DISCUSSION PAPER SERIES

DP17157

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DEVELOPMENT ECONOMICS

CEPR

ISSN 0265-8003

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Discussion Paper DP17157
Published 29 March 2022
Submitted 29 March 2022

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JEL Classification: D31, O12

Keywords: relative poverty, peer rankings, network analysis, West Africa

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Acknowledgements
We would like to acknowledge and thank Eva Lestant and Arsene Zongo for excellent field research assistance, and Innovations for Poverty Action (IPA) Cote d'Ivoire for collecting the data. We are grateful to the Stanford King Center on Global Development and to the IPA Research Methods Initiative for funding this research.

Measuring Relative Poverty through Peer Rankings: Evidence from Côte d'Ivoire*

Pascaline Dupas† Marcel Fafchamps‡ Deivy Houeix§

This draft: March 16, 2022

Abstract

We investigate a method for eliciting relative poverty rankings that aggregates partial poverty rankings obtained from multiple individuals. We first demonstrate that the method works in principle, then apply it in urban Côte d'Ivoire. We find that constructed rankings are often incomplete, not always transitive and sometimes contain cycles. Pairwise rankings reported by respondents and constructed aggregate rankings are poorly correlated with measures of poverty obtained from survey data. Measuring relative poverty through peer rankings appears difficult in urban and peri-urban settings.

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1 Introduction

Many developmental interventions aim to target the poor (e.g., Ravallion 2000, Ravallion 2009, Ravallion 2015). In some instances, such as when poverty is highly concentrated, community-based or spatial targeting may be sufficient. But more often than not, targeting requires within-community targeting mechanisms (Elbers et al. 2007).

Various strategies have been developed to identify the poorest members of communities. In the absence of universal administrative data such as income tax filings, one strategy is to survey individuals or households and rank them on the basis of the information they provide. One famous example of this is the eligibility assignment of the Progresa Cash Transfer program in Mexico (Skoufias et al. 1999). Approaches vary only in the type of information that is collected: detailed surveys on consumption and income (e.g., Deaton 2019; Grosh and Glewwe 2000); light surveys on poverty indicators (e.g., assets – Elbers and Lanjouw 2003, Elbers and Yin 2007); or answers to subjective well-being questions (e.g., Ravallion and Lokshin 2001, Ravallion and Beegle 2016). These methods all have shortcomings: detailed surveys are expensive and time-consuming; short surveys are thought to be easily manipulable by respondents (Banerjee and Sumarto 2020); and subjective well-being is often not well correlated with material well-being, either over time or across countries (e.g., Blanchflower and Oswald 2004; Layard 2009). Furthermore, the rankings are affected by measurement error and possible response bias or manipulation, leading to mis-assignment.[1]

Another method is to delegate the targeting decision to the local level. For example, local chiefs in Malawi are tasked with identifying poor households eligible for a large farming input subsidy (Basurto et al. 2020). A key concern with this is local capture or nepotism (Alatas et al. 2013). To mitigate this, one can solicit relative rankings from community members themselves – often gathered in a focus group. The focus groups are asked to produce complete relative poverty rankings of a set of individuals or households, typically all those in their village or neighborhood (e.g., Alatas et al. 2012). The main advantages of this method are that it is, on the one hand, simpler and cheaper to implement than detailed surveys, and, on the other hand, more transparent than relying on the local elite alone. This approach has been shown to produce reasonable rankings in

[1]The measurement error arises from the fact that respondents have only imperfect knowledge of the answer – e.g., because they do not recall or do not have full information about other household members. This noise leads to errors of assignment – known as type I and type II errors (e.g., Ravallion 2015). Response bias arises when respondents expect a benefit from being assigned to a high or low rank – such as a welfare benefit from being classified as 'below the poverty line'. To the extent that everyone faces the same incentive to bias their survey responses downward or upward, this need not lead to distorted rankings. But it can result in mis-classification of respondents as poor or non-poor (e.g., Ravallion 2008).

a rural context (e.g., Alatas et al. 2012).

One potential drawback is that focus group rankings may reflect local prejudices and views about who is a deserving poor, thereby deviating from the values of the developmental intervention (e.g., Galasso and Ravallion 2005, Ravallion 2008, Alatas et al. 2019). It also assumes that community members have the necessary information to provide the requested rankings. To investigate this assumption, Alatas et al. 2016 ask individuals to rank eight of their neighbors by economic well-being. They compare these reported rankings to self-reported economic status and they test whether the accuracy of the reported rankings varies with the position of the respondent in the local social network. They find that network proximity and network centrality predict more accurate rankings and conclude that poverty targeting should rely on key informants who are more socially central.

While relying on key informants can produce meaningful rankings in small rural hamlets, it is unclear whether it applies to urban and peri-urban areas with a more mobile population and less dense social networks. What methods can help with generating accurate poverty rankings in contexts where it is unlikely that any one individual is capable of ranking all local residents? In this paper, we seek to construct aggregate poverty rankings from partially overlapping rankings provided by multiple individuals in the same locality. In contexts where information on relative economic well-being is too diffuse for a small group of individuals to know everyone, individuals may nonetheless have enough local information to rank a small number of socially proximate households, e.g., neighbors. One study in an urban setting (Beaman et al. 2021) finds little evidence that individuals can accurately assess whether randomly selected community members are poor. They nonetheless target transfers to the poor modestly better than would be attributable to chance, suggesting that they possess partial but relevant information. If this diffuse information can be combined in a meaningful way, it could be used to derive an aggregate poverty ranking.[2]

We propose a novel methodology for aggregating partial rankings and implements it in a large African metropolis. We ask a small sub-sample of respondents in 34 different neighborhoods of the Greater Abidjan area in Côte d'Ivoire to rank up to 14 target households in that neighborhood. Three types of respondents are included: target households, who were selected at random from the neighborhood; neighboring households; and local hawkers and traders who can presumably observe the consumption patterns of their clients. We combine all their responses to construct an aggregate ranking of the 14 target households for each neighborhood.

[2] Alatas et al. 2016 note that, even in a rural setting, lack of information leads to partial rankings because respondents are unable or unwilling to rank certain individuals.

We then compare reported rankings and constructed (aggregated) rankings to rankings based on survey data. Target households answer an LSMS-style detailed survey covering incomes, consumption, assets, and many other household characteristics. Based on responses to this survey, we compute various measures of household income and consumption for each target household. We also construct two summary statistics often used in practice: a Proxy Means Test (PMT) of poverty that applies weights used in the studied country to survey data on assets and durables; and a Poverty Probability Index (PPI) calculated on answers to a small set of specifically selected survey questions proposed by Innovations for Poverty Action (IPA). We then compare the reported and constructed aggregate rankings from peer-to-peer comparisons to the rankings produced by the PMT and PPI indices as well as by various measures of consumption. Our empirical setting, Abidjan, a large metropolis of more than five million inhabitants in West Africa, is a good choice of setting because poverty measurement is a topical policy issue in the region. In particular, in 2019, Côte d'Ivoire started to roll out its universal health care coverage (CMU -Couverture Medicale Universelle) that provides free access to health care to the poorest members of a community. This is a context where, as we show, poverty levels are highly heterogeneous within neighborhoods, which means that geography-based targeting is insufficient. Under the ongoing government scheme, the poor are identified using a combination of observables (PMT) and community assessment with local leaders. Whether leveraging peer rankings can improve the targeting of the program is an outstanding question in this and similar contexts.

We also investigate the extent to which households bias their rankings when asked to self-rank, a question that has been the focus of recent theoretical work (e.g., Bloch and Olckers 2021a, Bloch and Olckers 2021b). To this effect, a randomly selected half of the respondents are asked to rank themselves among the 14 target households; the other half are only asked to rank the targets.

We have three main results. The first result is methodological. We demonstrate that it is, in principle, possible to construct the relative rankings of all households in a neighborhood by combining partial rankings provided by a multitude of individual informants. We also point out potential reasons why this theoretical possibility may be elusive in practice.

The second result is empirical. We show that the constructed rankings fall short of expectations, due to two critical shortcomings: (1) they are incomplete in all cases – sometimes severely so; and (2) they are not always transitive – many contain cycles. These empirical findings highlight the limitations of using peer rankings in high-density neighborhoods. Many respondents simply do not know many of the households around them.

As a result, there is little information to be harnessed from many of the respondents. This means that the very areas for which geographical targeting is known to be ineffective – i.e., dense urban neighborhoods – are also areas where low density peer ranking appears to be of little use. Higher density peer rankings could nonetheless yield more complete constructed rankings.

Our third result is that the pairwise rankings reported by respondents are not highly correlated with the various observational measures we collected on target households. This applies both to the pairwise rankings reported by individual respondents as well as to the constructed aggregate rankings obtained by combining individual answers. We also find that the PMT, PPI, and consumption measures from the survey are only moderately correlated with each other, suggesting the presence of measurement errors in those measures as well. But these observational measures are all more predictive of each other than rankings are of them. We also investigate whether reported rankings correlate better with the conspicuous consumption expenditures of the target households. They do not.

These results help provide some sense of when and how the method can yield useful information. The individual informants in our empirical application were asked to rank 14 households in neighborhoods that often contain more than 200. In a large number of cases, informants did not know the target households and, as a result, the fraction of reported rankings falls far below the number of rankings needed to reliably construct aggregate rankings for each neighborhood. This suggests that successful implementations of the method require a sufficiently large ratio of informants to target households, and a sufficiently limited geographical area from which the target households and informants are selected.

The low correlation between reported rankings and observational data also suggests that urban and peri-urban areas may experience too much income variation and spatial mobility to allow neighbors to accurately guess each other's relative economic standing. The fact that reported rankings are not even correlated with conspicuous consumption makes us further suspect that urban and peri-urban households do not, in general, pay much attention to each other – or at least that they do not compare themselves to others in their neighborhood. This is unlike in rural areas where relative rankings have been successfully collected from key informants in many countries. Our interpretation is consistent with the work of Fafchamps and Shilpi 2008 who find that rural respondents in Nepal gauge their subjective well-being relative to their neighbors while residents of Nepalese towns and cities do not. This could explain why it is possible to ask rural dwellers to rank each other, but not urban residents.

The remainder of the paper is organized as follows. Section 2 demonstrates the main methodological contribution of this paper. Section 3 explains the experimental design and data collection. Section 4 describes the empirical rankings obtained and the subsequent directed graph of relative rankings. Section 5 investigates whether rankings are informative. Section 6 examines the self-rank randomized treatment. Section 7 looks at characteristics that predict the propensity to rank others.

2 Methodology

Our objective is to construct an aggregate ranking from a multitude of partial rankings. Consider a set S of n individuals ranked in order of income:

$$y_1 < y_2 < ... < y_n$$

where, for now, we assume that all inequalities are strict. This true ranking can be represented as an $n \times n$ matrix R with $r_{ij} = 1$ if $y_i < y_j$. For instance, for $y_1 < y_2 < y_3 < y_4$, matrix R is:

$$R \equiv [r_{ij}] = \begin{bmatrix} . & 1 & 1 & 1 \\ 0 & . & 1 & 1 \\ 0 & 0 & . & 1 \\ 0 & 0 & 0 & . \end{bmatrix} \tag{1}$$

where r_{ii} is defined as missing (i.e., $r_{ij} = .$). The total rank t_i of individual i is simply the sum of its row $+1$:

$$t_i = \sum_j r_{ij} + 1$$

where the sum is taken over non-missing values. The richest person, individual 4, has rank 1; the second richest, individual 3, has rank 2, and so on. The poorest individual is the one with the largest number of individuals richer than he/she is, i.e., individual 1 in this case.

Missing information is easily accommodated. Say $y_1 < y_2 < y_3 < y_4$ but respondent a does not know y_3. We have:

$$R_a = \begin{bmatrix} . & 1 & . & 1 \\ 0 & . & . & 1 \\ . & . & . & . \\ 0 & 0 & . & . \end{bmatrix} \tag{2}$$

from which we see that a missing person shows up in R_a as a missing row and column. In this case, the rank of individual 3 is unknown from respondent a. The matrix representation can also accommodate disconnected rankings, e.g., let respondent b report $y_1 < y_2$ and $y_3 < y_4$. We then have:

$$R_b = \begin{bmatrix} . & 1 & . & . \\ 0 & . & . & . \\ . & . & . & 1 \\ . & . & 0 & . \end{bmatrix} \tag{3}$$

Disconnected rankings imply that R_b is a block diagonal matrix.

Equipped with this notation, we can combine rankings across respondents to obtain an aggregate ranking. We assume that the number of respondents is $m \leq n$ and that each respondent i observes the incomes of k_i individuals in set S. The rankings reported by the respondent k are represented by ranking matrix R_k. If respondents know the true income of the individuals they observe, their rankings always agree – which we assume for now. We show how the pairwise rankings of the individuals in set S can potentially be recovered using the average of the ranking matrices, which we denote:

$$\overline{R} \equiv \frac{1}{m_{ij}} \sum_{k=1}^{m} R_i \tag{4}$$

where m_{ij} is the number of non-missing values for matrix element ij. To illustrate the intuition behind the method, suppose one respondent reports that $y_1 < y_2$, another that $y_2 < y_3$ and a third that $y_3 < y_4$. Averaging yields:

$$\overline{R} = \begin{bmatrix} . & 1 & . & . \\ 0 & . & 1 & . \\ . & 0 & . & 1 \\ . & . & 0 & . \end{bmatrix} \tag{5}$$

from which we immediately see that $\overline{R} = [\overline{r}_{ij}]$ does not aggregate all the relevant information: by combining the three reports, we have $y_1 < y_2 < y_3 < y_4$, and yet $\overline{R}$ in equation (5) does not look like R in (1). To recover matrix R from $\overline{R}$ we use results from network analysis. By replacing each missing value by 0, ranking matrix $\overline{R}$ can be turned into the adjacency matrix of a directed network where a link from i to j means that i 'looks up to'

j, i.e., has lower income.[3] Let this matrix be denoted:

$$A \equiv \begin{bmatrix} 0 & 1 & 0 & 0 \\ 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 1 \\ 0 & 0 & 0 & 0 \end{bmatrix} \tag{6}$$

Asking whether there is a sequence of inequalities such that $y_1 < y_4$ is equivalent to asking whether there is a directed path from 1 to 4 in the directed network represented by A. It is well known that all paths in a network can be recovered iteratively by taking n successive (integer) powers of the adjacency matrix. Paths of length 1 are given by the non-zero elements in matrix A itself. A path of length $1 < l \leq n$ exists between nodes i and j if element a_{ij} in A^l is greater than 0. The shortest path between i and j is the smallest positive integer l at which $a_{ij} > 0$ in A^l. Let us now define $\hat{r}_{ij} = 1$ if there is a directed path, of any length, between i and j, and let us define the matrix:

$$\hat{R} \equiv [\hat{r}_{ij}]$$

Applying this method to matrix A in equation (6) yields $\hat{R} = R$ in equation (1). This demonstrates that, in this particular example, it is possible to recover the full ranking from the average ranking matrix $\overline{R}$.

There are many circumstances where the above approach does not identify the full ranking. This arises, for instance, when $\overline{R} = R_a$ or R_b. In matrix R_a, there is no information at all on y_3, which means it can never be ranked relative to the others. In matrix R_b, individuals $\{1, 2\}$ and $\{3, 4\}$ belong to two distinct components[4] and thus cannot be compared to each other. It is also possible that aggregating responses results in a partial ranking. For instance, if respondent a reports $y_1 < y_2$ and respondent b reports $y_1 < y_3$, we do not know whether y_2 is greater or smaller than y_3.[5] In that case, the network has a fork at y_1, with one arrow pointing to y_2 and another pointing to y_3. It is also possible to have two arrows pointing to the same node, e.g., if $y_1 < y_3$ and $y_2 < y_3$.

The ranking information recovered from respondents' reports can be represented in two ways. First, we produce graphs of the recovered rankings in each studied location. In order to convey all available information in an intuitive and compact format, we con-

[3]For the purpose of this calculation, if an average rank $\overline{r}_{ij}$ is less than 1,we set $\overline{r}_{ij} = 1$ if it is larger than 0.5 and 0 otherwise.

[4]A component of a network is a connected subnetwork that is not connected to any node outside of it.

[5]It is even possible that aggregating responses results in non-transitive rankings, i.e., cycles in the ranking matrix. We discuss this possibility below when we introduce reporting errors.

struct, for each studied location, the minimally connected graph $\hat{M}$ that spans $\hat{R}$. This graph is obtained by eliminating as many links from A as possible while ensuring that $\hat{M}$ still produces the same pairwise rankings $\hat{R}$ as A. For example, for the case where $y_1 < y_2 < y_3 < y_4$, $\hat{M} = A$ in equation (6): we only need three directed links to define all the relative rankings, we can drop the $\{1, 3\}$ link, the $\{1, 4\}$ link, etc, without changing the information about relative rankings that are presented in the graph.

Second, we also compute summary statistics that convey approximate information about the relative ranking of each individual in a compact fashion. These are r_i^{up}, the number of individuals who rank higher than i, and r_i^{down}, the number of individuals who ranked lower than i.[6] If we have complete ranking information on n individuals, then $r_i^{down} = n - r_i^{up} - 1$. When ranking information is incomplete, this is typically not the case. For individuals who are unranked, for instance, we have $r_i^{down} = r_i^{up} = 0$. The difference $P_i \equiv (n + r_i^{down} - r_i^{up})/2$ nonetheless remains informative about the *relative position* of individual i in the constructed network since it counts how many people can be ranked as poorer than i and subtracts how many can be ranked richer.[7] These rankings are instructive for individuals located within the same component but not across components since, by definition, individuals from different components cannot be compared to each other.

This approach can be generalized to allow for reporting mistakes. If these mistakes are uncorrelated across respondents, averaging rankings across respondents should converge to the true rankings. Formally, let the income of individual i that is observed by respondent k be given by:

$$y_i^k = y_i + e_i^k$$

where y_i is the true value and e_i^k is an i.i.d. observation error with mean 0 and variance σ_e^2. Respondent k reports $y_i < y_j$ iff $y_i^k < y_j^k$, which implies that:

$$Prob(y_i^k < y_j^k) = Prob(y_i - y_j < e_i^k - e_j^k)$$

Hence if we observe multiple reports r_{ij}^k, reporting mistakes can be minimized by averaging these reports across all k respondents.

Reporting mistakes can be particularly damaging for constructed aggregate rankings. In particular, it is possible for (directed) cycles to arise in the minimally connected graph $\hat{M}$ – and the corresponding matrix $\hat{R}$. In other words, it is possible for inferred rankings

[6] Variable r_i^{up} is the row sum of matrix $\hat{R}$ for individual i,while r_i^{down} is the column sum of matrix $\hat{R}$ for that same individual.

[7] Adding n and dividing by 2 ensures that, in the full information case, $P_i = r_i$. Unranked individuals receive a middle rank of $n/2$.

not to be transitive. To illustrate, let the true ranking be $y_1 < y_2 < y_3 < y_4$, but one respondent mistakenly reports $y_4 < y_1$. The minimally connected graph $\hat{M}$ that spans $\hat{R}$ is now a directed circle going from y_1 to y_4 – and then back to y_1. It follows that $r_i^{down} - r_i^{up} = 0$ for all individuals in this directed circle: they cannot be ranked globally. We expect this to arise when respondents are least able to rank two households by income, e.g., when their incomes are relatively similar, or households may assess poverty differently. Pairwise comparisons do, however, remain informative: in our example, it is only the pairwise comparison $y_4 < y_1$ that is incorrect; all the others correspond to the true ranking. For this reason, we conduct our analysis both in terms of aggregate and pairwise rankings.

