989; Green et al., 1997; Kirby and Maraković, 1995). The lion's share of these contributions is in the spirit of Thaler's original paper and relied on the ranking of hypothetical prospects and offered no real payouts. However, magnitude effects are also confirmed in several more recent incentivized experiments. Andersen et al. (2013) is one example. They studied magnitude effects based on incentivized experiments with adult Danes, and found small but statistically significant magnitude effects. However, the variation in magnitude levels was limited in their study, with the largest amounts (DKK 3000[4]) only double the smallest amounts (DKK 1500). Halevy (2015) used magnitude levels of 10 and 100$ with a week delay in a student sample in Canada and found highly significant magnitude effects. Holden and Quiggin (2017) also found highly significant magnitude effects in their rural sample in Malawi, where the largest magnitude levels were up to 20 times larger than the smallest amount.

A much less studied and recognized issue is limited or variable asset integration. Andersen et al. (2008) included constant asset integration with a daily wage rate when estimating discount rates for adult Danes to ensure positive discount rates. Andreoni and Sprenger (2012) estimated asset integration or base consumption integration with a Stone-Geary utility function based on the CTB data and revealed that the estimated discount rates and utility curvature were sensitive to base consumption levels. They suggested that future research should address the issue of asset integration.

There has been more focus on limited asset integration in risk experiments. It may explain small stakes risk aversion (Binswanger, 1981; Wik et al., 2004; Andersen et al., 2018). Variable asset integration in risk may also help explain the Rabin paradox (Rabin, 2000).

Holden and Quiggin (2017) propose varying asset integration as the main explanation for time and magnitude effects in intertemporal choice. Their zooming theory assumes that the degree of asset integration varies systematically with a prospect's magnitude level and time horizon. A more holistic perspective with more asset integration is associated with longer time horizons and larger future amounts. Holden and Quiggin (2017) shows that within-subject variation in time horizon and

[4]About 540 USD with the 2013 exchange rate.

magnitude levels from a field experiment with a sample of adults from Malawi is consistent with their theory. We rely on the same zooming framework and use population-averaged mental zooming theory models. These models allow for variable asset integration where longer time horizons and larger prospect magnitudes are associated with higher degrees of asset integration. Small amounts and a near-future time horizon may, in contrast, have close to zero asset integration.

We consider these time horizon and magnitude treatments as objective factors with reference to Böhm-Bawerk's distinction between objective and subjective factors (Böhm-Bawerk, 1889). Though our analysis rests on the zooming framework of Holden and Quiggin (2017), our study differs in one important way. We do not estimate individual risk aversion to determine individual utility curvature.[5] In contrast, we assume that a log utility function with variable asset integration is appropriate for the estimation of population- averaged discount rates.

Our estimation results demonstrate strong and consistent population-averaged general hyperbolic and magnitude effects in the Ethiopian data as a whole and by district, with discount rates falling with the length of time horizon and magnitude levels of future amounts. Moreover, the estimated zooming parameters are similar across the countries (Ethiopia and Malawi) and Ethiopian districts. It must be stressed that this zooming behavior is *not* present bias in disguise. By including some choice lists comparing present and future amounts, we test for present bias in the zooming models, and present bias remains highly significant both statistically and economically. In other words, our respondents appear to be present biased zoomers, and zooming behavior is the most salient population-averaged characteristic of all the independent population samples.

This paper has four contributions to the literature. First, it tests the external validity of the zooming theory of Holden and Quiggin (2017) using a large new data set from Ethiopia that allows for district-wise testing.[6] Second, it provides evidence of widespread strong general hyperbolic preferences based on incentivized field experiments. Third, it provides evidence of widespread strong magnitude effects in the same data based on the unique within-subject time-horizon times magnitude level treatments. Furthermore, the unit-free zooming parameters in time and magnitude are astonishingly consistent across samples. The zooming parameter in time is about the double in size of that for magnitude. Our mental zooming telescope is therefore adjusting more strongly in time than in money.

[5]The estimation of individual utility curvature relied on the assumption that utility under risk is the same as over time. Some recent studies have questioned this assumption (Abdellaoui et al., 2013; Andreoni and Sprenger, 2012, 2015; Cheung, 2016, 2019).

[6]The Malawian and Ethiopian data set consists of 350 and 978 respondents, respectively. This gives a total of 1328 respondents.

The remainder of the paper is organized as follows. Section 2 gives a description of experimental designs and summary statistics regarding the experiments. In Section 3, we briefly present the zooming theory and its implementation for experimental data at hand. Section 4 provides the results of the base model without zooming as well as the zoom model. Section 5 concludes.

## 2. Experimental Design and Data

The data sets used in the comparative zooming-analysis originate from two different field studies, one in Malawi (2012) and one in Ethiopia (2017). Both rely on a within-subject multiple choice list (MCL) design.[7] Both field experiments were incentivized by the respondents having a 10% chance of winning. [8] A random draw after the completion of all price lists determined whether or not the respondent was a winner or not. The local university guaranteed future payments. Moreover, the respondents had reason to trust the university as it had operated in the study areas for several years and lived up to its obligations.

The field experiments were carried out through interviews by carefully trained experimental enumerators as the respondents were computer illiterate. Classrooms in schools or farm training centers were used for the field experiments. Typically, each corner in the classroom had one enumerator, and one respondent facing the corner. Standardized explanations were translated to the local language to minimize enumerator bias.[9]

Each choice list (CL) had a fixed time horizon and a fixed future amount. Only the near future amount was varied in each list. This design allowed identifying very high discount rates, which is difficult and potentially costly for designs with the near future amount fixed. Future amounts and time horizons varied across CLs in the within-subject design. These future amounts and time horizons also become the within-subject exogenous treatments in our analysis. The order of the CLs, and thereby the within-subject treatments, was randomized for each respondent, and the order was recorded, allowing testing for order bias in the analysis.

We use a rapid elicitation approach to reduce the number of questions needed to

[7] Some authors use the terms multiple price lists (MPLs) and price lists (PLs).

