KEEPING UP APPEARANCES:
AN EXPERIMENTAL INVESTIGATION OF RELATIVE RANK SIGNALING

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ABSTRACT

We investigate the potential welfare cost of relative rank considerations using a series of vignettes and lab-in-the-field experiments with over 2,000 individuals in Abidjan, Ivory Coast. We show that:(1) individuals judged to be of a lower rank are perceived as more likely to be sidelined from beneficial opportunities in many aspects of life; and (2) in response, individuals distort their appearance and consumption choices in order to appear of higher rank. These effects are strong and economically significant. As predicted by a simple signaling model, the distortion is larger for individuals with low (but not too low) socio-economic status.

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"Allo, the Bouquet residence! Yes the name is 'Bouquet'. How it's spelt? B-U-C-K-E-T."
Keeping Up Appearances (British sitcom 1990-95)

"Dress how you want to be addressed." Popular saying in Nigeria.

1 Introduction

There is increasing recognition among economists that human beings care about their relative place in society, not just their material welfare, and that subjective welfare has a real material cost in terms of depression, suicide, addiction, and other health issues (Layard, 2005, 2011; Daly et al., 2011). Happiness and satisfaction with one's level of material consumption seem to depend on the consumption levels of others. In a well known experiment, Harvard students were asked whether they would prefer to earn $50k a year in a world where others earn $25k, or to earn $100k in a world where others earn $200k. Two thirds of respondent choose the former (Solnick and Hemenway, 1998). Brown et al. (2008) provide survey and lab-experimental evidence that satisfaction is affected by the *ordinal* position of one's wage. These patterns have been interpreted as indicating that individuals use their relative rank as measure of their life achievements. Research has also shown that what matters most to individuals is their rank relative to their peers, that is, individuals who started from similar initial conditions, such as individuals of the same gender, born in the same village, caste or social group, or attending the same school as a child (e.g., Fafchamps and Shilpi, 2008).

Relative rank concerns have also been shown to affect economic behavior. Veblen and Howells (1899) observed that individuals often engage in conspicuous consumption as a way of increasing their social status. Duesenberry (1949) similarly argued that considerations of relative social status increase people's propensity to consume out of current income to 'keep up with the Jones', thereby reducing savings and productive investment. These theories have regained some prominence recently (e.g., Ghiglino and Goyal, 2010; Bramoullé and Ghiglino, 2022; Langtry, 2023). In the United States, Charles et al. (2009) find evidence that visible consumption is declining in the mean income of one's reference group, consistent with a model of status seeking, and show that this explains most of the racial gap in consumption patterns. In lower-income countries, these theories have been brought to the fore to account for lavish

weddings (e.g., Rao, 2001; Bloch et al., 2004) and funerals (e.g., Jindra and Noret, 2011).

While Veblen and Duesenberry were primarily concerned with the upper class, rank concerns may affect the poor as well. There is abundant anecdotal evidence that the *appearance* of poverty at times attracts scorn, ridicule, discrimination and, often, moral condemnation (e.g., Walker and Bantebya-Kyomuhendo, 2014; Angeli et al., 2024). It is indeed common for the poor to complain about the lack of respect they receive from others—which may, in turn, reduce their own self-respect. These concepts have been revisited in the behavioral economics literature under the names of social image and self-image concerns (e.g., Austen-Smith and Fryer Jr, 2005; Bursztyn et al., 2018; DellaVigna et al., 2012) and shown to affect various types of behaviors. Bursztyn and Jensen (2015) and Bursztyn et al. (2019), for instance, find that social image concerns affect educational investment decision for high-school students in the USA. Of particular relevance for our paper, in an experiment on charitable behavior Butera et al. (2022) show that public recognition motivates desirable behavior but also creates unequal image payoffs: high-performing individuals enjoy significant utility gains, while low-performing individuals incur significant utility losses.