Simulation analysis In Appendix B we use simulations to examine how precisely our reconstructed rank index P_i approximates true ranks r_i. We do this under different scenarios regarding the information available to respondent observers k, namely: the number of target households they know – and thus can provide a report on; and the precision of the information they have about other households' incomes.

We find that when respondents observe a large enough proportion of local households and have accurate information about their income, a lot of information on true ranks r_i can be recovered from the reconstructed rank index – in many cases, P_i accurately ranks all or nearly all target households. As could be expected, the method starts to fail when observers only known a very small proportion of the target households. This is because, in this case, there is insufficient overlap across the pairwise rankings provided by different observers. As a result, the minimally connected set $\hat{M}$ does not constitute a giant component that contains a majority of target households and, hence, many household pairs cannot be ranked relative to each other.[8]

Allowing for observation error in the information available to observers unsurprisingly leads to a deterioration of the precision of reconstructed ranks P_i. This arises because of the creation of branches and cycles in the reconstructed ranking matrix $\hat{M}$.[9] While averaging reported ranks r_ij^k can in principle attenuate the effect of observation error, it is not sufficient, in our simulations, to stop the deterioration. Averaging observers' reports does nonetheless provide valuable information on specific pairwise rankings even in the presence of cycles. We make use of this feature in our analysis.

[8]This finding is reminiscent of the relationship between the average degree of random graphs and the presence and size of a giant component – e.g., Jackson 2010.

[9]The only exception is when the proportion of observed households is so small that the minimally connected set $\hat{M}$ is very sparse. Because the reconstructed matrix has few links, observation error does not lead to the creation of cycles. For this reason, the (poor) performance of P_i remains insensitive to observation error.

HodgeRank Algorithm Bloch and Olckers 2021b have proposed an alternative method for aggregating pairwise comparisons into a single measure. Their method relies on local network information and does not seek to construct the full ranking matrix $\hat{R}$ and its minimally connected set $\hat{M}$. In this method, the reported rank $Y_{ij}^k = 1$ if k ranks i above j, $Y_{ij}^k = -1$ if k ranks i below j, and 0 if k does not rank i relative to j. The researcher then computes the average of the reported ranks as:

$$Y_{ij} \equiv \frac{1}{N} \sum_k Y_{ij}^k$$

where N is the total number of respondents. The relative 'score' or ranking of individual i is then obtained by applying the HodgeRank algorithm of Jiang et al. 2011 to minimize the squared difference between the scores and the aggregated rankings Y_{ij}. It is the solution to:

$$\min[\sum_{\{i,j\} \in N} ((s_i - s_j) - Y_{ij})] \tag{7}$$

In practice, the scores are the coefficients of individual-specific dummies d_m in a least-square regression of Y_{ij} on $d_m = 1$ if individual $m = i$, $d_m = -1$ if $m = j$, and 0 otherwise.

Jiang et al. 2011 show that the residuals of the least-squares problem corresponds to the cycles in the directed graph. Based on this, they propose the following cycle ratio measure as an indicator of the importance of cycles in the graph:[10]

$$\text{Cycle ratio} = \frac{\sum_{\{i,j\} \in N} ((\hat{s}_i - \hat{s}_j) - Y_{ij})^2}{\sum_{\{i,j\} \in N} (Y_{ij})^2} \tag{8}$$

The higher the cycle ratio, the more cycles dominate the network and the less informative the graph is about aggregate rankings – irrespective of the information contained in pairwise rankings. The cycle ratio, however, only considers the ranked nodes and ignores unranked ones. It also does not correct for the presence of multiple (disconnected) components. We illustrate this limitation in Section 4.

[10]The reader will immediately recognize that the cycle ratio is equal to $1 - R^2$, and is thus also a measure of the inadequacy of fit. This is because the more the graph is dominated by cycles, the less discriminating the individual HodgeRank dummies $\hat{s}_i$ and $\hat{s}_j$ are, and the less well they predict pairwise rankings Y_{ij}.

3 Application: Study Design

3.1 Sampling frame

The sample for the peer ranking exercise was directly embedded in a data collection effort conducted under the African Urban Development Research Initiative (AUDRI) at Stanford University. The main objective of AUDRI is to generate representative data of urban and peri-urban populations in the Greater Abidjan, the capital city of Côte d'Ivoire. We use the National Statistical Institute (INS)'s enumerations areas (EAs) as a sampling frame. In 2014, EAs were defined as follows: (i) in urban areas, an EA includes exactly 200 households, (ii) in rural areas, an EA includes all the households living in a village, which can be more or less than 200.

We used the EA geographic delimitation as described in the 2014 database to infer the total rural and urban population. About 83% of the sampling frame live in urban areas in 2014. The AUDRI sample over-samples areas in the process of urbanizing, with 84 "semi-rural" EAs (peri-urban villages) and 622 urban EAs across 16 sub-districts around the capital city of Abidjan. These correspond to the yellow areas in Figure 1. For the ranking study, we selected 20 "semi-rural" EAs and 20 urban EAs among those in the AUDRI sample. [11] The 20 urban EAs were randomly selected among EAs (a) in the two most populated municipalities in Abidjan and (b) defined as "slums" according to the 2014 census.[12] These study areas are named *ranking areas* hereafter.

3.2 Household Sampling and Data

Listing and Individual Surveys A household listing survey was conducted for the AUDRI project in July-August 2019 in all 706 EAs. In the *ranking areas*, for the sake of this study, enumerators were instructed to list 14 *consecutive* households (neighbors). They started counting households from the centroid (barycentre) of the EA and moved in circles of increasing radii around the centroid, knocking on doors. The listing survey collected information about each member of the household and basic dwelling characteristics and asset ownership. Only one member of the household (above 18 years old) was surveyed and asked about other members.

The total listing sample in the *ranking areas* contains 207 households.[13] The short sur-

[11] Six villages could not be reached by the team of surveyors because the village chief did not allow the study to enter. So we ended up surveying 20 urban EAs and 14 rural EAs.

[12] Slums were defined based on the definition of UN-Habitat 2006, i.e., areas lacking access to improved water, improved sanitation, sufficient living area, durable housing, and secure tenure.

[13] Dwellings with no one at home at the time of the first knock were included in the count and, if sampled

vey administered during the listing exercise included all the questions necessary to construct a PMT score for all 207 households.

From the listing survey, 70% of households in each EA were sampled. Among the sampled households, we randomly selected one adult for what we call "the Individual Survey". The Individual Survey was a 4-hour long questionnaire, which included a wide range of topics about the individuals' labor activities, transport habits, health conditions, and public service access. The Individual Survey was conducted between December 2019 and early March 2020. Out of the 142 individuals selected for the individual survey in *ranking areas*, 119 individuals were surveyed, representing a completion rate of 84%. Reasons for non-completion include re-locations and long-term travel, non-availability, appointment refusal, and insufficient (working) information to join or reach out to the respondent (non-working cellphone numbers, GPS position, and home directives). A detailed description of the collected survey data is given in Dupas et al. 2021.

Ranking Survey The ranking exercise was administered in the ranking areas in early March 2020, typically a few weeks after the Individual Survey.

The ranking exercise was done with a total of 507 respondents, of four types:

1. *Target households*: These respondents are taken from the 70% listed households selected for the Individual Survey. We re-visited them for the ranking exercise a few weeks after the Individual Survey to collect rankings.

2. *Listed households*: These respondents are taken from the 30% listed households that were not selected for the Individual Survey. In order to administer the ranking survey with these households, we contacted the head and scheduled an appointment, then surveyed a household member available at home at the time of the enumerators' visit.[14] The survey includes a subset of the modules in the Individual Survey, notably consumption information, and the rankings.

   $\Rightarrow$ Those two groups of individuals are given ID numbers in the 200's.

3. *Additional Households*: These are households who were absent during the listing. We attempted to visit them again and, when we found someone available, administered

for listing, revisited later in the day or in the next few days to attempt to conduct the listing survey. Thus, listed households considered "absent" are households for whom no member could be surveyed during the listing. Note that in four EAs, due to miscommunication in the field, dwelling closed on the first visit were neither counted nor listed. Thus, surveyed households live quite far apart (a few blocks away) from each other in these four areas. We control for this case in the analysis when possible.

[14]Note that we did not randomly select the respondent for this group.

the ranking questionnaire. We also asked for additional information (e.g., PPI questions and consumption module) since these households were not surveyed before. We surveyed a household member found at home at the time of the enumerators' visits in the EA. These households are given ID numbers in the 300's.

4. *Key Informants*: We visited key informants in each ranking area. As "chiefs" are rare or nonexistent in Abidjan's urban areas, we decided to survey traders around the surveyed dwellings in both rural and urban areas for consistency. Those individuals are numbered 900's.

This protocol was used to generate a sufficient overlap between individuals' rankings while keeping the number of surveyed respondents small enough to be cost-effective compared to a full survey approach to poverty targeting. While we have detailed information on the target households and some information on listed households, we collected much less data on additional households and key informants since their role is primarily to rank target households. The distribution of respondents is described in Table 1. The share of household heads surveyed is relatively similar across groups – except for the key informants.

3.3 Poverty measures

We collected several poverty measures, described below. For all measures, a lower value indicates *greater* poverty.

Consumption / expenditures We focus on three main household consumption measures collected on most of the sample:[15] (1) *Value of food consumption in the last week* before the survey: we used a typical consumption module to collect recall information on the value of household consumption of cereals, pulses, spices, milk products, meat, bread/pasta, vegetables, fruits, drinks, alcohol, and other consumables[16]. (2) *Value of conspicuous/social consumption in the last month* before the survey: we asked specific questions about non-food expenses, such as communication, beauty products, entertainment (concert, bar, cinema, games), and charitable contributions. (3) *Spending on durables in the last 12 months* before the survey: these include expenses for clothing, shoes, furniture, school

[15]The information is missing for 13 respondents who could not be reached at the time of the individual survey or did not recall their past consumption

[16]Note that the consumption module was administered earlier for respondents in the individual survey (about a month before), and we pulled all data together for consistency.

fees. Consumption information was collected on most respondents either at the time of the individual survey or right before the ranking activity.[17]

Proxy Means Test (PMT) We re-compute the PMT score built by the government of Côte d'Ivoire in 2015. The weights imputed to each household characteristic are the coefficients from a regression run by the government of Côte d'Ivoire on survey data collected in 2015 as part of the living standards measurement study. The regression predicted the log(food consumption per capita) using about 25 predictors (assets, house characteristics, etc.). We use on these same weights to build a PMT poverty index, whose distribution for our sample is shown in Figure 2. We also cross-validate the methodology used by the Government of Côte d'Ivoire with our data and compare the fit of the regression in predicting log(food consumption per capita). We obtain a similar fit and relatively similar coefficients in terms of magnitude (Table A2).[18] We cannot construct this index on additional households and key informants since, as described above, they were not administered the full questionnaire.

Poverty Probability Index (PPI) We also use an index developed by Innovations for Poverty Action (IPA), tailored to the Ivorian context. The PPI was created in April 2018 using Côte d'Ivoire's 2015 living standards measurement study. The index relies on ten questions (geographic location, household characteristics, living conditions), and we built a specific PPI score from these ten questions (see in Table A1) for the entire sample of households. We observe in Figure 2 that our sample of households is widely distributed across their poverty status and slightly wealthier than the average household in the country. This fact is consistent given the urban sample studied here, living in or around the capital city Abidjan. In Table A2, Column (6) and (7), we report the fit from the PPI regression, i.e., regressing log(food consumption per capita) on the variables used to build the PPI index. We obtain a reasonably large R^2.

Table 2 shows summary statistics from the various measures, separately for urban and peri-urban (rural) EAs. Figure 3 shows how the measures of poverty are correlated with each other. In particular, the PPI index is positively correlated with the PMT score, food consumption, conspicuous consumption, and expenditure on durables.

[17] A few respondents answered that they did not know when asked about a particular good consumed (typically 1-3% of the sample in a given consumption question). In such a case, we replace the answer by the average in the enumeration area to preserve the sample size.

[18] Note that the number of observations is limited for PMT since most households surveyed for rankings did not respond to the detailed Individual survey, including all PMT-related questions, as described in Section 3.

3.4 The ranking exercise

A total of 507 respondents were asked to rank neighboring households based on neediness. Respondents were told that the study's goal was to understand the lives of the communities around Abidjan and the extent to which people interact and know their neighbors. We asked respondents to name all of their neighbors, and the surveyors classified each of them on the tablets using a pre-loaded household list from the listing[19]. We then asked the respondents questions about each listed neighbor. Finally, we asked respondents to rank *all* the neighbors listed from the poorest to the richest. We did not ask them to rank within each possible pair, but instead to put the list in order. They could not leave anyone out of the ranking.

Respondents were told that their rankings (and any other survey information we collected from them) would *not* be used to provide anyone with any gift or anything else. As such, there was no incentive for participants to be strategic in their rankings and reporting. All respondents were told that they could do no better than telling the truth.

We randomly varied whether respondents were asked to rank their own household relative to others. This was meant to estimate the potential of manipulation in peer rankings (Bloch and Olckers 2021a). Within each neighborhood, around half of the respondents were asked to rank their household; the others were not.

We collected specific data to understand how people form their rankings and how they perceive poverty more generally. We asked three questions about poverty's perceptions, i.e., (i) the perceived poverty level of their household; (ii) how they regard their households compared to others in the neighborhood; and (iii) how they think others perceive the respondent's household. This data is summarized in Table 3. While 29% of respondents surveyed consider their households as poor, 21% consider themselves poorer than their neighbors, and 21% think that other people would classify their households as poor. Interestingly, 53% of the respondents who report their household as poor do not think that others regard them as poor.

We then asked respondents to tell us the criteria they used to classify households and to define poverty "in their own words". The vast majority of respondents refer to poverty as "food deprivation" (80%); some mention "unresolved health problems" (43%). To rank households, respondents predominantly report using the household head's occupation (49%) and whether households reported facing financial problems to them (49%). Finally, most households in our sample declare visiting the listed neighbors regularly (56%) and about half of them report receiving health/money advice from them.

[19] Respondents had to list at least 5 neighbors and were told to list as many neighbors as possible (maximum of 14)

4 Poverty rankings

In this section, we report the poverty ranking estimates obtained using the methodology described in Section 2. For the purpose of constructing the adjacency matrix A, if there exist multiple reports on a particular pairwise ranking r_{ij}, we reset the average rank $\bar{r}_{ij}$ to 1 if it is larger than 0.5 and 0 otherwise.

Ranking graphs for the 34 locations in which relative rankings information was elicited are presented in the appendix (see Figures A1 and A2 in urban slums and rural villages, respectively). Each node represents a household, identified to respondents by the name of the head of household, spouse, age, and residence location. We also measured the PPI Index for each household whenever possible, divided it into four equal categories across the entire sample and added it directly on the directed graphs. Households with id numbers in the 200's are households sampled for the Individual Survey.[20] Households with an id number in the 300's are neighbors added as respondents for the ranking exercise. In contrast, households with an id number in the 900's are "key informants" identified in the neighborhood – typically traders. We did not seek to explicitly elicit rankings on households with 300's and 900's IDs, but, as they are neighbors of 200's, they are sometimes ranked, and we manually matched based on similar names, ages, and household sizes. We keep them in the graphs because omitting them sometimes breaks the graph into multiple components.

We immediately note that some locations provided much more information than others. Locations 13, 15, 22, and 28 only contain information on two or three pairs of households. Thirteen locations are broken into two or more components that cannot be ranked relative to each other (i.e., 1, 3, 5, 10, 11, 16, 17, 18, 23, 24, 25, 29, and 33). This pattern leaves fourteen locations with a single component containing at least five households (i.e., 2, 4, 6, 7, 8, 9, 12, 14, 19, 20, 21, 30, 31, 32). Of these 13 locations, some (i.e., 2, 4, 6, 7, 20, 30) contain at least one (directed) cycle involving a subset of nodes; while the others are transitive.

In Table A3, we report r_i^{up} and r_i^{down} statistics for the 14 locations with a single component and at least five ranked households. We immediately notice that r_i^{up}, the number of ranked households who are richer than a given household, is not the same as $n - r_i^{up} - 1$. For instance, in location EA 2, there are 16 ranked households. Household 203 has no household ranked richer but thirteen households ranked poorer, while household 301 has no household ranked richer, but ten households ranked poorer. If we look at the di-

[20] These households are included in the graphs even if they could not be found when the listing survey was done.

rected graph of relative rankings for location 2 (Figure A1), we note that household 203 is at the top of a long sequence of ranked households. In contrast, household 301 sits on a side branch above household 209, but is un-ranked relative to households 203 to 208. This characteristic means that household 301 could be as rich or even richer than 203, but in all likelihood, it is poorer. We cannot, however, clearly rank 301 relative to households 208, 205, 201, 202, and 203. We also do not know how 902 ranks relative to household 208: it could be poorer or richer. This example illustrates the partial nature of the information we can recover from the reported rankings.

We also observe situations in which multiple households share the same number of poorer and richer households. This arises when the constructed rankings are non-transitive, i.e., when they include a cycle. Households located at either end of the ranking chain stand on their own, but for instance, households 203, 206, 210, all have the same (large) number of households ranked above and below them in EA 6. This is because there is a directed cycle between them, meaning that, based on our definitions, they are both richer and poorer than each other – i.e., they are ranked, but not in a meaningful way. This is clear in the directed graph of relative rankings for location 6 (Figure A1). One extreme case of this is location EA 30, in which all households but two are located on a set of large cycles: pairwise rankings exist, but they do not induce a meaningful aggregate ranking.

While these findings do *not* constitute an indictment of the methodology, they reduce the usefulness of its results when, as in locations 2, 4, 6, 7, 20, 30 with one or more cycles, the rankings data is contradictory. Constructed *pairwise* rankings nonetheless remain potentially informative: as explained in Section 2, a cycle can be caused by a single misreported link by a single respondent. All the other directed links (i.e., inequality relationships) in this cycle may still be correct. Given this, in the subsequent statistically analysis we consider both the aggregate constructed rankings and relative position P_i, as well as the constructed and reported pairwise rankings $\hat{r}_{ij}$ and r_{ij}, respectively.

Finally, we incorporate two additional measures in the directed graphs:

1. The *eigenvector centrality*: a measure of the influence of a node in the network. In our case, it describes how much a node is connected to many nodes, which themselves have high scores (i.e., a higher number of connections).[21]

2. The *cycle ratio* described in Equation 8. The lower this measure is, the better HodgeRank dummies can predict reported pairwise rankings for those nodes that are ranked

[21]In the case of disconnected networks within a location, we only report the eigenvector centrality for the largest network.

relative to each other. As already noted, the cycle ratio only considers ranked nodes and it does not penalize disconnected networks. This explains why some graphs have a very low cycle ratio but very few ranked nodes.

We observe large differences across locations, as shown in Figures A1 and A2. For instance, location 30 has a large cycle ratio of 0.376 (higher than all others) since it has a large cycle that includes most nodes. Locations with little knowledge and rankings across pairs get a much lower cycle ratio (e.g., location 15) because the few links that they contain do not involve cycles.

5 How informative are the rankings?

We now examine whether reported rankings are informative about differences in consumption levels across households. To this effect, we start by regressing differences in our various poverty indices between households i and j (in the dyadic dataset) on the reported and constructed pairwise ranks between i and j, as described in Section 2. Here, the "reported rank" variable is the share of reported ranks showing j richer than i. The "constructed rank" variable is a dummy equals to 1 if j is ranked richer than i, computed for part of the possible pairs. In the presence of a cycle, it is possible that j is ranked richer than i *and* i is ranked richer than j.