[8] Andersen et al. (2014) tested the effect of paying only a subset of the participants by varying the probability of payment for discounting tasks from 10% to 100% and found that the effect of probabilistic discounting to be insignificant in their sample of adult Danes. In a review of alternative payment regimes Charness et al. (2016) indicated that in most comparisons of paying all or a subset, the loss of motivation is small, much smaller than the implied reduction in actual payment. This finding is also in line with non-linear probability weighting and over-valuation of low probabilities. An obvious benefit of payouts to only a fraction of respondents is the possibility to run more experiments for a given funding. It also reduces the administrative costs of future payments to the spatially dispersed respondents.

[9] In the analysis, we introduce enumerator fixed effects to control for enumerator bias.

identify each CL's switch point. The interviewer starts at a random starting row, and then proceeds either to the top or the bottom of the list. This choice, up or down, is done in the direction that is most likely to lead to a switch (see example list 9 in the Appendix). If a switch is recorded, the enumerator is instructed to go to the middle row between the two and repeat this process until the switch point is identified.[10] Some respondents preferred the near future amount even for the bottom row in the list with the smallest near future amount. In such cases, an additional row was added at the bottom with the near future amount reduced to extend the CL. This procedure was repeated until the switch point was reached.

We first describe the original design for the Malawi field study. Afterward, we briefly describe the larger Ethiopian field study which had a more narrow set of treatments. From the Malawi study we retained only the treatments that were identical to those in the Ethiopian experiment to facilitate comparison.

### *2.1. The Malawian Experimental Design and Implementation*

The Malawian design included three front end (near future) timing treatments, four back end (far future) timing treatments, and four far future amount (magnitude) treatments (3x4x4 factorial design but some combinations were excluded, retaining 27 combinations out of 48).[11] The front end or near future timing treatments included present, one-week into the future and one-month into the future timing. The far future timing treatments included 1-month, 3-months, 6-months, and 12-months from the present time.[12] The magnitude levels, which were fixed for the endpoints, were MK 1,000, MK 5,000, MK 10,000, and MK 20,000.[13] The largest far future amount is, therefore 20 times the smallest far future amount. The smallest amount represents 3.3 times the daily wage rate.

The Malawi experiment had a broader scope than the Ethiopian, and in this paper, we only consider treatments that are directly comparable with the Ethiopian ones (17 out of 27 treatments). The treatment variations included in our comparative study for both countries are presented in Table 1. The numbers in parentheses in Table 1 indicate the number of repetitions of each treatment level in the total set of treatments. The treatments were randomized across households. Each respondent

[10] This approach is also likely to reduce bias towards the middle. However, the randomly chosen starting point may lead to bias if the respondent makes an erroneous choice. We test for such potential bias.

[11] In all experiments, subjects have to choose between $M_A$ at $t_A$ and $M_B$ at $t_B$, where $t_A < t_B$. A front end timing treatment means that $t_A$ varies and $t_B$ remains fixed. Likewise, an endpoint timing treatment means that $t_B$ varies while $t_A$ remains fixed.

[12] To keep things simple for the respondents, we did not adjust the far future time horizons for the near future time delay but adjusted the one week difference in time horizon, e.g., 9 months - 1 week, in the mathematical calculation of discount rates during estimation.

[13] MK is Malawian Kwacha.

was confronted with 9 of the 27 series. The randomization was done in such a way that all 27 series were randomized across three respondents within a village. In this comparative study, as only 17 of 27 treatments are used, we have some variation in the number of CLs per respondent. We refer to Holden and Quiggin (2017) for further information about the Malawian experiments and data.

*2.2. The Ethiopian Experimental Design and Implementation*

The treatments used included two front end timing treatments (present time and one-week delay), three endpoint timing treatments (3, 6, and 12 months), and three endpoint magnitude treatments (100, 500, and 1000 ETB), in a 3*3+1 design. There was only one treatment with no front-end delay that included the lowest amount (100 ETB) and the longest time horizon (12 months) to test the importance of present bias. Table 1 summarizes the experimental design.

For delayed payouts, a guarantee was given by the local university (Mekelle University), and a reward card was given to the winners of future amounts, stating the time and amount to be paid out. One of the coauthors was in charge of the fieldwork and arranged all payouts through the local credit provider (DECSI). Table 9 in the Appendix gives an example of a CL. Furthermore, Table 8 in the Appendix gives an overview of the treatments in the whole MCL.

Table 1: Treatments in the Malawian and Ethiopian Experiments.

| Treatment type | Treatment levels |
|---|---|
| Malawi: | |
| Front end point in time | Current (5), 1 week delay (12) |
| Endpoint in time | 3 months (8), 6 months (5), 12 months (4) |
| Future amount level | 1 KMK (4), 5 KMK (5), 10 KMK (8) |
| Ethiopia: | |
| Front end point in time | Current (1), 1 week delay (9) |
| Endpoint in time | 3 months (3), 6 months (3), 12 months (4) |
| Future amount level | 100 ETB (4), 500 ETB (3), 1000 ETB (3) |

Note: ETB = Ethiopean Birr. KMK= Thousand Malawian Kwacha. In the analysis, we excluded the CLs with 20 KMK and the CLs with 1 month delay, as the Ethiopian experiments did not have corresponding CLs. The number of treatments in each treatment level in parenthesis.

*2.3. Magnitude levels in the Malawian and Ethiopian experiments*

In order to compare mental zooming through asset integration across countries, we need to measure time and magnitudes comparably. The universal measurement of time in days, months, and years are natural for comparison across the two countries. Payouts do not have the same widely recognized scale of measurement. We adopt the much used local daily wage as a unit for comparison. Table 2 gives the conversion to daily wage units for the CLs used in the subsequent analysis.

Table 2: A comparison of the Malawian and Ethiopian Experiments

| Ethiopia | | Malawi | |
|---|---|---|---|
| Amount Ethiopian Birr | Daily wage units | Amount Malawian Kwacha | Daily wage units |
| 1000 | 33.3 | 10000 | 33.3 |
| 500 | 16.7 | 5000 | 16.7 |
| 100 | 3.33 | 1000 | 3.33 |

Note: Calculations based on a daily wage rate of 300 MK in Malawi in 2012 and 30 ETB in Ethiopia in 2017.