This study is interested in the victimization of individuals based on their apparent social rank, which may involve discrimination in hiring, profiling by the police, or social exclusion. We investigate two questions central to the welfare cost of relative rank considerations: (1) are individuals judged to be of lower socio-economic status (SES) perceived as more likely to be victimized or discriminated against? and (2) as a result, do lower SES individuals distort their consumption pattern to appear of higher status than they actually are? We answer these two questions by combining a series of closely related experiments and measurements on more than 2000 respondents recruited among the poorer segments of urban and peri-urban dwellers of Abidjan, a large city in West Africa. We also offer a simple model showing that, in the presence of relative rank considerations, the distortion in consumption is larger for individuals with lower SES.

To answer the first question on victimization, we start with a randomized vignette approach to identify people's expectations regarding the victimization of low SES individuals. We find that respondents shown the photograph of an individual dressed in a high SES attire judge the pictured individual as less likely to be evicted by a landlord or suspected of committing a theft or burglary, and more likely to be invited to a social gathering—compared to the *same*

individual photographed in a medium or low SES attire. Using a different randomized vignette, we find that applicants who select a free meal at a job interview are thought less likely to be selected than those who choose a tote bag. Using another vignette, we compute the economic magnitude of these appearance effects and they are found to be large relative to, say, being currently unemployed or having a high school diploma. Taken together, this demonstrates that appearances matter for social and economic outcomes of importance, and that respondents are aware of this.

To answer the second question on choice distortion, we use a combination of experiments. We first examine whether respondents self-censor, i.e., whether lower-SES individuals do not bother *applying* for opportunities that are perceived as favoring higher-SES individuals. We find some evidence of such self-censoring in the aggregate, but not *within* our sample. We then investigate whether respondents modify their appearance to improve their perceived SES rank. This is achieved by taking a photograph of willing participants and then offering them the opportunity to have another picture taken at a later time, leaving them time to prepare. We experimentally vary whether participants are told that the photograph will be used to select participants in a documentary on Abidjan. We find that a significantly larger proportion of participants ask for a second photograph to be taken when the photograph matters for selection. What's more, we find that the prepared photographs are ranked significantly higher in terms of SES; and that a given individual is perceived by a panel of judges as less likely to be victimized when the panel is shown the prepared photograph, compared to when they are shown the unprepared photograph of that same individual. Together, these results suggest that respondents deliberately seek to signal a higher SES to increase chances of a favorable outcome.

We next test whether, beyond physical appearance, *consumption* patterns are used as signals. To this end, we invite respondents to apply to a one-day job under two different treatment conditions. Those in the "random" group are told that applicants will be randomly selected while those in the "screening" group are told that their responses to a short questionnaire will be transmitted to a selection committee. As part of their compensation for the work, applicants were given the choice between a meal or a pen. We hypothesize that participants in the "screening" group may be strategic in their choice: even if they prefer the free meal, they may decide to take the pen instead if they think this choice may influence the selection process. We find this to be the case: applicants in the screening group are less likely to select the meal than

those in the random group.

Last but not least, we conduct a field experiment to test whether individuals distort their consumption towards a status item when their consumption choice is publicly observable. To implement this idea, we offer survey respondents a choice between receiving a basket of food and household goods vs. making a contribution to a charity. These gifts are presented as a reward for participating to the survey. We do this under three observability conditions: a private treatment in which participants indicate their choice independently using a tablet; an enumerator-aware treatment, in which respondents disclose their choice to the enumerator; and a public treatment in which participants register their choice on a sheet of paper that subtly suggests it may be seen by the other respondents in the area. The observability treatments are randomized across respondents. We first verify that the charitable contribution is more likely to be selected by wealthier households. This suggests that it can be a signal of high SES. We then show that respondents in the two public treatments are more likely to select the charitable contribution, an effect that is statistically significant in the enumerator treatment. We then test whether this effect is larger for poorer household. We find that to be the case: on average, poorer respondents spend a larger share of their total budget on the charitable contribution. We also observe that, as predicted by the model, excess spending on charity disappears in the private treatment for those in the poorest segment of our sample.

Our findings contribute to various literatures. First, we contribute to the literature on social stigma. In his seminal contribution, Moffitt (1983) introduces the idea that social stigma can contribute to the low take-up of welfare programs oriented towards the very poor. Bhargava and Manoli (2015) and Celhay et al. (2022) provide recent experimental evidence for this in the United States. Our paper introduces, and provide evidence for, the idea that consumption patterns more broadly (not just the take-up of programs targeted at the poor) can be interpreted as signals of SES and hence distorted by social stigma concern.