We also add an alternative way to aggregate rankings: the HodgeRank score, as developed by Bloch and Olckers 2021b and described in sub-Section 2. The set of HodgeRanks of all individual i is the set of scores s_i that minimizes the squared difference between scores and aggregated rankings. The difference in scores between j and i is used as an independent variable: the higher the difference, the richer j is ranked compared to i. A negative difference implies that j is ranked poorer than i. We note that the observed correlation between the constructed rank and the difference in HodgeRank scores is high ($\rho = 0.84$)[22].

All differences in outcomes are taken as j's value minus i's value. Results are presented in Table 4. We show regressions on four main outcomes, (1) food consumption per capita; (2) number of months during which the household suffered from food shortages; (3) PMT score; and (4) PPI score.[23] Except for food shortages, these measures are constructed such that a greater value means *less* poverty. Thus, if ranks are informative, we

[22]Since HodgeRank scores are assigned to each household, it is possible to compute the differences in HodgeRank scores for all possible pairs $i - j$, thus increasing the number of observations.

[23]In Table A4, we run the same regression for an extensive set of outcomes, e.g., different consumption variables, shortfall in consumption, an index of improvement in consumption relative to last year.

expect the coefficient of the different rank measures to be positive in columns 1, 3 and 4: when i is ranked poorer than j, j's consumption, PMT and PPI scores should be higher than i's. The reverse is expected for column 2.

We observe limited evidence that pairwise rank measures are informative about consumption differences per capita, PPI, or PMT. Most of the estimates are non-significant and are sometimes of the wrong sign (e.g., for food expenditure per capita). The coefficients reported in column 2 are negative as predicted, but only significant for constructed and hodgerank score's difference. The estimated R^2 is quite low throughout. From this, we conclude that, in general, rankings contain relatively limited information about consumption differences across ranked households.

Next we move the analysis to the level of the individual. Here the dependent variable is the level of the consumption measure. We estimate two sets of regressions, depending on which measure of aggregate rank for household i we use: its relative position P_i; and its estimated HodgeRank score $\hat{s}_i$. Results, presented in Table 5, show that estimated coefficients are not statistically significant. These results are perhaps not surprising, given the findings from pairwise regressions – and the fact that the information content of P_i is more affected by the presence of cycles than reported ranks.

To understand why individuals do not seem to make accurate rankings, we examine which characteristics of households appear to be predictive of reported ranks. The results are shown in Table 6. Regarding the asset/wealth data, as aggregated by the PPI index, the patterns are mostly consistent with expectations: if i has a lower PPI index than j, k is more likely to report that i is poorer than j. For consumption variables, j is ranked richer than i (positive coefficient) if j reports higher food consumption or higher spending on durables. The variables "months of food shortages", "expressed food worries in the last 12 months", or "received gifted food in the past week" also consistently predict rankings households with food deprivation poorer. Interestingly, conspicuous consumption expenditures do not predict reported rankings.

5.1 Ranking Accuracy

To examine the accuracy of reported ranks, we compare them with the rankings obtained from the PMT and PPI indices constructed survey data – which, for the purpose of this exercise, we regard as the true rankings. Overall, ranking accuracy is pretty low: reported rankings are right 52.5% of the time when we take PMT rankings as comparison, and 55.8% when we use PPI rankings.[24] Ranking accuracy is even lower if we use food expenditure per capita as comparison. Such low levels of accuracy are only barely above what could be achieved by random guessing. This is not because the indices themselves are random noise, though. We indeed find that PMT and PPI predict consumption per capita rankings well: the pairwise $i - j$ difference in PMT and PPI has the same sign as the difference in consumption per capita in 71% and 69% of cases, respectively.

To get a better sense of informative the reported ranks are, we simulate a ranking model calibrated on the data to assess how large the standard deviation of observation error would have to be in order to produce the ranking accuracy reported above.[25] To conduct this counterfactual experiment, we use as 'truth' the two welfare indices constructed from the data, PPI and PMT. All indices are standardized to have mean 0 and variance 1. We assume that each observer k sees a signal $y_i^k = y_i + e_i^k$ where y_i is either the PMT or PPI of i and where e_i^k is, as before, an i.i.d. observation error with mean 0 and variance σ_e^2 – and similarly for y_j^k. We then construct a simulated reported rank $r_{ij}^k = 1$ if $y_i^k > y_j^k$ and 0 otherwise. We do this for various values of σ_e until we find a value that gives the same ranking accuracy as above.[26]

Unsurprisingly, given the poor ranking accuracy of the actual reported ranks, we must posit quite a large σ_e in order to reproduce their ranking accuracy: across simulated vectors of observation errors, σ_e has to be at least 7.5 in order to reproduce the 52.4% PMT targeting accuracy of reported ranks; and the corresponding values for PPI is 3.5. In most simulations, σ_e has to be larger than 10 to match the accuracy of reported ranks. In other words, the standard deviation of the observation error e_i^k has to be a large multiple of the standard deviation of the truth y_i in order to account for the low ranking accuracy of reported ranks. This exercise is purely indicative, since we do not observe the 'true' welfare of individuals i and j. But it gives an idea of the magnitude of the observation error that characterizes our empirical setting.

Poor ranking accuracy may be due to a poor selection of observers k. If so, the usefulness of our method may be improved by selecting respondents with observable characteristics that predict ranking accuracy. To investigate this possibility, we regress each observer's ranking accuracy on a vector of respondent characteristics. To avoid oversampling observers who provide more rankings, we define, for each individual ranker k, the

[24] Because PPI is an integer index, some i, j pairs have the same PPI value and thus cannot be ranked. They are omitted from this analysis.

[25] The simulated dataset includes all the i, j, k triads for which a rank is reported by k and PMT or PPI values exist for both i and j. Pairs i, j that have identical PPI are dropped from the simulation. The sample size is 446 distinct triads for PMT and 283 for PPI.

[26] By construction, the ranking accuracy of the simulated reported ranks is 100% when $\sigma_e = 0$.

Ranking Accuracy of that observer as the *share* of pairs $i - j$ for which k accurately ranked i poorer or richer than j according to the $i - j$ difference in PPI, PMT, of household food expenditure per capita. We then regress this variable on observer characteristics. The results are shown in Table 7.[27] We do not find any convincing evidence that observer heterogeneity predicts ranking accuracy, implying little scope for improving on our method by over-sampling observers with certain characteristics.

5.2 Poverty Targeting

From a policy standpoint, accurate rankings between any given pair may not be needed. Instead, the policymaker may simply want to identify who is, say, below the median of the distribution. To test whether aggregate peer rankings can be used to do such identification, we create a dummy equal to 1 if a household's aggregate ranking puts it below the median of its EA. It is zero if the household is ranked at or above the median and missing if the household is not ranked. In Table 8, we use this dummy as a regressor, testing whether it correlates with whether the household is below the median based on the survey measures. Column 1 compares the categorization obtained thanks to the peer rankings exercise to that obtained from the PMT measure, column 2 to the categorization obtained from the PPI measure, and column 3 to the categorization based on food expenditure per capita. Quite strikingly, being categorized below the median does not significantly increase the likelihood that one is below the median based on any of the three survey measures, suggesting that even coarse categorizations are difficult to obtain from peer rankings. The below median classification based on the Hodgerank score does slightly better in predicting the PMT-based poverty classification, but not the others.

To test whether these mostly-zero results are driven by the fact that the probability of being ranked could itself be affected by one's position, we create a dummy equal to 1 if a household could not be given an aggregate ranking (this happened when none of the respondents surveyed listed that household as a known neighbor). Around 23% of the sample is "unranked". To test whether those unranked are disproportionately poor or disproportionately rich, the bottom panel of Table 8 shows regressions with this "unranked" dummy as the regressor. There is no statistically significant correlation, suggesting that categorizing those "unranked" as poor would not help improve targeting based on peer rankings.

[27]We can only estimate accuracy for respondents who ranked at least one pair of neighbors for which we have completed surveys. This represents 58% of respondents.

6 The self-ranking treatment

This section tests whether including self-ranks helps improve accuracy, exploiting the fact that we randomized whether respondents were asked to include themselves into their own rankings. The advantage of including self-ranking is that people have a lot of information about themselves, so in expectation the likelihood that they are *able* to generate an accurate ranking between two households in the neighborhood, one of which is their own, is higher. A potential issue with self-rankings is that households may have *too much information* about themselves compared to others. For example, they may know what is conspicuous consumption for themselves but not others. Another potential issue is that respondents may not have incentives to report truthful rankings when they are in the mix, e.g., if they expect the rankings to be used for anti-poverty program targeting. We wanted to limit this possibility in our setting, in order to focus on the question of whether respondents even *have* information that is relevant (leaving aside the question of how to elicit that information in an incentive-compatible way). For this reason, we made sure that respondents understood there were no manipulation incentives in our exercise–namely, we explained that nothing was at stake.[28]

To test whether including self-ranks help improve accuracy of aggregate rankings, Table 9 reproduces the Poverty Targeting table described in the previous section, but *excluding* the self-ranks from the data. We find that excluding self-ranks *improves* targeting accuracy somewhat: those ranked below the median of one's EA in the aggregate ranking that excludes self-ranks are 12.7 percentage points more likely to be below the median on the PMT index (Panel A, column 1); and those below the median based on the HodgeRank score are 16.9 percentage points more likely to be below the median on the PMT (Panel A, column 2). These differences are significant at the 10% and 5% level, respectively, and larger than those observed when self-ranks are included (Table 8). This is quite striking, and suggests that the information that people have about themselves is unhelpful to the social planner.

To investigate why that is the case, we check whether respondents rank themselves differently than what other respondents report about them. To do this, we estimate a regression of the form:

$$y_{ij}^k = \alpha S_{ij}^k + \theta_{ij} + u_{ij}^k$$

where $y_{ij}^k = 1$ if respondent k ranks i poorer than j, 0 if k ranks i richer than j, and missing otherwise; and $S_{ij}^k = 1$ if $k = i$, -1 if $k = j$ and 0 otherwise; and θ_{ij} is a pairwise

[28]Of course, we cannot be sure they trusted that was true, but as we show below, respondents if anything underestimated their own poverty level, suggesting that gaming in order to get benefits was not common.

fixed effect. We test whether α is larger or smaller than 0. If it is larger, it means that respondent k tends to self-rank below j (i.e., $y^k_{ij} = 1$ when $k = i$) and i (i.e., $y^k_{ij} = 0$ when $k = j$) more than what other respondents do. Put differently, $\alpha > 0$ implies that respondents rank themselves lower than the rank others give them. The reason why they do this could be due to humility – i.e., either because it is bad form to boast about one's own prosperity or because it attracts requests for financial assistance from neighbors. Alternatively, it could be that people rank each other based on their conspicuous consumption but rank themselves based on full consumption. The latter would imply some myopia: people systematically misjudge the true poverty of others even though they realize that their own conspicuous consumption gives an inflated image of their true prosperity. The experiment is not designed to identify which is the most likely explanation. In contrast, $\alpha < 0$ would imply that respondents give themselves a higher rank than the rank others give them – e.g., out of vanity.

Results are presented in Table 10. We see that α is significantly smaller than 0. The effect is quite large. By construction, actual ranks are equal to 1 half of the time. A coefficient of -0.28 means that, when ranking themselves, respondents rank themselves poorer than others 22 percent of the time while others rank them poorer 50 percent of the time. This suggests that a large fraction of respondents rank themselves as richer than others even though they are judged to be poorer by third parties. In particular, we note that 62% of the respondents ranked themselves the richest among their neighbors while only 22% of respondents rank themselves the poorest. This suggests that strategic under-reporting (e.g., in hope of getting financial assistance) was successfully minimized by our survey protocols. But over-reporting seems substantial, suggesting that there may be a psychological cost to admitting one's own poverty.

7 Propensity to rank and to be ranked

A main finding from our application is that rankings are far from complete. This is because many respondents did not know some of their neighbors enough to list and rank them. To further investigate the correlates of the propensity to rank and the propensity to be ranked, we construct a *dyadic dataset* indexed by the respondent k and a ranked household i in the same EA. We create a dependent variable $m_{ki} = 1$ if respondent k ranks household i relative to any other household, and 0 otherwise. We regress this dummy on characteristics of both i and k in Table 11.

We find that the geographic distance between the respondent k and household i has a significant (negative) effect on reporting. The absolute magnitude of the coefficient is small, but this is primarily because average reporting is low to start with.[29]

In column (2), we add information about consumption. We see that, some variables indicating that household i is poor tend to be negatively correlated with being ranked by k. For instance, households who experience food shortages over more extended periods are less likely to be reported on by k. We find limited evidence, however, that detailed consumption expenditures as reported by household i consistently helps predict reporting by k. If anything, the higher the food consumption, the less likely a household would be ranked by others. The category of expenditures classified as 'conspicuous', e.g., beauty products, eating out, and charitable contributions, positively predict being ranked by others.

Overall, these findings confirm that k's propensity to rank i can be partly accounted for by observable characteristics of i and how they compare to k's. This is reassuring because it indicates that k takes relevant characteristics of i into consideration when choosing to rank i relative to other households. The findings also suggest that richer households, at least in terms of assets, are in general more likely to be ranked. A plausible explanation is that their wealth is easily observable. With this ranking methodology, the rich are more likely to be ranked and thus the poor are less likely to appear in constructed rankings. This finding is problematic if the purpose of eliciting income and wealth rankings is, as is often the case, to identify the poor.

8 Conclusion

This paper introduced a method for eliciting relative poverty rankings that aggregates partial poverty rankings obtained from multiple individuals. We demonstrated that the method works in principle. In our empirical application, however, the constructed rankings are incomplete in all studied neighborhoods. Furthermore, they are not always transitive and many contain cycles. A more accurate picture may nevertheless be obtained by increasing the density of reporting.

We find that pairwise rankings reported by respondents are not highly correlated with various poverty measures we collected on a subset of households. The same holds for constructed aggregate rankings. From these findings, we conclude that reported ranks do capture relevant information about relative welfare. But this information is noisy. These findings are similar to those of Alatas et al. 2016, who note that many respondents in rural Indonesia are unable to rank others and that reported rankings are not, in gen-

[29] Experimentation with alternative functional forms indicate that the log form chosen here fits the data well.

eral, very accurate. We also find that reported rankings seem to reflect a few observable expenditures only. In addition, we investigate whether reported rankings correlate better with the conspicuous consumption expenditures of the target households. We find that they do not.

From this experiment, we conclude that rankings constructed based on peer rankings are probably insufficient to achieve poverty targeting at a cost lower than surveying households directly. The method may nonetheless prove useful to construct aggregate rankings in situations where accurate but partial pairwise rankings can be obtained from a small number of individual respondents – e.g., key informants in rural villages.

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Appendices

Appendix A. Estimating the precision of spatial targeting

In this Appendix, we examine the extent to which material well-being in our study area can be predicted on the basis of spatial information. To investigate, we rely on data from an individual survey collected by AUDRI in December 2019 to March 2020. The population covered by the AUDRI survey is comparable to this paper in terms of spatial coverage and sampling. It includes respondents sampled from 622 urban enumeration areas (EA) and 84 peri-urban villages located in and around Abidjan.[30] The median number of respondents is 4-5 per EA (80% of the sample) and 8-9 per rural areas (village), with sizeable variation across sampling units. In the analysis, we combine both sampling units and refer to them as sampling unit (SU).

Our measure of household material well-being is the proxy-means test (PMT) index based on the formula and weights used by the government of Côte d'Ivoire to measure poverty. This index is chosen for two main reasons: it is available for all but one of the 2940 individuals covered in the AUDRI survey; and it is arguably the most reliable measure we can build and the most acceptable to policy-makers in the country. For the purpose of our calculation, we take the median PMT of the sample as poverty cutoff.[31] We estimate the precision of spatial targeting by calculating the proportion of individuals correctly identified as poor or non-poor based purely on the predicted poverty level in their location –i.e., either their sampling unit or their GPS location. For simplicity, we ignore measurement error in the PMT index itself; our focus is on the extent to which a policymaker can rely on spatial targeting to approach the level of poverty targeting comparable to a PMT survey of all households in the Greater Abidjan region.

We start by regressing the PMT index on SU fixed effects and we check which proportion of the respondents in the individual survey would be corrected assigned to a poor or non-poor status if targeting is based on interviewing a small sample of individuals in each SU and relying on the SU fixed effects to identify the poor. Based on this calculation, 69.7% of the poor and 70.0% of the non-poor are correctly assigned to their respective category, leaving 30.3% of poor identified as non-poor and 29.0% of non-poor identified as poor. This simple calculation seems to suggest that a relatively high level of accuracy (70%) could be achieved by surveying a small sample of respondents in each SU and targeting all individuals in the SU's that have a sample average of the PMT index below the cutoff.

This calculation, however, is misleading because the sample mean in each SU suffers

[30] The list of EA's and the distinction between EA's and villages mirrors the methodology used for the population census by the the National Statistical Institute of Cote d'Ivoire.

[31] This is estimated as follows. Respondents are selected among 15,075 adults living 5,127 households with a total population of 26,101 individuals, children included. From the individual survey, we know that the median income per adult is $54 (Dupas et al. 2021). With approximately three adults per household, this implies a median income per person of $31.5 per month or $1.04 per day, which we use as a conservative value for the poverty cutoff. From this calculation, it follows that the cutoff corresponds roughly to the material welfare of the median PMT index household in our sample.

from sampling error, i.e., the SU fixed effects over-fit the small sample in each SU. To investigate this issue, we begin to testing whether the variation in SU fixed effects could entirely be driven by sampling error across SU's. This is achieved by bootstrapping the distribution of estimated SU fixed effects that would be obtained under the null hypothesis of no SU fixed effects. To this effect, we scramble the observed values of the PMT but keep the division of the sample into sampling units of the original size. After scrambling, each SU contains the same number of observations as in the original sample, but these observations have been permutated and come, at random, from other SU's. We then regress the PMT index on SU fixed effects and calculate the standard deviation of the 635 fixed effects.[32] By repeating this process multiple times, we are able to simulate the standard deviation of SU fixed effects under the null of zero systematic variation in the PMT index across SU's. The distribution of the simulated standard deviations is presented in Figure 4, and is to be compared to the observed standard deviation of the SU fixed effects in the original sample, which is 0.3. We see from Figure 4 that the distribution of the simulated standard deviation remains well below 0.3, which indicates that there is systematic variation in PMT index across SU's. In other words, targeting on the basis of SU fixed effects is *a priori* informative. We just do not know how much.

We then simulate the impact of sampling error on targeting by mimicking predicting out of sample. To this effect, we first recover the prediction errors $\hat{e}_i$ from the regression of the PMT index on the SU effects. These errors capture the variation of individual indices among households within each SU. We then scramble the $\hat{e}_i$'s across observation and add these permutated errors to the estimated SU fixed effects to construct an artificial sample of PMT values. This artificial sample can be thought of as representative of other households in the same SU since it suffers from the same amount of prediction error as the sampled individuals. We then calculate the accuracy of SU fixed-effect targeting on this artificial sample. We find that targeting accuracy drops dramatically for out-of-sample households:[33] only 58.6% of the poor are properly assigned to the poor category, while 39.0% of the non-poor are assigned to the poverty status. This demonstrates that interviewing a small number of individuals in each SU's so as to select SU's to receive a poverty intervention would only achieve around 60% targeting accuracy – which non-negligible but only 10% better than randomly targeting half of the SU's.