Table 2 displays the closely matched magnitude treatment levels across the two countries (measured by the number of daily wage units).[14]

## 3. The Zooming Theory in Brief

The zooming theory's fundamental idea is that decisions involving longer time horizons and larger amounts get a more holistic assessment and consideration than decisions that involve shorter time horizons and smaller amounts. Therefore, mental zooming involves a higher degree of asset integration for prospects with longer time horizons and larger amounts compared to prospects involving shorter time horizons and smaller amounts. Rather than thinking about asset integration as something that takes place or not, the theory assumes that mental zooming acts through a varying degree of asset integration.[15]

In order to give this theory an algebraic formulation, consider that a respondent faces the choice between two payouts, $M_A$ and $M_B$ at time $t_A$ and $t_B$, respectively. Furthermore, let $t_0 \leq t_A < t_B$, where $t_0$ denotes the present time.

In this case, the respondent must decide between:

$$U_A = e^{-\delta(t_A - t_0)}u(y_1 + M_A) + e^{-\delta(t_B - t_0)}u(y_2) \tag{1}$$

and

$$U_B = e^{-\delta(t_A - t_0)}u(y_1) + e^{-\delta(t_B - t_0)}u(y_2 + M_B) \tag{2}$$

[14] This meant that Malawian CLs that involved 20000 Kwacha, as well as the one month front end and back end delay treatments were excluded from this comparative study.

[15] Holden and Quiggin (2017) proposed this theory and found empirical support for zooming relying on data from Malawi. Moreover, they claimed that zooming accounted for the lion's share of the hyperbolic discounting and magnitude effects present in the data. We follow closely their formulation with some minor notational changes.

where $u(\cdot)$ is the utility function, $\delta$ is the discount rate, and $y_1$ ($y_2$) is the amount (asset or background consumption integration) that the prospect amount is integrated with at time $t_A$ ($t_B$).

We use the daily wage, $y_0 = w_0$, as a starting reference point for the asset integration base consumption level. Moreover, we model zoom adjusted base consumption the following way:

$$y_A = y_B = w_0 f(t_B - t_A, M_B) \tag{3}$$

where $w_0$ is the daily wage rate and $f(t, M)$ is a differentiable function with $\frac{\partial f}{\partial t} > 0$ and $\frac{\partial f}{\partial M} > 0$.

Note that the monotonicity property (in both arguments) ensures a higher degree of asset integration for higher and more distant payouts. Moreover, asset integration is only driven by the highest and most distant prospective payout ($M_B$), which is the future reference amount in each CL. As we assume the same level of asset integration for both alternatives $A$ and $B$ in each CL, we can simplify the notation and rewrite the two utility alternatives Equation 1 and Equation 2 (by using Equation 3):

$$U_A = e^{-\delta(t_A - t_0)} u(w_0 f(t_B - t_A, M_B) + M_A) + e^{-\delta(t_B - t_0)} u(w_0 f(t_B - t_A, M_B)) \tag{4}$$

$$U_B = e^{-\delta(t_A - t_0)} u(w_0 f(t_B - t_A, M_B)) + e^{-\delta(t_B - t_0)} u(w_0 f(t_B - t_A, M_B) + M_B) \tag{5}$$

In the following, we will rely on a log-utility function:[16]

$$U_A = e^{-\delta(t_A - t_0)} \log(w + M_A) + e^{-\delta(t_B - t_0)} \log(w) \tag{6}$$

$$U_B = e^{-\delta(t_A - t_0)} \log(w + M) + e^{-\delta(t_B - t_0)} \log(w + M_B) \tag{7}$$

where $w$ is the asset integration (measured in daily wage rates). Note that this is a CRRA-utility function with $r = 1$.[17]

In the zoom models, we allow the asset integration $w = w_0 f(t_B - t_A, M_B)$ to vary across treatments in the within-subject design but not across individuals within the

[16] We use Equation 3 to write the utilities in a more condensed form.

[17] The family of constant relative risk aversion utility (CRRA) functions, $u(c) = \frac{1}{1-r} c^{1-r}$ has $\ln(c)$ as a limiting case corresponding to $r = 1$. In the analysis we only consider population-averaged zooming, and assume that CRRA=1 is appropriate for population averages.

same sample as we study population-averaged treatment effects.

### *3.1. Model estimation*

To estimate the model parameters, we use the maximal likelihood estimation approach with the Luce error specification (Holt and Laury, 2002).[18] We use the $\mu$-dependent utility differential:

$$\nabla EU = \frac{EU_B^{\frac{1}{\mu}}}{EU_A^{\frac{1}{\mu}} + EU_B^{\frac{1}{\mu}}} \tag{8}$$

This gives rise to the following likelihood function:

$$\begin{aligned} &\ln L(\delta(x_i), \mu(x_i); Choice_{ijk}) = \\ &\sum_i ((ln(\Phi(\nabla U)|Choice_{ijk} = 1) + (ln(\Phi(1 - \nabla U)|Choice_{ijk} = 0)) \end{aligned} \tag{9}$$

where $i$ ranges over respondents, $j$ choice lists, and $k$ over choice list rows. $Choice_{ijk} = 1(-1)$ denotes the choice of alternative A (B), and the $x_i$'s include the CL level treatments and other covariates.[19]

## 4. Analysis

In this section we first estimate base models for the annulized discount rate, where the asset integration is fixed, and continue to estimate zooming models where the asset integration is allowed to vary.

### *4.1. Base models with fixed asset integration*

We start out by considering a base model, where we allow for the annualized discount rate to be dependent on both the magnitude of future amounts and the time

[18]The Luce specification allows for respondents to make mistakes and choose the alternative with the lowest utility. The probability of choosing the lowest utility decreases as the difference in utility between alternatives increases. The mistake probability is parametrized by the parameter $\mu$ in the Luce specification. For a more thorough discussion, see Holt and Laury (2002).

[19]The CL treatments are specified differently across models. Other covariates also vary from model to model, see equation 10. A respondent's probability of picking the alternative with the lowest utility may depend on the enumerator skill, we allow for enumerator bias in the error rate ($\mu$). In other words, the frequency of respondent mistakes depends on enumerator skill. We do not allow for enumerator fixed effects in (log)$\delta$ for the following reason. Enumerator fixed effects tend to give a downward bias of the constant (log)$\delta$ term, as some of this "reference" point annualized discount rate is wrongly attributed to enumerators.The effect varies with the irrelevant default enumerator, which is a smoking gun for the unintended modeling cost of enumerator fixed effects in this class of models. We have neither found papers that use enumerator fixed effects nor found papers that comment on the potential enumerator bias in models of this type. We believe this is due to this challenge of "an arbitrary partition" of the reference point annualized discount rate.

horizon (measured by the time difference between the two points in time for alternative dated amounts, and present bias). The explicit base model for the annualized discount rate is:

$$\log \delta_i = C + \alpha \log W_i + \beta \log T_i + \gamma PB_i + \sum_{j=1}^{10} \zeta_j ST_{ji} \tag{10}$$

where $C$ is a constant, $W_i$ is far future amount measured by the number of daily wages, $T_i$ is the time difference in months between payout $A_i$ and payout $B_i$, $PB_i$ is the present bias dummy, and $ST_{ji}$'s are starting row dummies for the CL in question.

The utility models are of the form given by Equation 6 and Equation 7, and it is a partial asset integration model in the sense that $f(t_B - t_A, M_B) = 1$ in Equation 3, such that the base consumption integrated asset level is $w = w_0$, i.e. one daily wage across all CLs. With this specification eventual hyperbolic and magnitude effects should show up in the parameters on the time, magnitude and present bias treatment variables.

Our prime focus in this paper is the time and magnitude treatment effects in the annualized discount rate. Tables 3 and 4 give the estimation results for Ethiopia and Malawi, respectively, with an increasing number of controls introduced. An important, perhaps surprising finding is that controlling for present bias does leave the log time-difference coefficient virtually unchanged[20]. This implies that we can immediately reject the quasi-hyperbolic model for both countries. It must be noted that present bias also contributed to significantly higher discount rates in both countries in the range of 13 to 16 percentage points higher rates than for CLs with delayed initial points in time. In short, the respondents, in addition to being present biased, show a very strong tendency of having lower discount rates for more distant time horizons.

The additional controls possibly associated with measurement errors had minimal effects on the population-averaged time and magnitude parameters in both countries. The main conclusion is that the estimated time and magnitude effects remain robust to the inclusion of additional controls.[21]

More importantly, the across-country point estimates of the most refined models (model 4) for the time and magnitude coefficients are not significantly different. This is comforting, as it tells us that the population-averaged marginal effects of more distant time horizons or larger future amounts are essentially the same across

[20]-0.588 versus -0.609 for Ethiopia, and -0.694 versus 0.696 for Malawi.

[21]Due to the smaller sample in the Malawi case, the standard errors are considerably higher (roughly 3 times higher), which raises the bar regarding finding statistically different point estimates. We also note that the parameter for the error rate, $\mu$, is similar across countries.

Table 3: The Ethiopian base models. Asset integration: 1 daily wage.[a]

| | *Dependent variable:* $\log \delta$ | | | |
|---|---|---|---|---|
| | Model 1 | Model 2 | Model 3 | Model 4 |
| logdwr | -0.352*** | -0.335*** | -0.334*** | -0.329*** |
| | (0.006) | (0.006) | (0.006) | (0.006) |
| logtimediff | -0.588*** | -0.609*** | -0.610*** | -0.605*** |
| | (0.009) | (0.009) | (0.009) | (0.009) |
| presentdummy | | 0.164*** | 0.163*** | 0.154*** |
| | | (0.015) | (0.015) | (0.015) |
| Constant | 2.147*** | 2.131*** | 2.130*** | 2.136*** |
| | (0.030) | (0.031)) | (0.030) | (0.031) |
| $\mu$ | 0.044*** | 0.044*** | 0.044*** | 0.064*** |
| | (0.001) | (0.001) | (0.001) | (0.006) |
| Starting row FE | No | No | Yes | Yes |
| $\mu$-Enumerator FE | No | No | No | Yes |
| Observations | 109,384 | 109,384 | 109,384 | 109,384 |
| Respondents | 978 | 978 | 978 | 978 |

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[a]logdwr= log daily wage rate units, logtimediff= log time difference between alt. A and alt. B in months.

Table 4: The Malawian base models. Asset integration: 1 daily wage.[a]

| | *Dependent variable:* $\log \delta$ | | | |
|---|---|---|---|---|
| | Model 1 | Model 2 | Model 3 | Model 4 |
| logdwr | -0.392*** | -0.385*** | -0.384*** | -0.388*** |
| | (0.033) | (0.032) | (0.033) | (0.032) |
| logtimediff | -0.694*** | -0.696*** | -0.695*** | -0.692*** |
| | (0.063) | (0.061) | (0.062) | (0.062) |
| presentdummy | | 0.140*** | 0.137*** | 0.127*** |
| | | (0.046) | (0.047) | (0.048) |
| Constant | 2.516*** | 2.437*** | 2.456*** | 2.468*** |
| | (0.133) | (0.138) | (0.140) | (0.148) |
| $\mu$ | 0.061*** | 0.061*** | 0.062*** | 0.058*** |
| | (0.004) | (0.004) | (0.004) | (0.007) |
| Starting row FE | No | No | Yes | Yes |
| $\mu$-Enumerator FE | No | No | No | Yes |
| Observations | 15,214 | 15,214 | 15,214 | 15,214 |
| Respondents | 350 | 350 | 350 | 350 |

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[a]logdwr= log daily wage rate units, logtimediff= log time difference between alt. A and alt. B in months.

the two country samples. However, the constant terms, the starting point of the annualized (log) discount rates are significantly different across countries.[22] Figure 1 illustrates this point for the magnitude and time horizon.

Another way to shed light on the difference across countries is to compare across-country variation to within-country variation. We utilize the much larger sample size for Ethiopia and make separate population-averaged estimates for five district samples. The results are given in Table 5.