We also examine whether better targeting can be achieved by using the detailed GPS coordinates of respondents instead of relying on their sampling unit. To this effect, we fit a two-way kernel regression in (decimal) latitude and longitude on the PMT index.[34] The idea behind the approach is that average poverty varies relatively smoothly across space in and around the city. It is intended to capture the way a knowledgeable city planner would form a mental representation of the spatial distribution of affluent and poor neighborhoods – and may target anti-poverty interventions on that basis.

The resulting fit is illustrated in Figure 5, where each color represents a prediction quartile. We see that, as could be anticipated, predicted index values are higher in the vicinity of the city center and lower in the periphery, with a few exceptions corresponding

[32] 71 SU's with a single observation are omitted from this procedure.

[33] The predicted fit varies somewhat from one simulation to another. We present here the result from one representative simulation.

[34] We use the *npregress* command in Stata with an Epanechnikov kernel and an optimal bandwidth.

to secondary towns or affluent sub-urban neighborhoods. We expect this map to provide a reasonable match of where an informed Ivorian planner would expect the poor to live. We then compare these spatial predictions to actual PMT index values in our sample, using the same cutoff value as before. We find that this method misses most of the poor: it assigns 21.1% of the poor (and 12.6% of the non-poor) to the poverty category. Using this method would lead to massive under-targeting of anti-poverty interventions. Consistent with this, we observe large wealth differences (as measured in the PPI indexes) across households within EA in the directed graphs, Figures A1 and A2.

From this analysis, we conclude that there is considerable variation in poverty levels within small geographical units, making it difficult for policymakers to effectively reach the poor by targeting on the basis of local averages in our urban setting – irrespective of how these averages are obtained. This means that much room is left to improve targeting by refining poverty information within small sampling units.

Appendix B. Simulation of the performance of reconstructed ranks

To illustrate the effectiveness of the method, we conduct a simulation analysis that loosely mimics our empirical setting. The policy maker wishes to rank a sample of N individuals residing in the same locality. To this effect, B local observers are asked to rank the N individuals by income – which, as discussed above, is equivalent to ranking each pair of individuals in the locality.[35] We set $N = 30$ and $B = 9$. The number of distinct ij pairs is thus $N(N-1)/2 = 435$. True ranks r_{ij} are set by drawing, for each individual i, a log income y_i from a standard normal distribution with unit variance.[36] We generate 100 different localities for each simulation; they can be understood as replications of the data generating process (DGP).

Each local observer only knows a random subset S of the N individuals, with S taking values $\{0.7, 0.5, 0.3, 0.1\}$. For instance, $S = 0.5$ means that an observer asked to rank individuals i and j knows each of them with probability 0.5 – and is thus capable of ranking the ij pair with probability 0.25. Each of the nine observers thus ranks on average a quarter of the individuals pairs in their locality. Since there are $B = 9$ observers, this generates overlap in rankings across ij pairs, making it unlikely that no ranking is reported on any particular pair. In contrast, when $S = 0.1$, each observer ranks any ij pair with a 1 percent probability – i.e., provides on average 4.35 pairwise rankings. The 9 observers thus collectively provide at most 39 distinct rankings on average or 9% if the total number of 435 ij pairs. It follows that, when $S = 0.1$, directly elicited pairwise rankings do not rank all individuals in the locality – and our reconstruction method comes to the fore. We then combine these partial rankings r^k_{ij} to calculate reconstructed ranks P_i as explained above. Un-ranked individuals end up in the middle of the reconstructed distribution with $P_i = n/2$.

[35]For ease of interpretation, we set the B observers to be distinct from the N target individuals.

[36]The log-normal assumption guarantees that income is positive and mimics the actual distribution of income in many populations.

Simulation results are presented in Tables A5 and A6. The first row, first column of the Table focus on the individual reports r_{ij}^k without observation error. As anticipated, when $S = 70\%$ the proportion of pairs on which we have a report is 49.06% $\approx 0.7^2$. This proportion falls to 24.95% when $S = 50\%$, 8.88% when $S = 30\%$, and 1.02% when $S = 10\%$. There is considerable variation around this average across the 100 simulated localities. In Table S1 the variance of observation errors is 0, which implies that ranked pairs are correctly ranked.

The second parts of Tables A5 and A6 summarize our results regarding our main object of interest, namely, the reconstructed ranks $\hat{r}_i = P_i$. These ranks are constructed by sorting all individuals according to their relative position index P_i: individuals i is estimated to be ranked higher than j if $P_i > P_j$. When reconstructed ranks are not complete, $P_i = P_j$ for some ij and individuals i and j cannot be ranked relative to each other. Having sorted individuals according to their P_i value, we examine what proportion of the *consecutive* pairs are unranked. This measures the completeness of the reconstructed ranks. Since each locality has 30 individuals, there are 29 consecutive sorted pairs $\{\hat{r}_i, \hat{r}_{i+1}\}$. From Table A5 we see that, when $S = 70\%$, the proportion of missing consecutive pairs is small: 0.41%. This proportion increases rapidly as S falls: it rises to 3.93% when $S = 50\%$, 16% when $S = 30\%$ and 60% when $S = 10\%$ (Table A6). We also note the presence of considerable variation across replications, especially at lower values of S. This is because, when S is small, the number of reports is very small – vanishingly so in some replications.

Conditional on a consecutive pair $\{i, i+1\}$ being ranked relative to each other (i.e., $P_i \neq P_{i+1}$), the reconstructed ranking often agrees with the true ranking, in the sense that $r_i > r_{i+1}$ if and only if $P_i > P_{i+1}$. The proportion of correct reconstructed ranks, however, falls rapidly with S. Furthermore, since randomly assigning ranks between two individual pairs results in a correct ranking half of the time, we see that the predictive power of the method falls rapidly with S.

Combining the loss of information due to erroneous and missing rankings, we find that when $S = 70\%$, our method produces a correct ranking for 99.55% of the consecutive pairs. This confirms that the method proposed here can work in the sense of producing a close approximation of the true ranking of target individuals by income level in each given location. The performance of the method, however, deteriorates rapidly when S falls, that is, when observers know a smaller proportion of the target individuals. In particular, when each observer only knows 10% of the target individuals, i.e., 3 individuals on average, the reconstructed rankings only match 25.93% of the true rankings. We also note that there is considerable variation in the performance of the method across replications, suggesting that, in some cases, it can still yield valuable results even when S is low.

Next we investigate the role played by averaging by introducing a observation error term e_i^k to the income of individual i that is observed by k. This error enters the model as $log(y_i) + log(e_i^k)$ where $log(e_i^k)$ is normally distributed with mean 0 and a variance σ_e^2. The variance of $log(y_i) = 1$. In Tables A5 and A6, we then report simulation results for values of σ_e^2 between 0.1 and 0.9 in columns 2-5. The proportion of correctly ranked pairs is close to the theoretical mean of 0.49, with some variation around that mean. The next part of the Table shows the proportion of observer reports that correctly rank ij pairs. As expected, this proportion falls as σ_e^2 increases, with a little variation across locations. But the majority of pairs remain correctly ranked.

The rest of the table shows the effect of observation error on reconstructed ranks P_i. We first look at the proportion of missing reconstructed rankings, that is, the proportion of ij pairs such that $P_i = P_j$. Equal reconstructed ranks arise for two reasons: (1) because the income of individual i, say, was not observed by any of the B observers and hence no report was given that involves that individual who therefore get $P_i = n/2$; or (2) because the reconstructed graph $\hat{M}$ is either incomplete (e.g., with multiple branches) or contains one or more directed cycles. Simulation results (not shown here) indicate that the first channel has a severe effect only when $S = 10\%$: in this case, around 38-39% of individuals are not ranked at all. But observation error does not affect this proportion since it does not increase the frequency of missing pairwise *reports* r_{ij}^k.[37] Hence the effect of observation error on missing ranks comes entirely from the way it degrades the reconstructed graph. Turning to this channel, we know that when $\hat{M}$ contains two or more branches, it is possible for two individuals to share the same difference $r_i^{down} - r_i^{up} = r_j^{down} - r_j^{up}$ even if $r_i^{down} \neq r_j^{down}$. In this case i and j have the same reconstructed rank P_i even though they sit on separate branches of $\hat{M}$. In addition, when the reconstructed ranking graph contains a directed cycle involving L individuals, all these L individuals share the same values of r^{down} and r^{up} – and thus the same value of the reconstructed index P. Since observation error makes directed cycles more common and can introduce multiple branches in $\hat{M}$, it increases the proportion of missing reconstructed rankings. This effect, however, can be partly compensated by the averaging effect of multiple reports on the same ij pair: even though individual reports may be distorted by error, their average may still be correct since the mean observation error $E[e_i - e_j]$ on $y_i - y_j$ tends to 0 as $kS \to \infty$. Based on this, we expect the mitigating effect to be stronger when S is larger. But it is unclear how much of a mitigating effect averaging has.

We show that the degrading effect of observation error on missing ranks is dramatic: even when $S = 70\%$, the proportion of missing reconstructed ranks rises to 83.48% when $\sigma_e^2 = 0.9$. This arises even though the large value of S implies a relative large number of reports r_{ij}^k on the same ij pair. This means that, within the parameters of our simulation exercise (and the context of our data collection), averaging is unlikely to help much. We also see that there is a lot of variation in the proportion of missing ranks across localities/replications.

Next, the Tables A5 and A6 shows the proportion of correctly reconstructed consecutive ranks, as a fraction of non-missing consecutive ranks. Here the picture is more encouraging: non-missing ranks do, in their majority (76 to 87%), agree with the true ranks. Again this varies a lot across replications. The bottom of the Table combines the two to document the effect of observation error on the proportion of correctly ranked consecutive pairs. The Table shows that, even with $S = 70\%$, this proportion falls rapidly from 74.83 to 12.97% is σ_e^2 increases.

[37] For other values of S in our simulations, the proportion of totally unranked individuals is never more than 3%., irrespective of observation error. The fact that the size of the giant component in the reconstructed graph abruptly falls when S falls below a threshold is a well-known phenomenon in network analysis (e.g., Jackson 2010).

We repeat the same exercise for $S = 50, 30$ and 10%. The same pattern is by and large reproduced: a very gradual decline in the proportion of correct reports; a rapid increase in the proportion of missing reconstructed ranks; and a relative stability of the proportion of correct reconstructed ranks with respect to observation error. All in all, we note a very rapid deterioration in the performance of our rank reconstruction method as observation error increases.

Things are different regarding the proportion of missing ranks: it was already 60% when $\sigma_e^2 = 0$; but it does not increase further with observation error. We also find that the proportion of correct ranks is quite low (its expected value under pure random guess is 50%) – but it is relatively constant. As a result, the proportion of correctly reconstructed ranks under $S = 10\%$ is fairly constant and insensitive to observation error. This arises for the same reason that the reconstructed graph $\hat{M}$ contains a lot of unconnected nodes when $S = 10\%$: being very sparse, the graph $\hat{M}$ contains few branches and nearly no cycles (e.g., Jackson 2010), thereby ruling out the two main sources of missing ranks in the better connected $\hat{M}$ generated when $S \geq 30\%$.

The Tables A5 and A6 have focused on a specific section of the reconstructed ranks, namely, the consecutive pairs. We now broaden our focus to include all ij pairs, whether or not they are consecutive in the reconstructed ranks. We expect reconstructed ranks to be more accurate for ij pairs that are far apart in the true ranks, since the larger difference between their incomes is more likely to survive observation error. Results are presented in Figures 6. Each Sub-Figure shows,for a particular value of S, the proportion of correctly ranked ij pairs as a function of the distance in their true ranks $r_j - r_i$. Each line corresponds to a different value of σ_e^2.

Starting with the top-left sub-Figure, we immediately see that, in the absence of observation error, the constructed P_i index correctly rank ij pairs irrespective of the distance in their true ranks. When observation error is added, we find, as expected, that the P_i index is more likely to correctly rank distant ij pairs than pairs with a more similar income. This contrast remains as σ_e^2 is increased from 0.1 to 0.9, at which point, as we have seen in Table A5, most pairwise ranks are not correctly reconstructed – primarily because of missing ranks. As we move to lower values of S in Figure 6, the proportion of correctly ranked pairs initially falls with S – but then it rises somewhat at high values of σ_e^2, as already observed in the simulation Tables. This may be due to the fact that, with large observation errors, the abundance of reports when $S = 50\%$ increases the likelihood of cycles. With $S = 30\%$, the reconstructed graphs are more sparse and, as a result, are less likely to contain cycles. The bottom right sub-Figure confirms the very different pattern observed in the Tables with $S = 10\%$. Here the likelihood of cycles is low and index P_i is more able to identify correct ranks at high levels of observation error – as already noted.

To summarize, these simulations have demonstrated that index P_i is capable of identifying true ranks with high accuracy in situations where a sufficiently large number of reports are provided by observers, and when these reports are not affected too much by observation error. The performance of the index nonetheless deteriorates when S falls, and especially when σ_e^2 increases. The deterioration in performance due to a lower S arises because of an increase in the proportion of unranked pairs; the deterioration associated with observation error arises because of the creation of branches and, especially, cycles in the reconstructed graphs. It may be possible to improve the performance of the method by penalizing cycle formation, but this is left for future research.

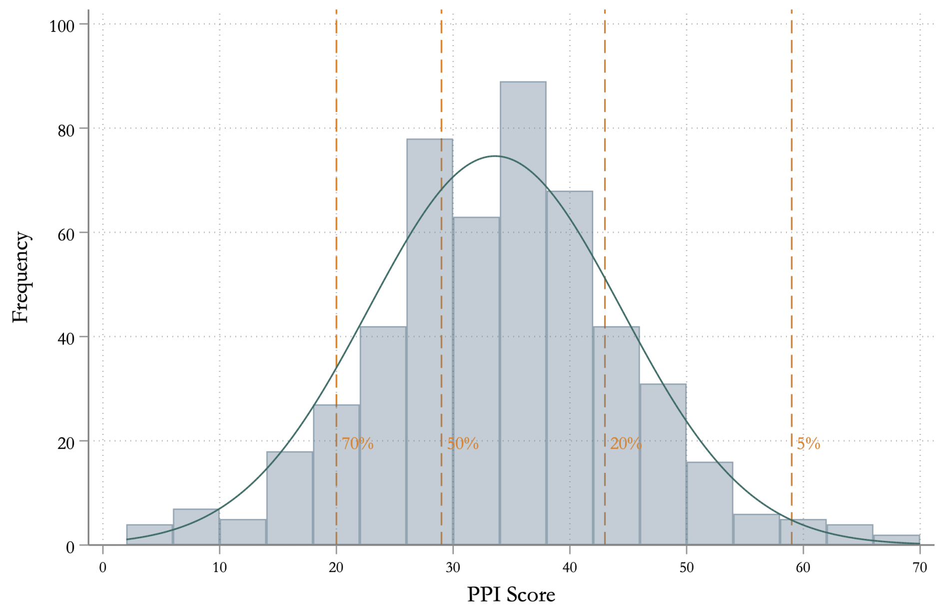
Figures

Figure 1: Sampling Areas

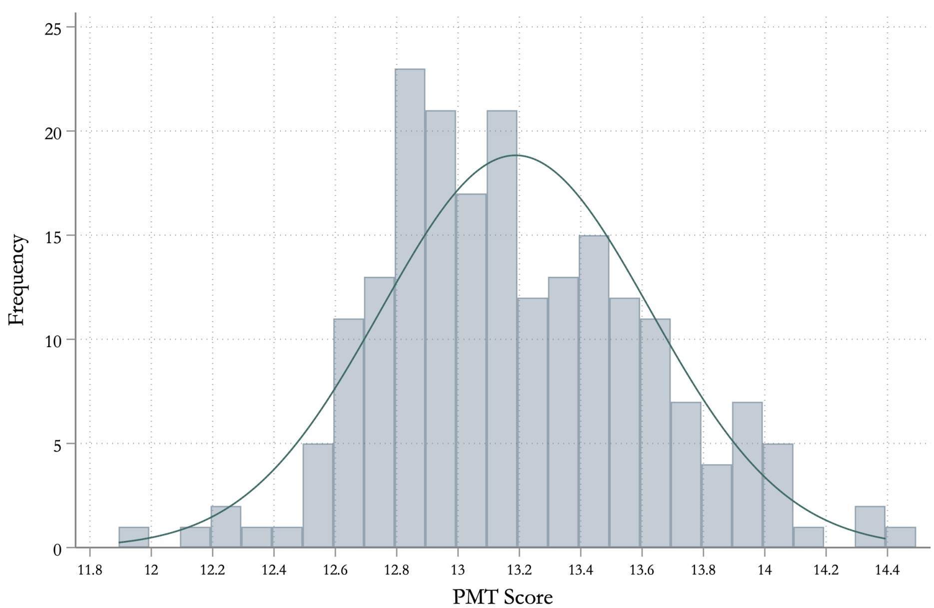
Notes: Enumeration areas selected for the ranking study are indicated in blue.