Table 5: Base models estimation for Ethiopian districts.[a]

| | *Dependent variable:* $\log\delta$ | | | | |
|---|---|---|---|---|---|
| | Raya Azebo | Degua Tembien | Seharti Samre | Kilite Awlalo | Adwa |
| logdwr | -0.330*** | -0.360*** | -0.326*** | -0.323*** | -0.310*** |
| | (0.012) | (0.014) | (0.018) | (0.018) | (0.012) |
| logtimediff | -0.539*** | -0.575*** | -0.550*** | -0.684*** | -0.632*** |
| | (0.020) | (0.019) | (0.027) | (0.023) | (0.015) |
| presentdummy | 0.150*** | 0.147*** | 0.153*** | 0.137*** | 0.163*** |
| | (0.031) | (0.028) | (0.047) | (0.051) | (0.026) |
| Constant | 1.925*** | 2.190*** | 1.950*** | 2.433*** | 2.141*** |
| | (0.066) | (0.069) | (0.082) | (0.070) | (0.055) |
| $\mu$-Constant | 0.058*** | 0.067*** | 0.059*** | 0.078*** | 0.064*** |
| | (0.012) | (0.013) | (0.020) | (0.016) | (0.012) |
| Observations | 20,983 | 25,861 | 12,688 | 15,043 | 34,809 |
| Respondents | 186 | 233 | 115 | 133 | 311 |

Model 4 specification (Table 3) with present bias, enumerator and start row FE.
Asset integration= 1 daily wage.
Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

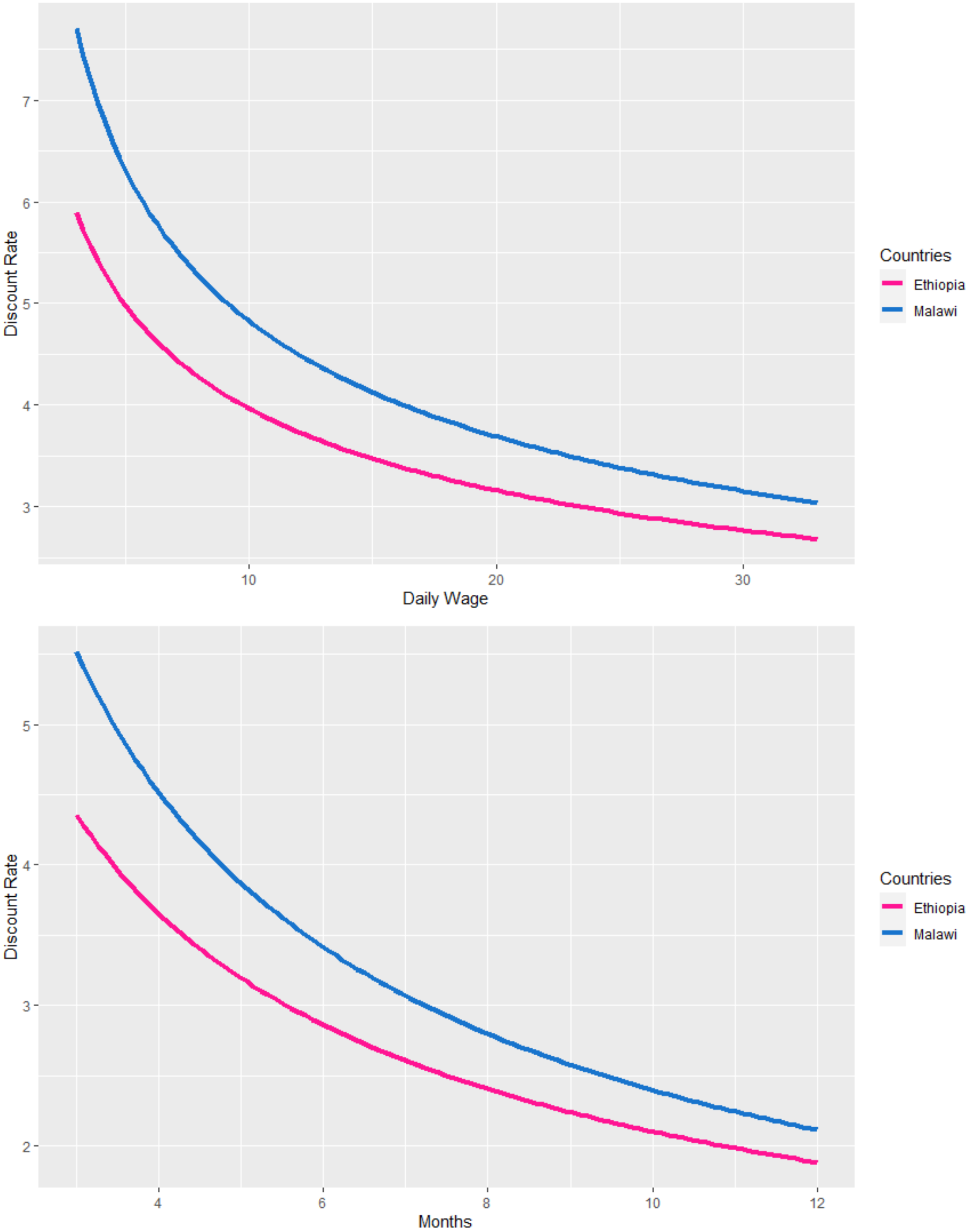
[a]logdwr= log daily wage rate units, logtimediff= log time difference between alt. A and alt. B in months.

We see that there is striking stability across districts for the magnitude coefficient. Only one pair, the lowest (Degua Tembien), and the highest (Adwa) are not within two standard deviations of each other.[23] Malawi has a slightly higher

[22]The point estimate for the Ethiopian constant is 2.136, which is just outside two standard deviations of the Malawian point estimate ($2.468 - 2 \times 0.148 = 2.17$)

[23]It must be noted that the probability of the max draw and min draw being more than two

Figure 1: Discount rate as a function of magnitude and discount rate as a function of time. Ethiopia and Malawi.



coefficient than the district of Degua Tembien, but within two standard deviations of this point estimate.

In the case of the time coefficient, there is a clear and statistically significant division between the three first districts (Raya Azebo, Degua Tembien, Seharti Samre), which are in the $-0.54$ to $-0.59$ range, compared to the two last districts (Kilite Awlalo, Adwa), which are in the $-0.64$ to $-0.69$ range. The latter two districts are on par with Malawi ($-0.69$). For the time horizon coefficient, the cross country variation, therefore, is within the district variation.

We note a significant variation in the constant estimates across Ethiopian districts in Table 5. These represent treatment levels outside the specified treatment ranges (one month time horizon and future amount of one daily wage), which also explain the large size of these coefficients which are measured as 100% units of annualized discount rates. For the districts, the range is 1.9 to 2.43, and Malawi is at the high end of this range (2.45). Malawi resembles Kilite Awlalo for the constant term and timing coefficients, and Degua Tembien for the magnitude coefficients. At the higher level, the coefficients for Malawi are within the district variation of Ethiopia. However, no district is a close match when we look at all three parameter estimates (constant, time, and magnitude) simultaneously.