Figure 2: Poverty Measures Distribution

Poverty Probability Index PPI Distribution in Our Sample

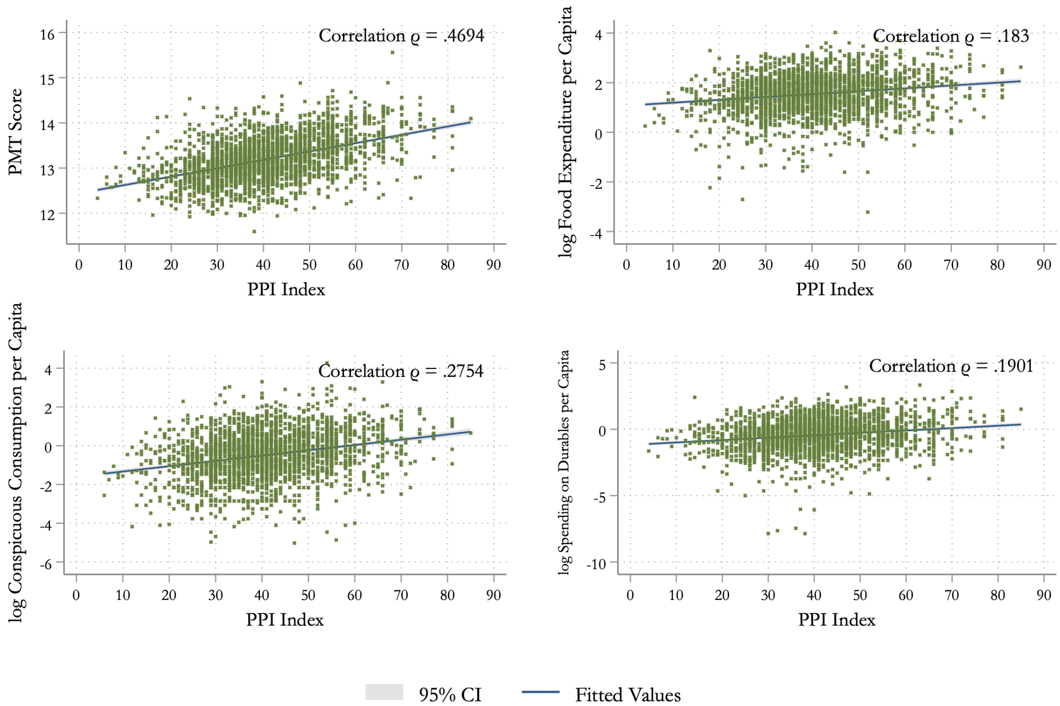


PMT Score in Our Sample



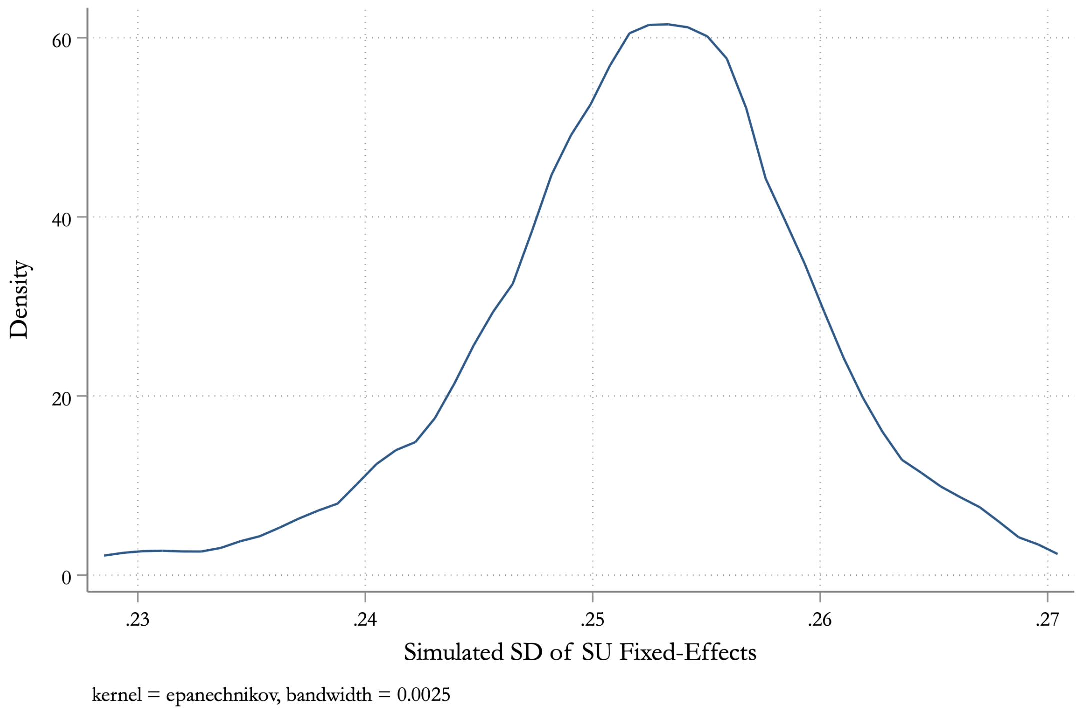
Notes: We plot the distribution of the PPI and PMT indexes. In the top figure, we compute PPI in our sample, as built by Innovations for Poverty Action (IPA) in April 2018 using Côte d'Ivoire's 2015 Enquête sur le Niveau de Vie des Ménages Survey. The "Poverty Likelihood", i.e., the probability to be below the National Poverty Line is indicated in orange. The bottom figure indicates the PMT score developed by the Ivorian Government. The two indexes are described in more details in Section 3.3.

Figure 3: Correlation across Poverty Measures



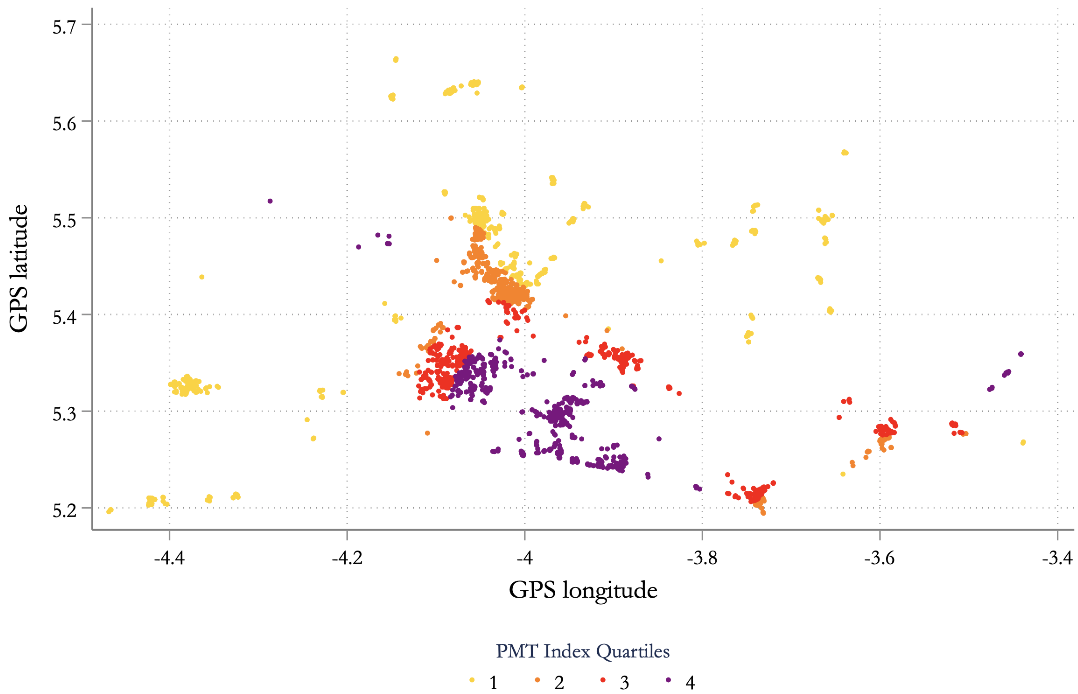
Notes: Correlation across the three measures of poverty as described in Section 3.3. We used the full AUDRI study sample, no matter whether they participated in the ranking exercise or not, in order to maximize the number of observations.

Figure 4: Distribution of the Standard Deviations of SU-Fixed Effects



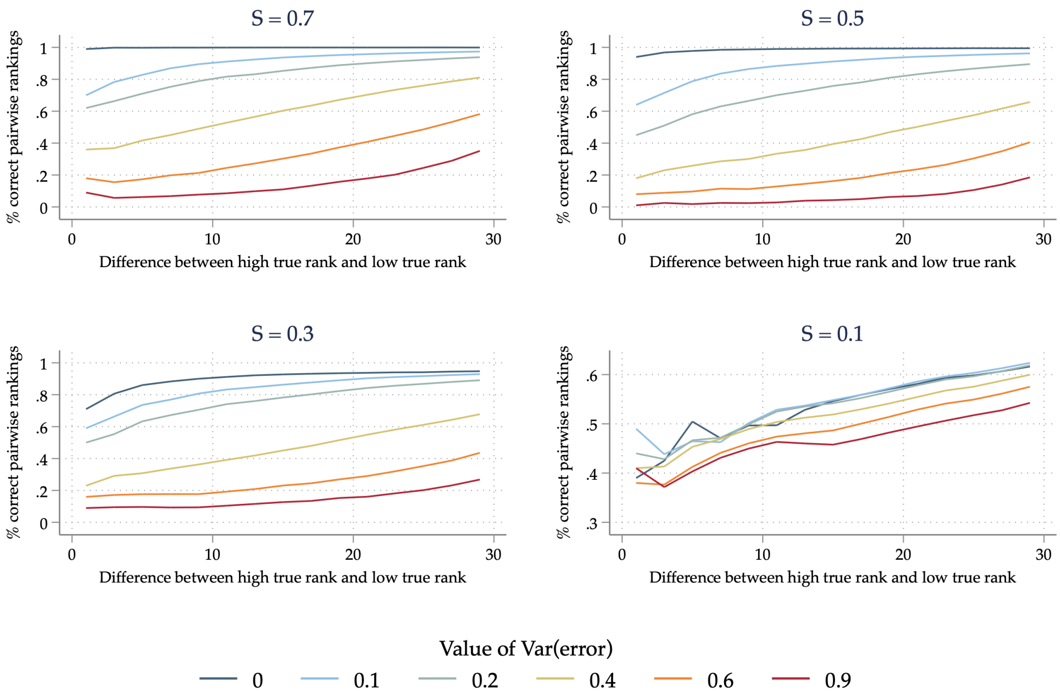
Notes: To obtain the distribution of SD of SU fixed effects, we bootstrap the distribution of estimated SU fixed effects that would be obtained under the null hypothesis of no SU fixed effects. For that, we scramble the observed values of the PMT but keep the division of the sample into sampling units of the original size. We then regress the PMT index on SU fixed effects and calculate the standard deviation of the 635 fixed effects. We are able to simulate the standard deviation of SU fixed effects under the null of zero systematic variation in the PMT index across SU's by repeating this process multiple times.

Figure 5: Spatial Predictions of the PMT Index in and around the City of Abidjan



Notes: We fit a two-way kernel regression in (decimal) latitude and longitude on the PMT index. To do so, we use the *npregress* command in Stata with an Epanechnikov kernel and an optimal bandwidth. The idea behind the approach is that average poverty varies relatively smoothly across space in and around the city. It is intended to capture the way a knowledgeable city planner would form a mental representation of the spatial distribution of affluent and poor neighborhoods – and may target anti-poverty interventions on that basis. Each color represents a prediction quartile.

Figure 6: Index Accuracy by Difference in True Ranks



Notes: Each line is a fitted fractional polynomial of % of correctly predicted pairwise comparisons. We show four graphs with different values of S, the random subset the individual knows.

Tables

Table 1: Respondents' Types

Ranking respondent	**#**	**%**	**% of Household Head**	**% of Women**
Households from the listing only - 200's	88	17.36 %	38.64%	69.32%
Resp. from the indiv. survey - 200's	119	23.47%	47.06%	56.30%
Resp. selected on the spot - 300's	230	45.36%	46.52%	46.96%
Key informants - 900's	70	13.81%	25.71%	71.43%
Total	**507**	**100%**	**42.41%**	**56.41%**

Table 2: Summary Statistics - Poverty Measures

	(1) Urban	(2) Rural
Consumption Expenditures		
Value of food expenditure in the last week	15.43	13.17
	(8.12)	(7.20)
Value of conspicuous expenditures in the last month	2.47	2.59
	(3.29)	(2.74)
- Communication expenditures	1.09	1.00
	(1.25)	(1.39)
- Entertainment (concert, bar, cinema, games) expenditures	0.22	0.29
	(1.55)	(0.91)
- Beauty products/hairdresser expenditures	0.48	0.75
	(0.79)	(1.40)
- Charitable expenditures	0.67	0.55
	(2.38)	(1.18)
Spending on durables in the last 12 months	2.34	2.10
	(3.00)	(2.38)
- Clothes/shoes HH expenditures	1.10	1.34
	(1.09)	(1.48)
- Furniture HH expenditures	0.39	0.27
	(1.16)	(0.84)
- School fees HH expenditures	0.89	0.52
	(2.80)	(1.35)
Value of food expenditure in the last week per capita	3.76	3.74
	(2.89)	(3.39)
Spending on durables in the last 12 months per capita	0.53	0.61
	(0.67)	(1.46)
Indexes		
PPI Index	37.13	28.70
	(9.55)	(10.64)
Score PMT	13.23	13.13
	(0.43)	(0.44)
Other variables		
HH's head unemployed or inactive	0.20	0.15
	(0.40)	(0.36)
# of mobile phones per capita	0.81	0.82
	(0.47)	(0.48)
Observations	294	213

Notes: Consumption expenditures are *all* normalized per week and in 1,000 FCFA

Table 3: How do Respondents Think About Poverty? Summary Statistics from Survey Data

	(1) Share of respondents
Uncertain about their ranking	0.09
	(0.29)
Own poverty's perceptions	
Consider their household to be poor	0.29
	(0.45)
Consider their household to be poorer than neighbors	0.21
	(0.41)
Think that other households consider their household to be poor	0.21
	(0.41)
Criteria used to classify	
Household expressed their financial problems	0.49
	(0.50)
Household members' health	0.14
	(0.35)
Household head's occupation	0.49
	(0.50)
Households' daily number of meals	0.19
	(0.39)
Household children's school enrollment	0.07
	(0.26)
Respondents' own definition of poverty	
Food deprivations	0.80
	(0.40)
No decent housing	0.31
	(0.46)
Unresolved health problems	0.43
	(0.49)
No proper toilet/bathroom	0.16
	(0.37)
Knowledge about neighbors	
# of neighbors listed in total	5.75
	(1.47)
% of neighbors they regularly visit	0.56
	(0.38)
% of neighbors receive health/money advice from	0.44
	(0.39)
% of neighbors they'd ask money from	0.38
	(0.38)
Observations	507

Notes: Survey data collected early March 2020. Definition of poverty manually entered by the enumerators and re-classified by the research team.

Table 4: Relative Rank vs. Relative Survey-Measured Poverty: Pair-Level

Difference j - i in:	(1) Food exp per ca	(2) Months food short	(3) PMT Score	(4) PPI Index
Reported rank	-0.053	-0.491	0.054	0.949
	(0.397)	(0.412)	(0.061)	(1.137)
log(distance)	0.067	0.049	0.016	0.031
	(0.071)	(0.076)	(0.011)	(0.245)
Constant	0.095	0.127	-0.120*	-1.230
	(0.476)	(0.468)	(0.066)	(1.347)
R2	0.002	0.004	0.006	0.004
Observations	710	663	432	771
Constructed rank	-0.523*	-0.909***	0.112**	0.431
	(0.287)	(0.320)	(0.048)	(0.839)
log(distance)	0.018	-0.009	0.002	0.067
	(0.065)	(0.065)	(0.011)	(0.213)
Constant	0.489	0.844**	-0.108	-1.567
	(0.434)	(0.428)	(0.066)	(1.199)
R2	0.006	0.011	0.011	0.006
Observations	973	910	550	1057
Diff in HodgeRank	-0.425	-1.295***	0.117	2.374*
between j and i	(0.468)	(0.480)	(0.092)	(1.378)
log(distance)	0.014	-0.001	-0.001	-0.136*
	(0.024)	(0.021)	(0.006)	(0.073)
Constant	0.182	0.416***	-0.025	-1.043**
	(0.163)	(0.148)	(0.035)	(0.470)
R2	0.000	0.003	0.001	0.001
Observations	4579	4436	1625	5590

Notes: We report three separate regressions for each constructed variable of aggregated rankings, as described in Section 5. The reported rank variable is the share of reported ranks showing j richer than i. The constructed rank variable is a dummy equals to 1 if j is ranked richer than i (including the case where it is both). The difference in HodgeRank scores between j and i is used as an independent variable: the higher the difference, the richer j is ranked compared to i, following the HodgeRank algorithm described in part 2. A negative difference would indicate that j is ranked poorer than i. All dependent variables are differences in consumption variables. They are calculated as the value for household j minus the value for household i. The complete list of dependent variables is given below. For consumption variables, a positive difference means that i is poorer than j. We also display the PMT and the PPI indexes. Robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All dependent variables are the difference in values between households i and j and described the following:
Food expenditures: Total of consumption expenditures collected with a one week recall period: staples, meat, vegetables, fruits, drinks, alcohol. *Months food short:* Number of months the household experienced a food shortage over the last twelve months. PPI and PMT Scores are wealth measures, computed following the methodology described in Section 3.3.

Table 5: Aggregate Rank vs. Survey-Measured Poverty Level : Individual Level

Levels of:	(1) Food exp per ca	(2) Months food short	(3) PMT Score	(4) PPI Index
Relative Position	-0.007	-0.024	0.008	-0.008
	(0.036)	(0.039)	(0.007)	(0.128)
log(distance)	-0.064	-0.078	0.013	-0.387*
	(0.054)	(0.056)	(0.013)	(0.210)
Constant	4.081***	1.955***	13.072***	35.651***
	(0.353)	(0.358)	(0.077)	(1.282)
R2	0.004	0.004	0.048	0.008
Observations	474	464	207	474

HodgeRank score	-0.112	-1.028	0.230	1.317
	(1.697)	(1.476)	(0.225)	(5.329)
log(distance)	-0.064	-0.078	0.013	-0.389*
	(0.054)	(0.056)	(0.014)	(0.210)
Constant	4.081***	1.949***	13.071***	35.663***
	(0.354)	(0.357)	(0.078)	(1.284)
R2	0.003	0.005	0.044	0.008
Observations	474	464	207	474

Notes: We report two separate regressions for each constructed variable of aggregated rankings, as described in Section 5. Only the surveyed respondents for which we recovered a rank are included in this regression. The constructed rank difference is the difference between the number of nodes that, in the constructed ranking graph, i looks down towards, and the number of nodes that i looks up towards. The difference is 0 when everyone is in a circle, meaning no one is ranked above anyone else. The HodgeRank scores follow the HodgeRank algorithm described in part 2. A high score means the individual is ranked richer.
All dependent variables are levels of consumption variables. The complete list of dependent variables is given below. Robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Outcomes: *Food expenditures:* Total of consumption expenditures collected with a one week recall period: staples, meat, vegetables, fruits, drinks, alcohol. *Months food short:* Number of months the household experienced a food shortage over the last twelve months. PPI and PMT Scores are wealth measures, computed following the methodology described in Section 3.3.

Table 6: Predictors of rankings: Pairwise comparisons

	(1)	(2)	(3)
	Dep. var. =1 if resp. k reported that i is poorer than j		
	OLS	OLS	OLS
Indep. Variables: Differences between j and i in:			
PPI Index	0.002	0.001	0.003**
	(0.001)	(0.001)	(0.001)
HH's head unemployed or inactive	-0.035	-0.038	-0.033
	(0.029)	(0.029)	(0.031)
Household Size	0.005	0.013**	0.005
	(0.004)	(0.005)	(0.005)
Value of food expenditure in the last week per capita		0.010***	
		(0.004)	
Value of conspicuous consumption expenditures in the last month		-0.006	
		(0.004)	
Spending on durables in the last 12mo per capita		0.018	
		(0.019)	
Received gifted food last week (yes=1)			-0.118***
			(0.043)
Food worries during last 12mo (yes=1)			-0.044
			(0.034)
Months with food shortages last 12mo			-0.004
			(0.006)
Days with skipped meals last 3mo			0.000
			(0.001)
Improvement in food situation last year (1 to 5)			-0.029*
			(0.017)
log(distance from k to i)	0.011	0.012	0.004
	(0.011)	(0.011)	(0.011)
Semi-Rural EA	-0.006	0.010	-0.021
	(0.040)	(0.041)	(0.044)
Constant	0.515***	0.490***	0.537***
	(0.047)	(0.047)	(0.048)
R2	0.009	0.020	0.030
Observations	887	887	813

Notes: The unit of observation at the triad level. The outcome variable is a dummy equal to 1 if k ranked j poorer than i, 0 otherwise. It is missing if no ranked were assigned. Pairs i-j involving the respondent k are dropped. The three columnns contain different types of predictors, e.g., assets and expenditures, and assets and experienced poverty. The predictors are all differences between j and i. Missing distance is replaced by the average distance in the EA and we control for such a case in the regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Determinants of Ranking Accuracy

	(1)	(2)	(3)
	Ranking Accuracy compared to survey measure:		
	PMT	PPI	Food expenditure per capita
Respondent: Woman	0.047	-0.082	0.088*
	(0.054)	(0.053)	(0.051)
Respondent: Migrant	0.024	0.002	-0.065
	(0.059)	(0.057)	(0.055)
Respondent: Non-Ivorian	0.036	-0.008	-0.020
	(0.058)	(0.059)	(0.052)
Respondent: Key Informant	0.065	0.068	-0.054
	(0.084)	(0.094)	(0.065)
Respondent: Household Head	0.038	-0.040	0.094*
	(0.055)	(0.054)	(0.053)
Semi-Rural EA	-0.040	0.027	0.040
	(0.053)	(0.050)	(0.050)
PPI Index	0.000	0.004	0.002
	(0.002)	(0.002)	(0.002)
Value of food expenditure in the last week per capita	0.015*	0.003	0.013**
	(0.007)	(0.008)	(0.006)
Asked to self rank	0.043	0.020	0.045
	(0.044)	(0.043)	(0.040)
# of neighbors listed in total	-0.010	0.013	0.003
	(0.011)	(0.011)	(0.010)
Household Size	0.015*	-0.000	0.026***
	(0.008)	(0.008)	(0.007)
Constant	0.391***	0.351***	0.112
	(0.136)	(0.135)	(0.115)
R2	0.031	0.033	0.071
Observations	294	287	294
Sample Mean of Ranking Accuracy	0.535	0.521	0.511

Notes: Regression of the propensity to rank accurately two respondents according to their respective indexes. The number of observations is the number of respondents for whom we could obtain the accuracy measures (i.e., they ranked enough neighbors within our sample). The number of observations for the PPI is lower since accuracy is missing when two ranked neighbors had the exact same index (which happened more often for PPI than for the PMT/food expenditure that are more precise indexes). Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. M

Table 8: Poverty Targeting: Can peer rankings identify those below the median?

	Dep. Var: Ranked below the median according to:		
	PMT	PPI	Food Expenditure per capita
Panel A. Indep. Var: Ranked below the median (aggregate ranking)			
Below median	0.101	0.032	-0.033
	(0.076)	(0.054)	(0.054)
Constant	0.413***	0.425***	0.490***
	(0.045)	(0.031)	(0.031)
Observations	189	390	390
Panel B. Indep. Var: Ranked below the median (HodgeRank score)			
Below median	0.132*	0.007	-0.051
	(0.073)	(0.051)	(0.051)
Constant	0.393***	0.433***	0.502***
	(0.047)	(0.034)	(0.034)
Observations	189	390	390
Panel C. Indep. Var: Unranked			
Unranked	0.167	-0.023	0.035
	(0.121)	(0.052)	(0.053)
Constant	0.444***	0.459***	0.469***
	(0.036)	(0.025)	(0.025)
Observations	207	507	507

Notes: In the first two panels, we create a dummy equal to 1 if the aggregate ranking puts the household below the median of its EA. It is zero if the household is ranked at or above the median and missing if the household is not ranked. We use two separate constructed variable of aggregated rankings: (1) the relative position of individual i in the constructed network, i.e., how many people can be ranked as poorer than i and subtracts how many can be ranked richer. (2) the HodgeRank algorithm assigns a score to each households, described in part 2. Only the individuals ranked by at least one other respondent are considered.
We run OLS regressions of the dummy on a dummy for whether the household is below the median based on the survey measure (PMT in column 1, PPI in column 2, and the food expenditure per capita in column 3). The table reads as follows: individuals ranked below the median in the aggregate peer ranking are 10.1 percentage points more likely to be indeed below the median of the PMT score distribution (Column 1). In the bottom panel, the dependent variable is a dummy equal to 1 if the household was ranked by no one.
Robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Poverty Targeting: Predict being ranked below median- No Self-Rank

	Dep. Var: Ranked below the median according to:		
	PMT	PPI	Food Expenditure per capita
Panel A. Indep. Var: Ranked below the median (aggregate ranking)			
Below median	0.127	0.101	0.020
	(0.081)	(0.067)	(0.067)
Constant	0.406***	0.410***	0.458***
	(0.049)	(0.041)	(0.042)
Observations	161	236	236
Panel B. Indep. Var: Ranked below the median (HodgeRank score)			
Below median	0.169**	0.058	-0.007
	(0.079)	(0.065)	(0.066)
Constant	0.383***	0.423***	0.469***
	(0.050)	(0.044)	(0.044)
Observations	161	236	236
Panel C. Indep. Var: Unranked			
Unranked	0.081	0.048	0.061
	(0.084)	(0.044)	(0.044)
Constant	0.441***	0.428***	0.445***
	(0.039)	(0.032)	(0.032)
Observations	207	507	507

Notes: See Table 8 notes.
Robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Testing for Self-Ranking Bias

	(1)	(2)
S_{ij}^{k}	-0.281***	-0.283***
	(0.031)	(0.031)
Constant	0.518***	0.515***
	(0.004)	(0.004)
R2	0.136	0.139
Observations	1298	1337
Number of ij pairs	704	732

Notes: The dependent variable is 1 if the respondent k reports that i is poorer than j, 0 if i is richer, and missing if k does not rank i and j. Variable S_{ij}^{k} is 1 if k=i and -1 if k=j, and 0 if k is not i or j. Column 1 only uses the 30 EAs without sampling issues / Column 2 uses all 34 EAs. Observations from the no-self-ranking treatment are omitted since they contain no useful information. Including them anyway produces identical results. Fixed effects are include for each (i,j) pair. Robust standard errors are provided in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Predictors of propensity to rank others

	(1) OLS	(2) OLS
log(distance from k to i)	-0.017***	-0.017***
	(0.001)	(0.001)
Semi-Rural EA	-0.036***	-0.044***
	(0.008)	(0.008)
Respondent k: Key Informant	-0.048***	-0.048***
	(0.010)	(0.010)
Value for i of the following var:		
PPI Index	-0.002***	-0.001**
	(0.000)	(0.001)
Household Size	-0.003*	-0.003
	(0.002)	(0.002)
HH's head unemployed or inactive	0.013	0.023*
	(0.012)	(0.012)
Respondent: Non-Ivorian	0.010	0.026*
	(0.013)	(0.014)
Respondent: Migrant	-0.020	-0.022*
	(0.013)	(0.013)
Respondent: Woman	-0.014	-0.011
	(0.010)	(0.010)
Received gifted food last week (yes=1)		0.007
		(0.016)
Food worries during last 12mo (yes=1)		-0.018*
		(0.010)
Value of food expenditure in the last week per capita		-0.001
		(0.002)
Value of conspicuous expenditures in the last month		0.005***
		(0.002)
Spending on durables in the last 12 months per capita		0.002
		(0.005)
Value for i - Value for k of the following var:		
PPI Index	0.001***	0.002***
	(0.000)	(0.000)
Household Size	0.002*	0.001
	(0.001)	(0.001)
HH's head unemployed or inactive	0.004	-0.001
	(0.009)	(0.009)
Respondent: Non-Ivorian	0.006	-0.001
	(0.009)	(0.010)
Respondent: Migrant	0.005	0.008
	(0.009)	(0.009)
Respondent: Woman	0.009	0.004
	(0.007)	(0.007)
Received gifted food last week (yes=1)		0.014
		(0.012)
Food worries during last 12mo (yes=1)		0.017**
		(0.007)
Value of food expenditure in the last week per capita		-0.002
		(0.001)
Value of conspicuous consumption expenditures in the last month		-0.001
		(0.001)
Spending on durables in the last 12mo per capita		-0.001
		(0.004)
Constant	0.290***	0.269***
	(0.024)	(0.026)
R2	0.042	0.050
Observations	6835	6620

Notes: The unit of observation at the dyad level. The outcome variable is a dummy equal to 1 if k report a ranking for the individual i, 0 otherwise. Pairs i-k involving the respondent k are dropped. Missing distance is replaced by the average distance in the EA and we control for such a case in the regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A1: PPI – Scorecard Côte d'Ivoire 2015 National Poverty Line

Indicators	**Responses**	**Points (National Poverty Line)**
1. In which district does this household reside?	A. Abidjan	7
	B. Yamoussoukro	5
	C. Bas-Sassandra	9
	D. Comoé	4
	E. Denguélé	0
	F. Gôh-Djiboua	3
	G. Lacs	3
	H. Lagunes	2
	I. Montagnes	5
	J. Marahoué	0
	K. Savanes	2
	L. Vallée du Bandama	2
	M. Woroba	4
	N. Zanzan	4
2. How many members does the household have?	A. Three or less	17
	B. Four or more	0
3. What is the highest educational level that the household head has completed?	A. None	0
	B. Primary	4
	C. Secondary	5
	D. Higher	12
4. Did all children aged 6 to 16 attend school this school year?	A. There are no children aged 6 to 16	11
	B. All children aged 6 to 16 attended school this year	7
	C. At least one child aged 6 to 16 did not attend school this year	0
5. What is the mode of water supply?	A. Tap water in the dwelling	10
	B. Tap water in the yard	4
	C. Tap water outside of the property	4
	D. Well in the yard	1
	E. Public well	2
	F. Village pump	2
	G. Surface water (creek, river, etc.) or other	0
6. What type of toilet do you use?	A. W-C inside	7
	B. W-C outside	6
	C. Latrines in the yard	5
	D. Latrines out of the yard	5
	E. In nature (no toilet) or other	0
7. Where do you take your shower?	A. Outside	0
	B. Rudimentary shower	3
	C. Bathroom	9
	D. Other	1
8. Did the household own a moped, car or van in good working order in the last 3 months?	A. The household owns a car or van	15
	B. The household owns a moped and does not own a car or van	9
	C. None	0
9. Did the household own a fan in good working order in the last 3 months?	A. Yes	6
	B. No	0
10. Did the household own a bed in good working order in the last 3 months?	A. Yes	4
	B. No	0
	PPI Score	SUM

Table A2: Poverty Predictions Models: Proxy-Means Test (PMT) and Poverty Probability Index (PPI)

	PMT Score					PPI Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log(per capita conso)	Rural - Gov	Urban - Gov	Tot - Gov	Tot - Ranking	Tot - AUDRI	Tot - Ranking	Tot - AUDRI
R^2	0.497	0.612	0.568	0.563	0.491	0.484	0.446
Observations	7,076	5,748	12,773	193	2,871	493	2,666

The table reports the R^2 and the number of observations from the regressions run by the government of Côte d'Ivoire to build their PMT score (columns 1, 2, 3). The numbers were shared to us by the CNAM in Côte d'Ivoire. The government regressed log(food expenditure per capita) on the variables used to build the PMT score. In Column 4, we report the R^2 from the same regression run on the data for households involved in the ranking exercise. Column (5) reports the R^2 from the same regression run on the data for households involved in the full AUDRI sample. Column (6) reports the fit from the PPI regression, i.e., regressing log(food consumption per capita) on the variables used to build the PPI index. Column (7) reports the latter PPI regression on the full AUDRI sample. Note that the sample size is not exactly the same between columns (5) and (7) due to differential missing patterns between variables used in the PMT vs. the PPI score.

Table A3: Reconstructed aggregate rankings

Number of ranked households who are richer or poorer than the target household																				
EA 2	**richer**	**poorer**	**EA 4**	**richer**	**poorer**	**EA 6**	**richer**	**poorer**	**EA 7**	**richer**	**poorer**	**EA 8**	**richer**	**poorer**	**EA 9**	**richer**	**poorer**	**EA 12**	**richer**	**poorer**
201	2	11	201	0	9	201	10	10	201	10	2	201	0	5	205	4	4	203	4	1
202	1	12	202	5	3	202	13	0	202	10	11	203	1	5	208	0	6	204	1	2
203	0	13	203	3	5	203	10	10	203	10	11	207	1	5	209	5	0	205	5	0
204	10	10	204	2	7	206	10	10	204	10	11	209	2	5	210	3	5	305	0	3
205	3	10	205	5	0	207	12	2	205	11	1	211	12	5	211	2	2	307	0	2
206	10	10	206	6	0	208	11	3	206	10	11	212	11	2	212	1	7	308	0	2
208	10	10	207	1	8	209	13	0	207	10	11	213	10	3	213	8	1			
209	10	10	209	7	0	210	10	10	208	1	11	214	8	4	301	1	0			
210	12	1	210	6	1	211	2	11	212	12	0	301	0	6	303	0	1			
211	14	0	211	3	5	302	10	10	302	10	11	302	0	6	304	6	2			
212	10	5				303	0	10	303	10	0	304	13	0	306	1	3			
213	11	3				304	10	4	304	10	11	306	0	4	307	9	0			
214	11	3				308	1	12	305	0	12	307	0	6	901	0	9			
301	0	10				309	3	0	903	0	11	308	0	6						
302	0	10																		
902	13	0																		
EA 14	**richer**	**poorer**	**EA 19**	**richer**	**poorer**	**EA 20**	**richer**	**poorer**	**EA 21**	**richer**	**poorer**	**EA 30**	**richer**	**poorer**	**EA 31**	**richer**	**poorer**	**EA 32**	**richer**	**poorer**
202	5	3	201	0	8	201	6	1	202	7	2	201	0	10	201	1	9	201	12	0
203	5	7	205	1	3	203	7	0	203	1	3	203	13	10	202	12	0	202	9	1
207	9	0	206	2	2	204	6	2	204	0	4	204	13	10	203	6	5	203	0	7
209	7	1	208	4	0	205	6	12	205	0	6	205	13	10	204	11	4	205	7	7
210	6	2	211	1	2	206	0	12	207	3	9	206	0	11	205	7	1	206	7	7
211	5	7	212	3	0	207	6	12	208	10	1	207	13	10	206	0	8	207	7	1
214	0	1	213	5	1	208	0	12	210	4	5	208	13	10	208	4	6	209	7	7
303	0	7	214	0	3	209	6	12	211	5	2	209	13	10	211	3	7	214	1	1
304	5	7	302	3	1	210	6	12	301	3	9	210	13	10	212	11	4	302	0	7
305	0	7	303	6	0	214	7	0	302	5	2	211	1	10	214	11	4	303	1	2
304	2	3	304	2	3	301	6	1	303	3	9	212	13	10	302	0	4	304	7	7
902	0	4	902	0	4	302	6	0	304	11	0	213	13	10	305	2	6	306	0	7
						303	7	0				214	13	10	902	0	10	903	0	4
						902	7	0												

This Table reports the reconstructed rankings for the most informative enumeration areas (EAs). In each EA column appears the id code of the household in that enumeration area. Numbers from 201 to 214 represent individuals from the individual or listing surveys. Numbers above 301 were given to respondents added on the spot. Numbers from 901 and above are key informants who appear in this Table because they self-ranked.

Table A4: Relative Rank vs. Relative Survey-Measured Poverty: Pair-Level

Difference j - i in:	Social exp per ca	Durables per ca	Total cons per ca	Was given food (yes)	Food worries (yes)	Days skipped	Improvement in food
Reported rank	-0.521***	-0.204	-0.778	-0.071*	-0.009	0.959	0.136
	(0.195)	(0.130)	(0.578)	(0.038)	(0.061)	(1.794)	(0.105)
log(distance)	0.021	0.001	0.089	-0.001	-0.005	-0.539*	-0.014
	(0.038)	(0.024)	(0.105)	(0.007)	(0.012)	(0.304)	(0.021)
Constant	0.320	0.251**	0.667	0.010	0.021	1.457	-0.009
	(0.249)	(0.124)	(0.671)	(0.043)	(0.074)	(1.793)	(0.119)
R2	0.052	0.007	0.008	0.006	0.000	0.012	0.004
Observations	710	710	710	668	668	657	668
Constructed rank	-0.530***	-0.188**	-1.240***	-0.095***	-0.046	-1.482	0.126
	(0.158)	(0.087)	(0.428)	(0.027)	(0.046)	(1.302)	(0.079)
log(distance)	-0.005	0.002	0.016	-0.001	-0.019*	-1.223***	-0.017
	(0.035)	(0.021)	(0.097)	(0.006)	(0.011)	(0.411)	(0.018)
Constant	0.327	0.187	1.003	0.022	0.140**	6.148***	-0.032
	(0.238)	(0.117)	(0.633)	(0.038)	(0.068)	(2.275)	(0.105)
R2	0.014	0.006	0.009	0.016	0.005	0.024	0.004
Observations	973	973	973	919	919	904	919
Diff in HodgeRank	-1.195***	-0.414***	-2.034***	-0.192***	-0.094	-0.143	0.319***
between j and i	(0.227)	(0.113)	(0.651)	(0.044)	(0.071)	(1.589)	(0.121)
log(distance)	-0.001	0.011	0.024	0.006**	-0.004	-0.409***	0.000
	(0.012)	(0.009)	(0.037)	(0.003)	(0.004)	(0.116)	(0.006)
Constant	-0.022	-0.012	0.148	-0.040**	0.053**	1.825**	0.008
	(0.085)	(0.054)	(0.246)	(0.017)	(0.024)	(0.729)	(0.039)
R2	0.006	0.002	0.002	0.006	0.004	0.004	0.004
Observations	4579	4579	4579	4450	4450	4422	4450

Notes: We report three separate regressions for each constructed variable of aggregated rankings, as described in Section 5. The reported rank variable is the share of reported ranks showing j richer than i. The constructed rank variable is a dummy equals to 1 if j is ranked richer than i (including the case where it is both). The difference in HodgeRank scores between j and i is used as an independent variable: the higher the difference, the richer j is ranked compared to i, following the HodgeRank algorithm described in part 2. A negative difference would indicate that j is ranked poorer than i. All dependent variables are differences in consumption variables. They are calculated as the value for household j minus the value for household i. The complete list of dependent variables is given below. For consumption variables, a positive difference means that i is poorer than j. We also display the PMT and the PPI indexes. Robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All dependent variables are the difference in values between households i and j and described the following:
Social expenditures Total of consumption expenditures collected with a one month recall period: telecom, beauty products, entertainment, charitable contributions. *Annual expenditures:* Total of consumtion expenditures collected with a one year recall period: shoes and clothing, furniture, school fees. *Total consumption:* Weekly expenditures x 52 + monthly expenditures x 12 + annual expenditures. We then divide by the number of household members (adults and children). *Was given food (yes):* Dummy equal to 1 if members of the household have received free food from other households or organizations. *Food worries (yes):* Dummy equal to 1 if respondents answers yes to question . *Days skipped:* Number of days with skipped meals over the last three months. *Improvement in food:* Likert scale from 1 (much worse) to 5 (much better) on whether food situation of the respondentś household has improved relative to previous year.