A third way to illustrate the difference in point estimates of constant, time, and magnitude coefficients is to plot the discount rate as a function of time and magnitude. Figure 2 plots Ethiopian districts together with Malawi.

### *4.2. Zoom models*

In this section, we estimate zoom models for Malawi, Ethiopia, and Ethiopian districts to explore to what extent the zooming appears to be robust across countries and districts.

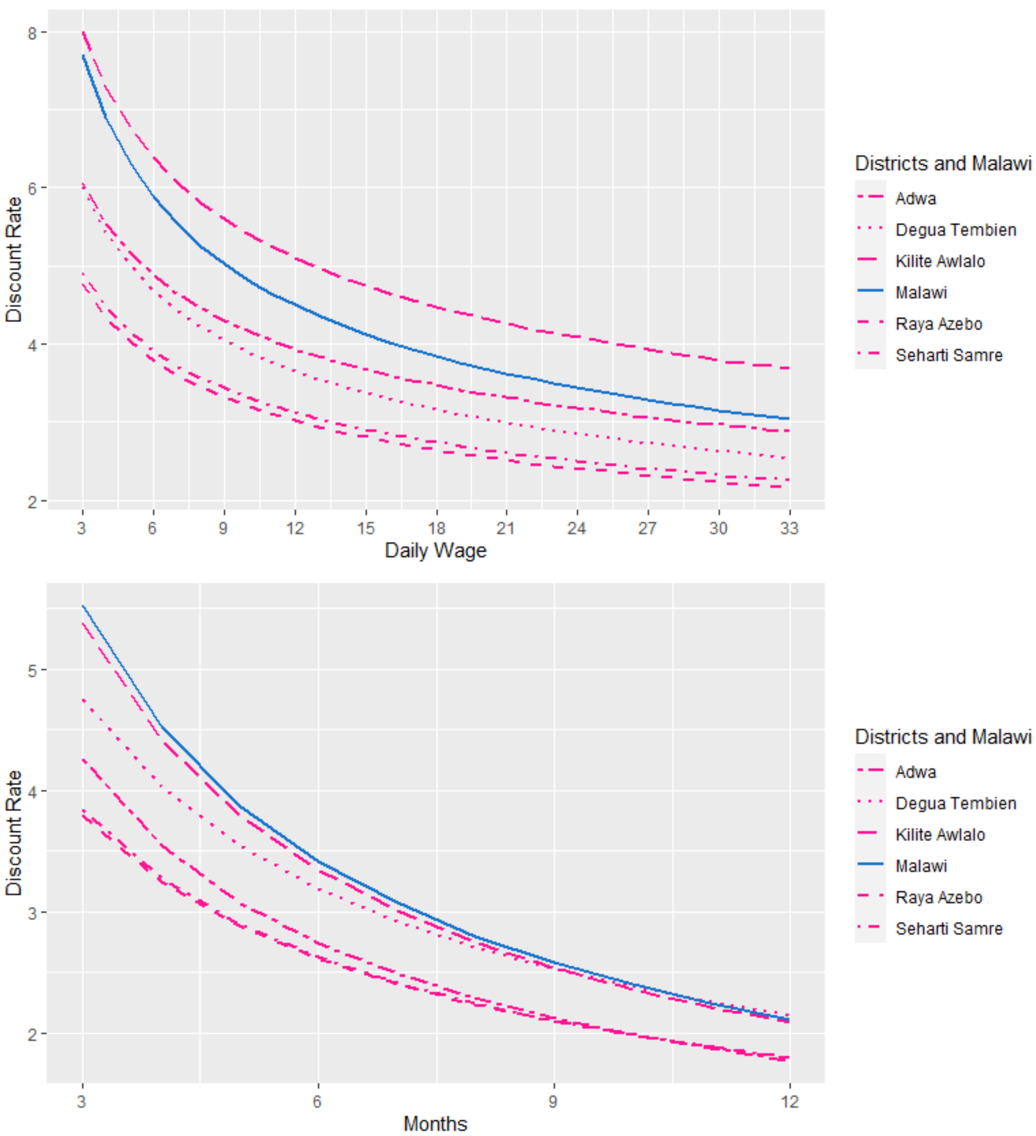
For the base models, we assumed that the degree of asset integration was constant $f(t_A - t_0, M_B) = 1$ (all prospects were combined with one daily wage unit). In the zooming models, we fit a function of the following form:

$$f(t_B - t_A, M_B) = c \cdot (\frac{t_B - t_A}{6})^a (\frac{M_B}{16.7w_0})^b \tag{11}$$

where $t_B - t_A$ is the time difference between the far future and near future points in time for alternative potential payouts, measured in months. $M_B$ is the future amount measured in daily wage units, $w_0$ the daily wage, and where $a$, $b$, and $c$ are parameters to be determined.

standard deviations apart is more than 60 percent in the case of 5 draws from a normal distribution. In other words, we cannot reject the hypothesis that all districts have an equal magnitude coefficient.

Figure 2: Discount rate as a function of magnitude and discount rate as a function of time. Ethiopian districts and Malawi.



The zoom parameters of interest are $a$ and $b$. They represent the marginal time and magnitude zooming coefficients. The parameter $c$, in contrast, is the base point asset integration and is set just high enough to ensure that all discount rates are positive.[24]

Table 6 displays the zoom parameters for the Ethiopia and Malawi models. The zoom parameters for time horizon are similar. One way to get a feeling for the difference is to look at a doubling of the time difference from 6 months to 12 months. The doubling results in a $2^{1.96} = 3.89$ fold increase in the asset level integrated with the prospect in contrast to $2^{1.84} = 3.58$ fold increase for Malawi. The zoom effect of doubling magnitude from the median magnitude ($16.7w_0$) gives a $2^{0.96} = 1.95$ fold increase in the integrated asset level for Ethiopia. The corresponding number for Malawi is 1.59. In other words, there is some across-country variation in the time and magnitude zooming parameters. More striking is the difference between the zooming degree for time and magnitude. A doubling of the time horizon leads to close to a four-fold increase in the level of asset integration. In contrast, a doubling of the amount has only half the effect, a (close to) two-fold effect on the level of asset integration. This indicates that time effects (hyperbolic) are relatively stronger than magnitude effects. This comparison is dimensionless and hence independent of how time and money are measured. It is an intriguing possibility that this asymmetry between time and magnitude is a robust characteristic of human nature just like loss aversion is.