Table A5: Performance of the method - Simulations (1)

$S = 70\%$	$Var(error)$				
	0	0.1	0.2	0.6	0.9
Pairwise rankings r_ijk					
Reported {i,j,k} pairs / total {i,j,k} pairs	49.06% [38.83 - 59.44]	48.21% [39.64 - 56.37]	48.21% [39.64 - 56.37]	48.21% [39.64 - 56.37]	48.21% [39.64 - 56.37]
Correctly ranked {i,j,k} pairs / ranked {i,j,k} pairs	100.00% [100.00 - 100.00]	96.70% [93.62 - 98.52]	93.59% [89.73 - 96.85]	82.44% [73.39 - 88.32]	76.34% [66.51 - 83.31]
Reconstructed consecutive ranks r_i **and** r_{i+1}					
Missing reconstructed consecutive ranks / 29	0.41% [0.00 - 3.45]	12.59% [0.00 - 34.48]	23.52% [3.45 - 58.62]	69.97% [17.24 - 100.00]	83.48% [51.72 - 100.00]
Correctly reconstructed consecutive ranks / reconstructed consecutive ranks	99.96% [96.43 - 100.00]	85.95% [62.96 - 100.00]	80.64% [56.25 - 100.00]	78.17% [46.15 - 100.00]	81.78% [33.33 - 100.00]
Correctly reconstructed consecutive ranks / 29	99.55% [93.10 - 100.00]	75.00% [55.17 - 86.21]	61.34% [31.03 - 86.21]	22.62% [0.00 - 58.62]	12.97% [0.00 - 34.48]

$S = 50\%$	$Var(error)$				
	0	0.1	0.2	0.6	0.9
Pairwise rankings r_ijk					
Reported {i,j,k} pairs / total {i,j,k} pairs	24.95% [18.47 - 33.79]	24.27% [18.01 - 31.09]	24.27% [18.01 - 31.09]	24.27% [18.01 - 31.09]	24.27% [18.01 - 31.09]
Correctly ranked {i,j,k} pairs / ranked {i,j,k} pairs	100.00% [100.00 - 100.00]	96.76% [92.84 - 98.55]	93.62% [89.07 - 97.15]	82.37% [72.88 - 88.93]	76.19% [64.63 - 83.37]
Reconstructed consecutive ranks r_i **and** r_{i+1}					
Missing reconstructed consecutive ranks / 29	3.93% [0.00 - 10.34]	16.83% [0.00 - 51.72]	35.38% [6.90 - 72.41]	81.14% [55.17 - 100.00]	91.31% [68.97 - 100.00]
Correctly reconstructed consecutive ranks / reconstructed consecutive ranks	98.23% [85.71 - 100.00]	81.58% [65.22 - 95.65]	78.12% [47.06 - 100.00]	80.50% [0.00 - 100.00]	87.52% [0.00 - 100.00]
Correctly reconstructed consecutive ranks / 29	94.38% [79.31 - 100.00]	67.72% [37.93 - 86.21]	49.83% [27.59 - 68.97]	14.83% [0.00 - 37.93]	7.14% [0.00 - 17.24]

Notes: Results using different values of S, the random subset each local observer knows, and $var(error)$. Mean across 100 localities. The bounds below the mean are for the minimum and maximum across localities in brackets.
The total number of {i,j,k} pairs in each locality is 9*(30*29/2)=3915. The total number of {i,j} pairs is 30*29/2=435. Ranks are reconstructed using the relative position index P_i. A reconstructed consecutive rank is missing when $P_i - P_j = 0$. All batches of simulations use the same randomization seed.

Table A6: Performance of the method - Simulations (2)

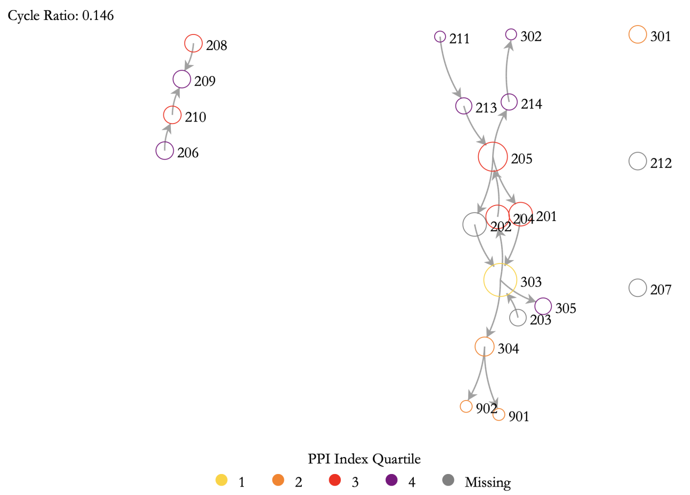
$S = 30\%$	$Var(error)$				
	0	0.1	0.2	0.6	0.9
Pairwise rankings r_ijk					
Reported {i,j,k} pairs / total {i,j,k} pairs	8.88% [4.90 - 12.67]	9.09% [5.26 - 13.69]	9.09% [5.26 - 13.69]	9.09% [5.26 - 13.69]	9.09% [5.26 - 13.69]
Correctly ranked {i,j,k} pairs / ranked {i,j,k} pairs	100.00% [100.00 - 100.00]	96.87% [92.72 - 99.18]	93.87% [86.59 - 97.50]	82.54% [72.89 - 89.75]	76.43% [64.84 - 84.68]
Reconstructed consecutive ranks r_i **and** r_{i+1}					
Missing reconstructed consecutive ranks / 29	15.97% [3.45 - 41.38]	20.45% [3.45 - 51.72]	28.83% [6.90 - 62.07]	75.86% [37.93 - 100.00]	85.38% [58.62 - 100.00]
Correctly reconstructed consecutive ranks / reconstructed consecutive ranks	83.67% [62.96 - 100.00]	73.15% [52.63 - 90.00]	68.48% [40.00 - 88.89]	71.76% [0.00 - 100.00]	68.46% [0.00 - 100.00]
Correctly reconstructed consecutive ranks / 29	70.34% [41.38 - 86.21]	58.10% [31.03 - 79.31]	48.62% [24.14 - 68.97]	16.52% [0.00 - 41.38]	9.59% [0.00 - 24.14]
$S = 10\%$	$Var(error)$				
	0	0.1	0.2	0.6	0.9
Pairwise rankings r_ijk					
Reported {i,j,k} pairs / total {i,j,k} pairs	1.02% [0.26 - 2.76]	1.08% [0.28 - 2.89]	1.08% [0.28 - 2.89]	1.08% [0.28 - 2.89]	1.08% [0.28 - 2.89]
Correctly ranked {i,j,k} pairs / ranked {i,j,k} pairs	100.00% [100.00 - 100.00]	96.45% [78.57 - 100.00]	93.82% [75.00 - 100.00]	83.15% [62.26 - 96.77]	76.37% [52.17 - 93.55]
Reconstructed consecutive ranks r_i **and** r_{i+1}					
Missing reconstructed consecutive ranks / 29	59.97% [37.93 - 86.21]	58.03% [31.03 - 82.76]	58.38% [34.48 - 82.76]	59.86% [34.48 - 79.31]	59.62% [27.59 - 79.31]
Correctly reconstructed consecutive ranks / reconstructed consecutive ranks	64.98% [30.00 - 100.00]	63.05% [36.36 - 100.00]	63.64% [28.57 - 100.00]	56.18% [14.29 - 83.33]	55.15% [30.77 - 88.89]
Correctly reconstructed consecutive ranks / 29	25.93% [6.90 - 44.83]	26.31% [10.34 - 44.83]	26.21% [6.90 - 48.28]	22.72% [3.45 - 44.83]	22.28% [6.90 - 37.93]

Notes: Results using different values of S, the random subset each local observer knows, and $var(error)$. Mean across 100 localities. The bounds below the mean are for the minimum and maximum across localities in brackets.
The total number of {i,j,k} pairs in each locality is 9*(30*29/2)=3915. The total number of {i,j} pairs is 30*29/2=435. Ranks are reconstructed using the relative position index P_i. A reconstructed consecutive rank is missing when $P_i - P_j = 0$. All batches of simulations use the same randomization seed.

Network Figures

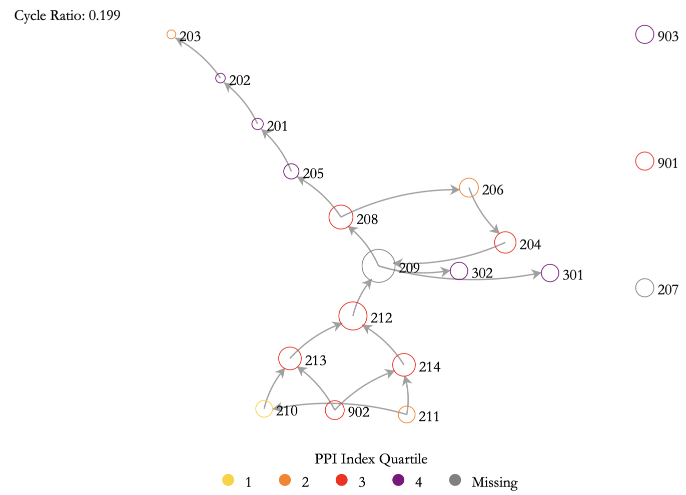
Figure A1: Directed graph of relative rankings - Urban Slums

Location 1



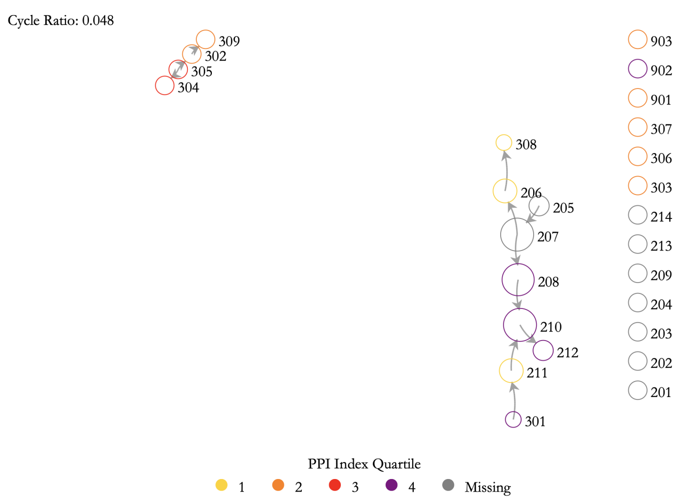
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 2



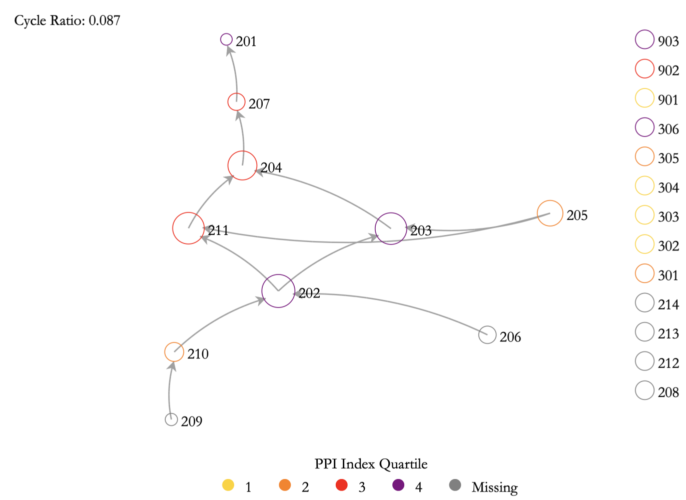
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 3



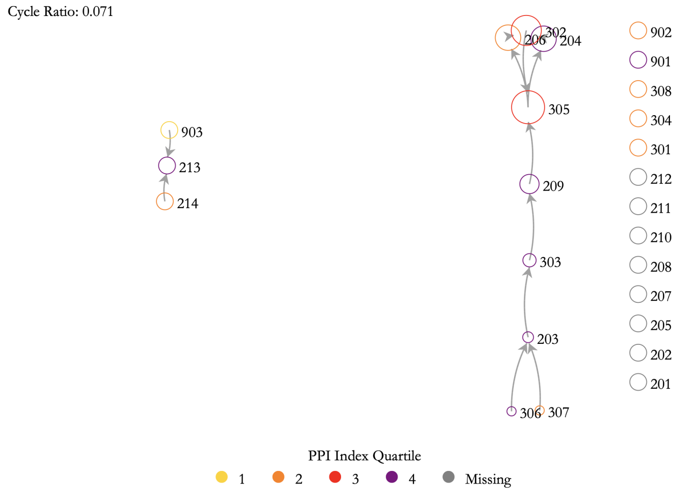
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 4



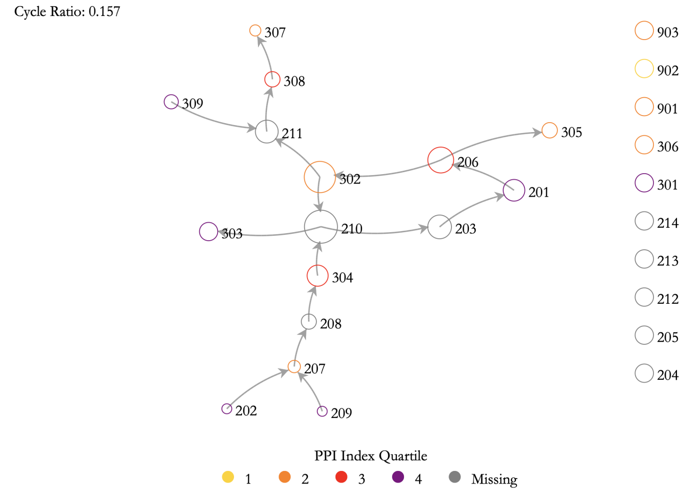
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 5



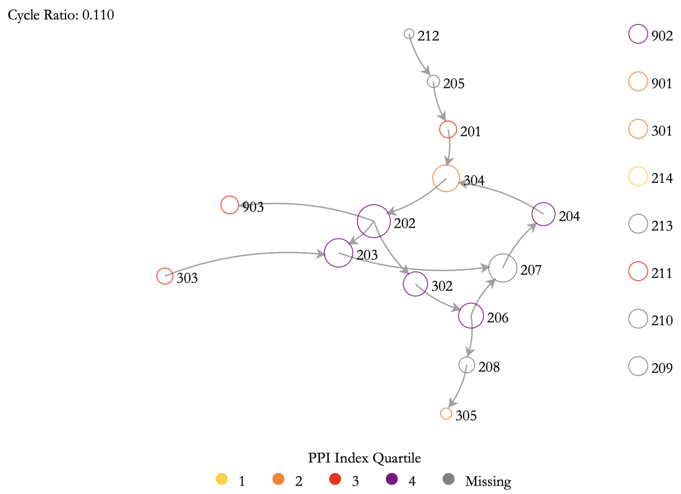
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 6



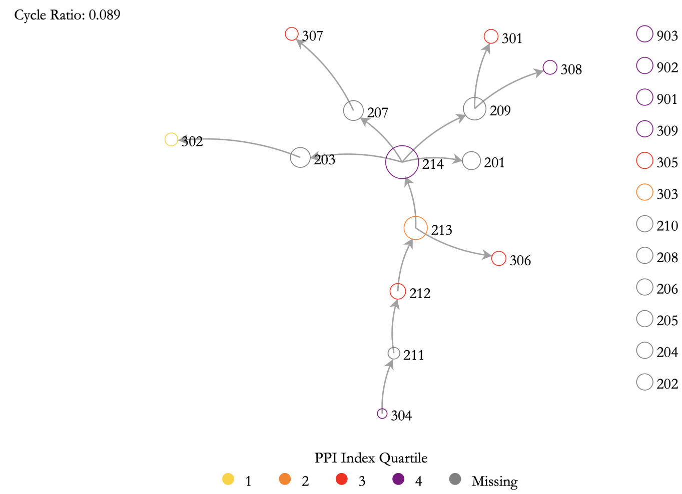
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 7



Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

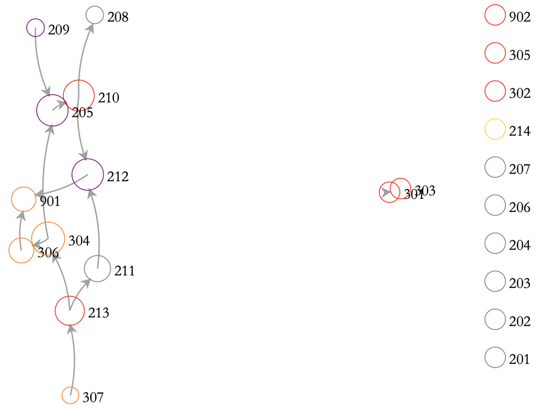
Location 8



Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 9

Cycle Ratio: 0.076



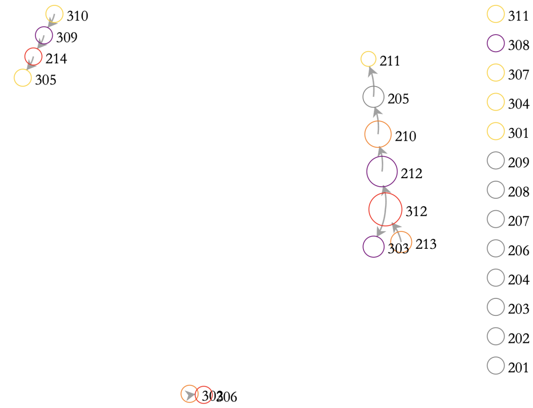
PPI Index Quartile

1 2 3 4 Missing

Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 10

Cycle Ratio: 0.046



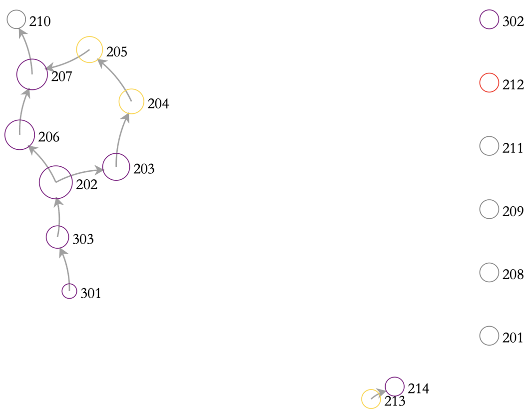
PPI Index Quartile

1 2 3 4 Missing

Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 17

Cycle Ratio: 0.139



PPI Index Quartile

1 3 4 Missing

Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 18

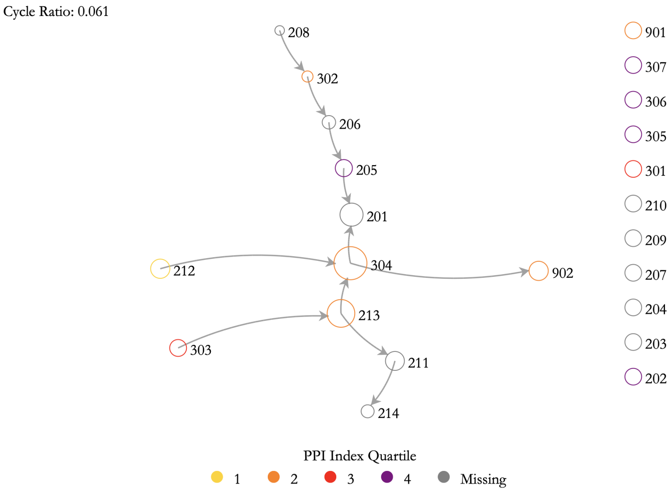
Cycle Ratio: 0.108

PPI Index Quartile

1 2 3 4 Missing

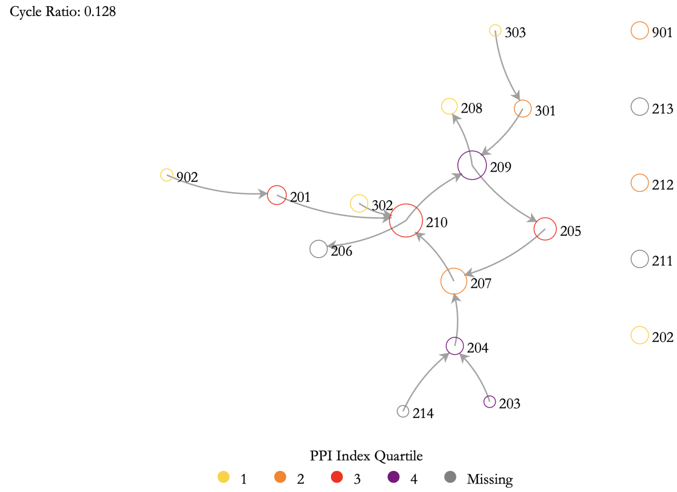
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 19



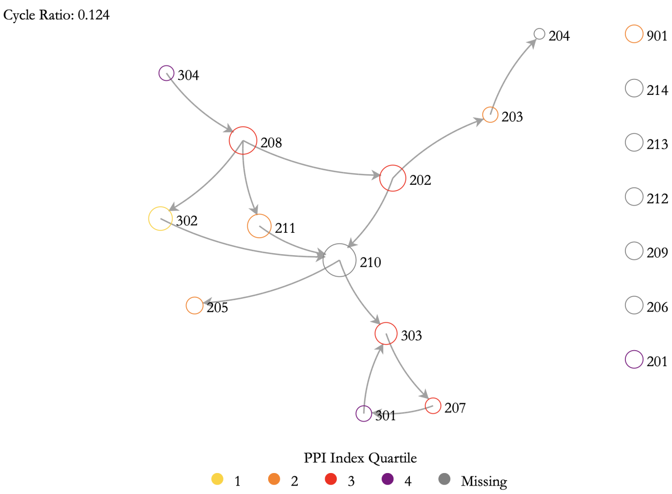
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 20



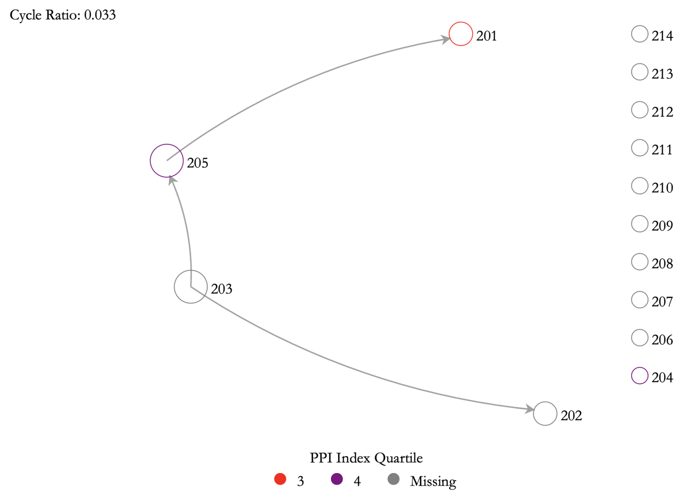
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 21



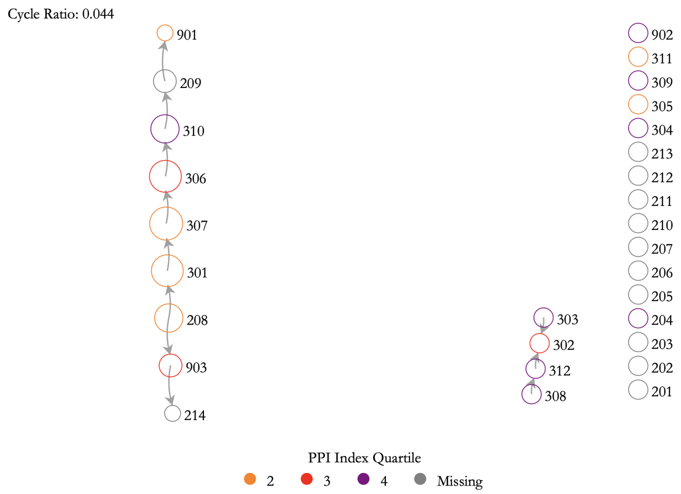
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 22



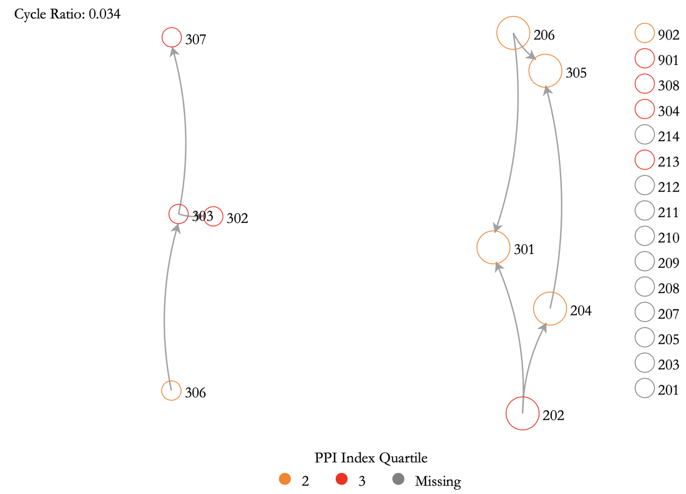
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 23



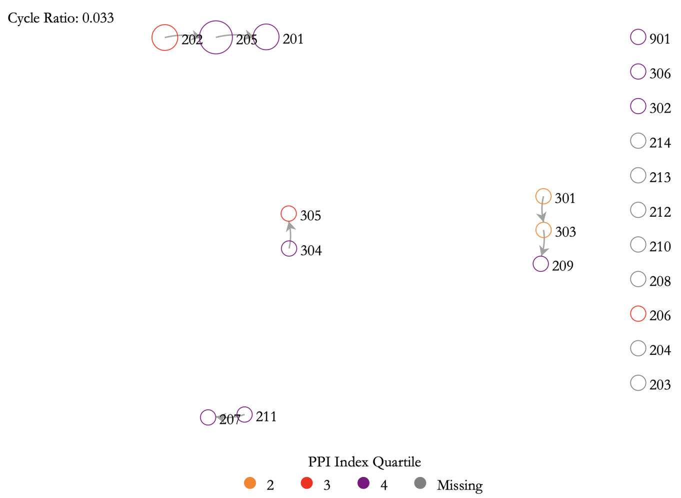
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 24



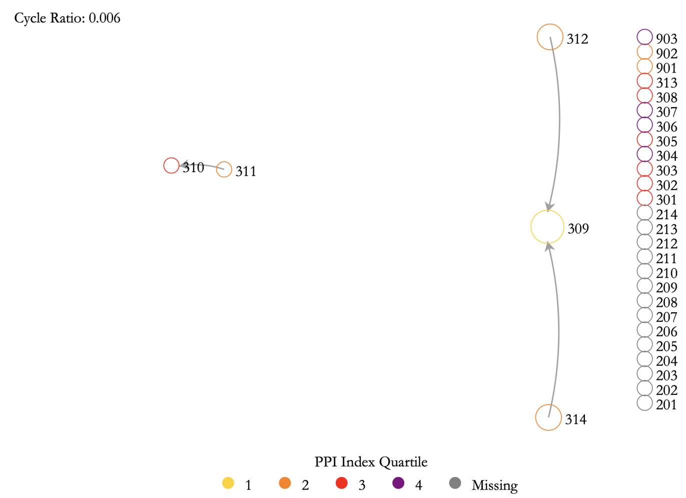
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 25



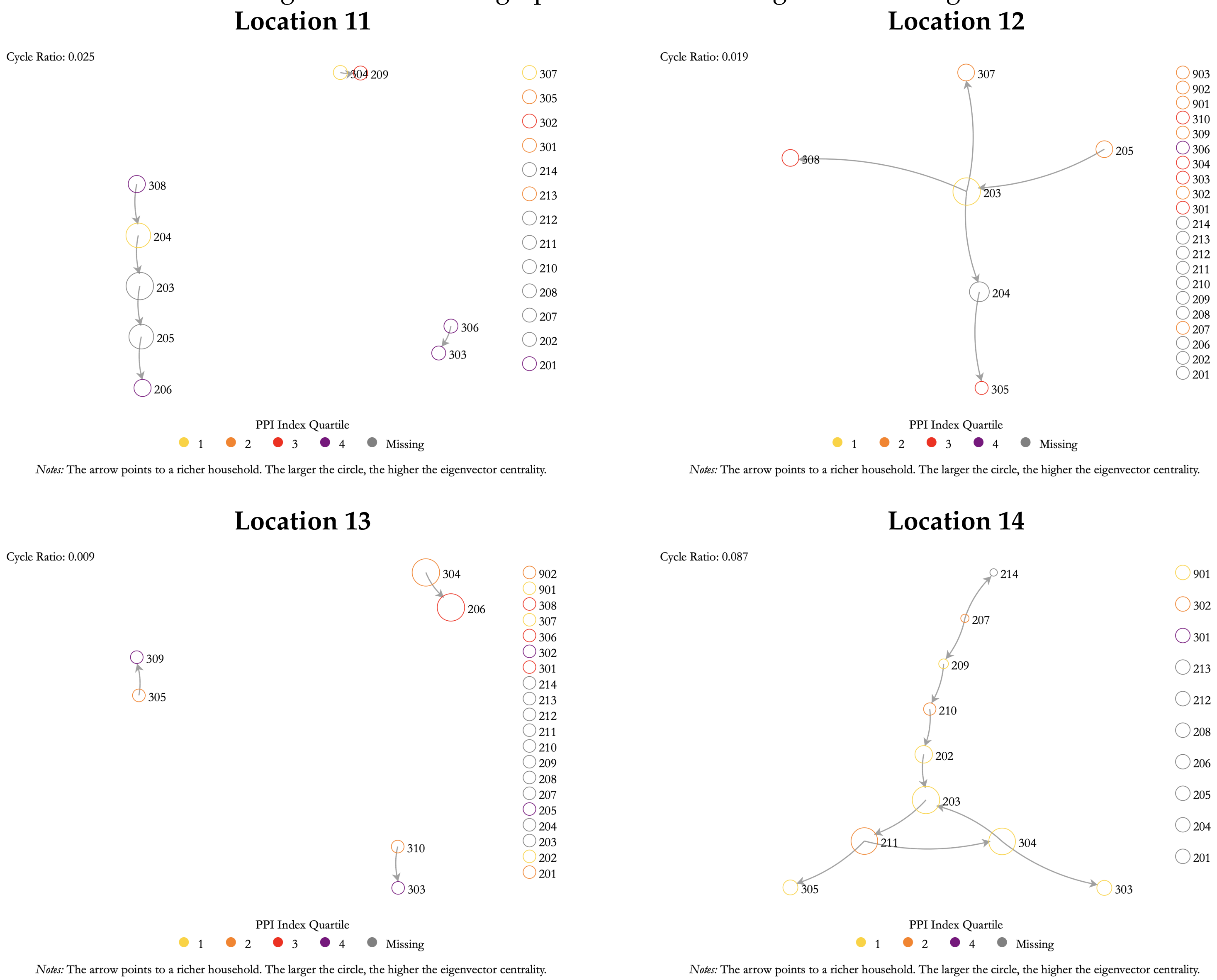
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 26



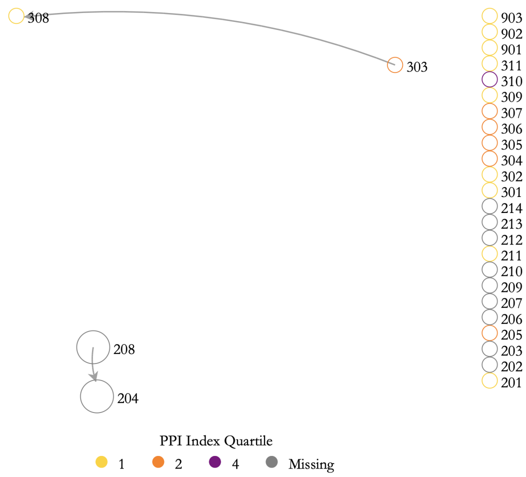
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Figure A2: Directed graph of relative rankings - Rural Villages



Location 15

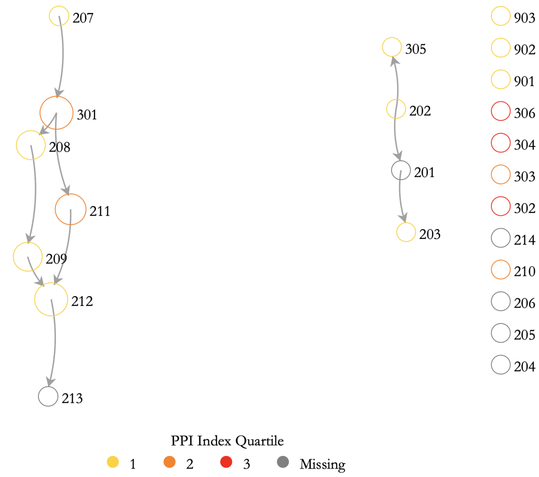
Cycle Ratio: 0.005



Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 16

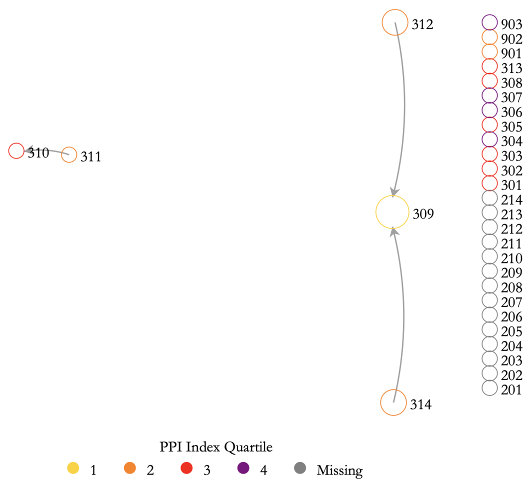
Cycle Ratio: 0.062



Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 27

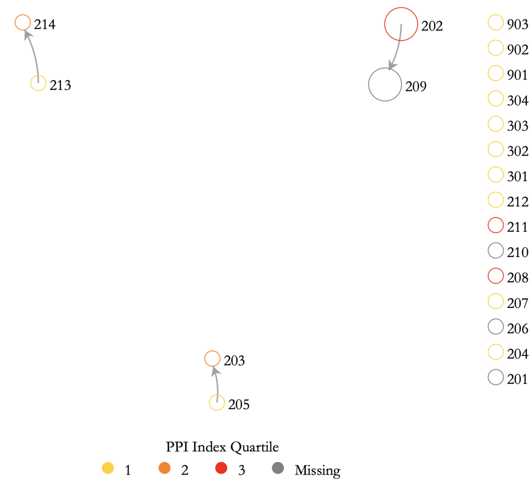
Cycle Ratio: 0.006



Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 28

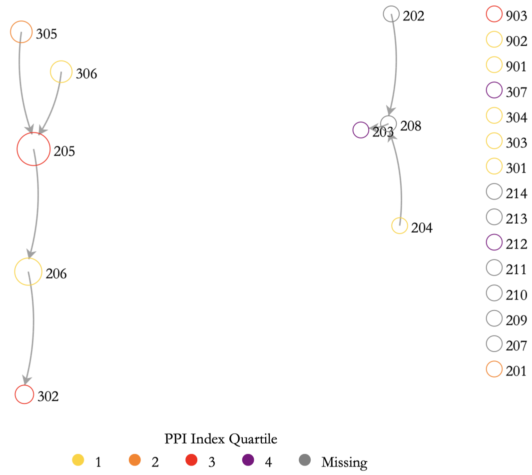
Cycle Ratio: 0.013



Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 29

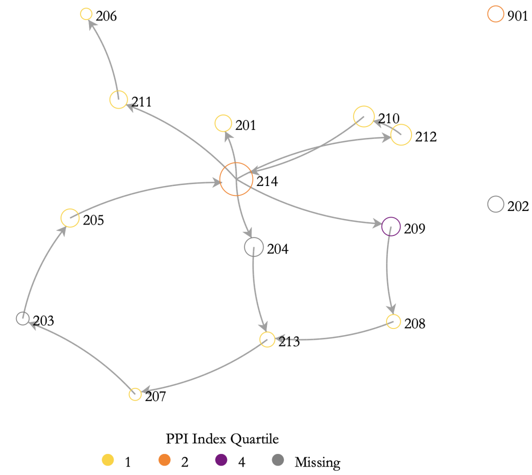
Cycle Ratio: 0.030



Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 30

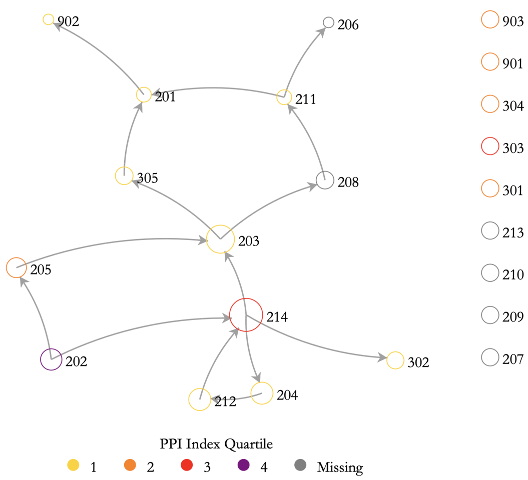
Cycle Ratio: 0.376



Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 31

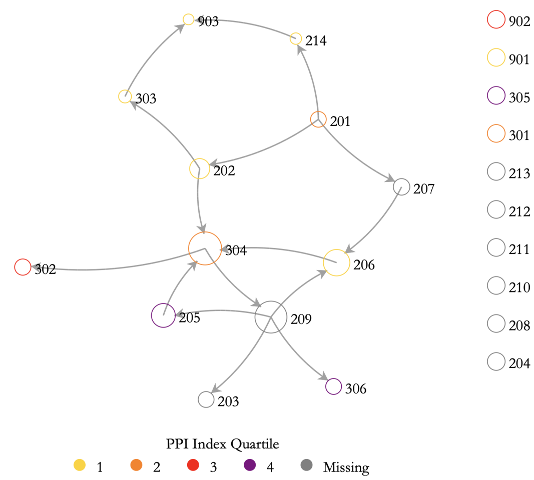
Cycle Ratio: 0.119



Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

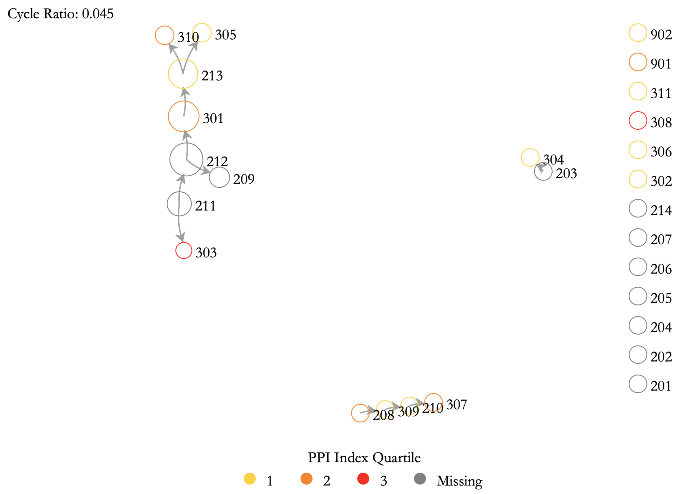
Location 32

Cycle Ratio: 0.087



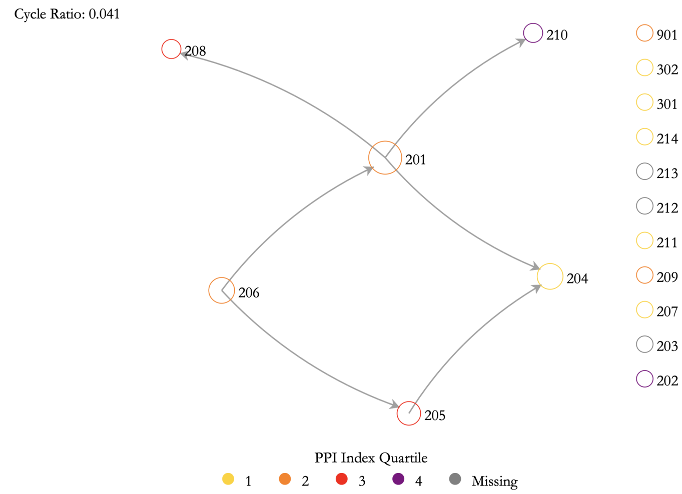
Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 33



Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.

Location 34



Notes: The arrow points to a richer household. The larger the circle, the higher the eigenvector centrality.