The zooming models by district in Ethiopia are presented in Table 7. The estimates of the zoom parameters appear to be robust across districts. All the district-wise time horizon zoom parameters are in the $2 \pm 0.25$ interval. Likewise, the district-wise magnitude zoom parameters are all in the $1 \pm 0.1$ interval. This implies that the district-wise spread is around 10 percent for time horizon and magnitude. Moreover, the asymmetry mentioned above between time and magnitude also applies to the district zooming estimates.

[24]The base (point) asset integration corresponds to $t_B - t_A = 6$ (months) and $M_B = 16.7$ (daily wages). The model's actual fitting relies on an iterative procedure, where the stopping criterion is when time and magnitude coefficients get close to zero (and insignificant), and $c$ is high enough to ensure positive discount rates.

Table 6: Zoom model estimation by country with present bias FE and start row FE. [a]

| *Dependent variable:* $\log \delta$ | | |
|---|---|---|
| | Ethiopia | Malawi |
| Zoom parameters: | | |
| a | 1.96 | 1.84 |
| b | 0.96 | 0.67 |
| Base asset parameter: | | |
| c | 1 | 3 |
| logdwr | 0.000 | -0.001 |
| | (0.007) | (0.029) |
| logtimediff | 0.001 | -0.000 |
| | (0.009) | (0.071) |
| presentdummy | 0.138*** | 0.106** |
| | (0.015) | (0.041) |
| Constant | 0.190*** | 0.074 |
| | (0.029) | (0.144) |
| $\mu$-const | 0.080*** | 0.081*** |
| | (0.008) | (0.011) |
| Observations | 109,384 | 15,214 |
| Respondents | 978 | 350 |

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[a] logdwr= log daily wage rate units, logtimediff= log time difference between alt. A and alt. B in months. This model corresponds to Model 4 in Table 3 and Table 4.

Table 7: Zoom models by district for Ethiopia. [a]

| | *Dependent variable:* $\log\delta$ *by district* | | | | |
|---|---|---|---|---|---|
| | Raya Azebo | Degua Tembien | Seharti Samre | Kilite Awlalo | Adwa |
| <u>Zoom:</u> | | | | | |
| a | 1.76 | 1.80 | 1.86 | 2.21 | 2.11 |
| b | 1.01 | 1.06 | 1.02 | 0.91 | 0.90 |
| <u>Base asset:</u> | | | | | |
| c | 1 | 1 | 1 | 1 | 1 |
| logdwr | -0.002 | -0.001 | 0.001 | 0.000 | 0.000 |
| | (0.012) | (0.014) | (0.019) | (0.018) | (0.012) |
| logtimediff | 0.001 | 0.001 | -0.000 | -0.000 | -0.001 |
| | (0.019) | (0.019) | (0.027) | (0.026) | (0.015) |
| presentdummy | 0.126*** | 0.116*** | 0.143*** | 0.141*** | 0.164*** |
| | (0.031) | (0.028) | (0.046) | (0.050) | (0.025) |
| Constant | 0.116* | 0.296*** | 0.141* | 0.337*** | 0.150*** |
| | (0.064) | (0.064) | (0.080) | (0.068) | (0.050) |
| $\mu$-Constant | 0.069*** | 0.077*** | 0.069*** | 0.104*** | 0.081*** |
| | (0.015) | (0.015) | (0.024) | (0.021) | (0.015) |
| Observations | 20,983 | 25,861 | 12,688 | 15,043 | 34,809 |
| Respondents | 186 | 233 | 115 | 133 | 311 |

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[a] All models have starting row FE and enumerator FE for the Luce error. logdwr= log daily wage rate units, logtimediff= log time difference between alt. A and alt. B in months.

## 5. Conclusion

We have used experimental field data from two countries (Ethiopia and Malawi) to assess the external validity of the zooming theory of Holden and Quiggin (2017). The zooming theory argues that general hyperbolic and magnitude effects in time preference experiments are explained by variable asset integration. A more holistic evaluation takes place for prospects with longer time horizons and larger amounts. This implies that such prospects are, to a larger extent, integrated with the current wealth, income, or consumption level of the respondents than prospects with a shorter horizon and smaller amounts.

We use a large data set from five districts in Ethiopia and compare with the original data set of Holden and Quiggin (2017) from Malawi. Our analysis shows a highly consistent pattern of population-averaged hyperbolic time horizon and magnitude effects across locations. Moreover, the pattern appears to be consistent with the zooming theory. The time horizon and magnitude effects imply that discount rates fall as time horizons and payout magnitudes increase. It must be stressed that the hyperbolic pattern persists when we control for the present bias in the data.

The results are promising in the sense that an introduction of two zooming parameters, one for the time horizon (a) and one for the magnitude (b), was enough to capture a large share of the within-subject treatment effects across all experimental samples. Equally important, the actual zooming appears to be roughly at the same level across districts and countries.

An intriguing regularity is the difference between dimensionless zooming parameters for time and magnitude. The parameter for time is roughly twice the parameter for magnitude. This implies that a doubling of the time between payout alternatives gives roughly a fourfold increase in asset integration compared to a doubling of the magnitude, which gives only a twofold increase in asset integration. This difference is also robust across countries and districts. Future research may shed more light on the extent to which this is a fundamental part of human cognition and heuristics.

At the aggregate population level, we found that the behavioral responses in the time preference experiments were consistent with the zooming theory and variable asset integration. While evidence of partial or no asset integration has been observed in risk preference experiments, asset integration has received less attention with respect to time preferences. We show that mental zooming or narrow bracketing not only may explain small stakes risk aversion but also hyperbolic and magnitude effects in time preferences.

## References

Abdellaoui, M., Bleichrodt, H., l'Haridon, O., and Paraschiv, C. (2013). Is there one unifying concept of utility? an experimental comparison of utility under risk and utility over time. *Management Science*, 59(9):2153–2169.

Andersen, S., Cox, J. C., Harrison, G. W., Lau, M. I., Rutström, E. E., and Sadiraj, V. (2018). Asset integration and attitudes toward risk: Theory and evidence. *Review of Economics and Statistics*, 100(5):816–830.

Andersen, S., Harrison, G. W., Lau, M. I., and Rutström, E. E. (2008). Eliciting risk and time preferences. *Econometrica*, 76(3):583–618.

Andersen, S., Harrison, G. W., Lau, M. I., and Rutström, E. E. (2013). Discounting behaviour and the magnitude effect: evidence from a field experiment in denmark. *Economica*, 80(320):670–697.

Andersen, S., Harrison, G. W., Lau, M. I., and Rutström, E. E. (2014). Discounting behavior: A reconsideration. *European Economic Review*, 71:15–33.

Andreoni, J. and Sprenger, C. (2012). Estimating time preferences from convex budgets. *American Economic Review*, 102(7):3333–56.

Andreoni, J. and Sprenger, C. (2015). Risk preferences are not time preferences: reply. *American Economic Review*, 105(7):2287–93.

Augenblick, N., Niederle, M., and Sprenger, C. (2015). Working over time: Dynamic inconsistency in real effort tasks. *The Quarterly Journal of Economics*, 130(3):1067–1115.

Augenblick, N. and Rabin, M. (2019). An experiment on time preference and misprediction in unpleasant tasks. *Review of Economic Studies*, 86(3):941–975.

Balakrishnan, U., Haushofer, J., and Jakiela, P. (2020). How soon is now? evidence of present bias from convex time budget experiments. *Experimental Economics*, 23(2):294–321.

Benzion, U., Rapoport, A., and Yagil, J. (1989). Discount rates inferred from decisions: An experimental study. *Management science*, 35(3):270–284.

Binswanger, H. P. (1981). Attitudes toward risk: Theoretical implications of an experiment in rural india. *The Economic Journal*, 91(364):867–890.

Böhm-Bawerk, E. v. (1889). The positive theory of capital, trans. *W. Smart (New York: Stechert).*

Charness, G., Gneezy, U., and Halladay, B. (2016). Experimental methods: Pay one or pay all. *Journal of Economic Behavior & Organization*, 131:141–150.

Cheung, S. L. (2016). Recent developments in the experimental elicitation of time preference. *Journal of Behavioral and Experimental Finance*, 11:1–8.

Cheung, S. L. (2019). Eliciting utility curvature in time preference. *Experimental Economics*, pages 1–33.

Epper, T., Fehr-Duda, H., and Bruhin, A. (2011). Viewing the future through a warped lens: Why uncertainty generates hyperbolic discounting. *Journal of risk and uncertainty*, 43(3):169–203.

Green, L., Myerson, J., and McFadden, E. (1997). Rate of temporal discounting decreases with amount of reward. *Memory & cognition*, 25(5):715–723.

Grijalva, T. C., Lusk, J. L., Rong, R., and Shaw, W. D. (2018). Convex time budgets and individual discount rates in the long run. *Environmental and Resource Economics*, 71(1):259–277.

Grijalva, T. C., Lusk, J. L., and Shaw, W. D. (2014). Discounting the distant future: An experimental investigation. *Environmental and Resource Economics*, 59(1):39–63.

Halevy, Y. (2008). Strotz meets allais: Diminishing impatience and the certainty effect. *American Economic Review*, 98(3):1145–62.

Halevy, Y. (2015). Time consistency: Stationarity and time invariance. *Econometrica*, 83(1):335–352.

Holden, S. T. and Quiggin, J. (2017). Bounded awareness and anomalies in intertemporal choice: Zooming in google earth as both metaphor and model. *Journal of Risk and Uncertainty*, 54(1):15–35.

Holt, C. A. and Laury, S. K. (2002). Risk aversion and incentive effects. *American economic review*, 92(5):1644–1655.

Kirby, K. N. and Maraković, N. N. (1995). Modeling myopic decisions: Evidence for hyperbolic delay-discounting within subjects and amounts. *Organizational Behavior and Human decision processes*, 64(1):22–30.

Loewenstein, G. and Prelec, D. (1992). Anomalies in intertemporal choice: Evidence and an interpretation. *The Quarterly Journal of Economics*, 107(2):573–597.

Phelps, E. S. and Pollak, R. A. (1968). On second-best national saving and game-equilibrium growth. *The Review of Economic Studies*, 35(2):185–199.

Rabin, M. (2000). Risk-aversion for small stakes: A calibration theorem. *Econometrica*, 68:1281–1292.

Samuelson, P. A. (1937). A note on measurement of utility. *The review of economic studies*, 4(2):155–161.

Thaler, R. (1981). Some empirical evidence on dynamic inconsistency. *Economics letters*, 8(3):201–207.

Wik, M., Aragie Kebede, T., Bergland, O., and Holden, S. T. (2004). On the measurement of risk aversion from experimental data. *Applied Economics*, 36(21):2443–2451.

## 6. Appendix

Table 8: Details regarding the Ethiopian Experiment.

| Series | Initial time (weeks) | Future time (months) | Future Amount (ETB) | Task Row 10 Amount (ETB) |
|---|---|---|---|---|
| 1 | 1 | 3 | 100 | 5 |
| 2 | 1 | 6 | 100 | 5 |
| 3 | 1 | 12 | 100 | 5 |
| 4 | 1 | 3 | 500 | 25 |
| 5 | 1 | 6 | 500 | 25 |
| 6 | 1 | 12 | 500 | 25 |
| 7 | 1 | 3 | 1000 | 50 |
| 8 | 1 | 6 | 1000 | 50 |
| 9 | 1 | 12 | 1000 | 50 |
| 10 | 0 | 12 | 100 | 5 |

Example of CL for time preference estimation:

Table 9: The Ethiopian Experiment.

| Time pref. Series no. | Start point | Task no. | Receive at far future period | Choice | Receive at near future period | Choice |
|---|---|---|---|---|---|---|
| 8 | | 1 | 1000 | | 1000 | |
| 8 | | 2 | 1000 | | 900 | |
| 8 | | 3 | 1000 | | 800 | |
| 8 | | 4 | 1000 | | 700 | |
| 8 | | 5 | 1000 | | 600 | |
| 8 | | 6 | 1000 | | 500 | |
| 8 | | 7 | 1000 | | 400 | |
| 8 | | 8 | 1000 | | 300 | |
| 8 | | 9 | 1000 | | 200 | |
| 8 | | 10 | 1000 | | 100 | |
| 8 | | 11 | 1000 | | 50 | |