Second, we provide a rationale for the finding that *perceived* inequality is often lower than *true* inequality within a society. For example, Cullen and Perez-Truglia (2022) have shown that workers in a Southeast Asian bank do not realize how much more higher paid workers earn, and they similarly do not realize how low the earnings of low paid workers are. Hvidberg et al. (2020) document a "center bias" in Denmark: within a given reference group, those with higher incomes tend to overestimate the incomes of others, and therefore underestimate their

own income position. And in our previous work (Dupas et al., 2023a), we have found that urban and peri-urban households in Côte d'Ivoire know very little about the relative consumption levels of their neighbors. The process investigated here offers a possible explanation for these findings, namely, that efforts by individuals to manipulate their social image compresses the distribution of perceived income relative to the truth. As a result, people believe that they are more similar than they actually are.

Third, our findings relate to the literature on under- and over-reporting to enumerators. To date, concern about mis-reporting in surveys has focused on strategic *under-reporting* in the context of government surveys used to target social programs (e.g., Camacho and Conover, 2011). Martinelli and Parker (2009) show similar evidence of under-reporting, but also document *over-reporting* of assets due to an 'embarrassment' effect when the respondent does not have an asset owned by a majority of their neighbors. We find a similar pattern in our last experiment: respondents sought to signal a higher socio-economic status to the *enumerator* by choosing the charitable contribution over the basket of goods. This suggests that surveying individuals about their assets, income level or other proxies of social status may be affected by relative rank considerations, an issue we conceptualize in the paper.

Finally, our findings have implications for the literature on targeting. In contexts where the state has limited information about individuals' income (because the majority of workers are informally or self-employed), targeting benefits to the poor requires either costly (e.g., via household visits) or cheap but imprecise (e.g., based on dwelling's features) proxy-means tests. An attractive alternative is community-based targeting (Alatas et al., 2012). The premise behind the promise of community-based targeting is that members of a neighborhood have information about each other's economic status, and this information can be aggregated to generate, if not a full ranking, at least some binning by quantiles (Dupas et al., 2023a). This premise breaks down if members of a community do not have accurate information about each other. Our model suggests that identifying the *very poor* may be "easy" since they cannot afford to hide their status, but identifying those who are poor but not destitute may be much more difficult.

The paper proceeds as follows. Section 2 presents our conceptual framework. Section 3 briefly introduces our study setting and the type of data we bring to bear. Section 4 presents the results, before concluding remarks in Section 5.

2 Conceptual framework

We wish to experimentally investigate the role that relative rank considerations play at low-income levels. Our approach is organized around a joint hypothesis that we dub 'Keeping Up Appearances' (KUA) for the purpose of this paper:

H1 People who look poor have fewer economic and social opportunities, and this is common knowledge;

H2 Given H1, people distort their appearance or, more generally, their consumption choices to appear less poor than they are.

Why people are treated differently for looking poor is not something we investigate empirically in this paper. It may arise for a variety of reasons already discussed in the existing literature: statistical discrimination, such as 'social profiling'; a taste for discrimination, perhaps grounded in the belief that a poor appearance reflects weakness, laziness, or immorality; or a sense of identity by which a 'proper' appearance is needed to belong to a social group (Walker and Bantebya-Kyomuhendo, 2014). Whatever the reason, looking poor may lose the respect of others, and result in less favorable treatment by landlords, employers, members of the community, or the police. Keeping up appearances to gain the respect of others may thus be optimal from an individual point of view, and there is plenty of anecdotal evidence that many people believe this to be true—if only from the English language itself, e.g., 'to look respectable'.

The KUA hypotheses forms the basis of our experimental design and they can be formalized in a model in which individuals who appear poor are victimized, and the fear of being victimized induces poor people to engage in behaviors that reduce their immediate material welfare but protects them from victimization.[1]

2.1 Modeling assumptions

We are interested in situations in which a person—the observer—decides to assign or not assign a target individual to a beneficial state. Since our focus is on urban areas where populations

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[1]Our conceptual framework can be expanded to include social taxation on the rich, based on their observed consumption (see Appendix C for details). Given that our study population is predominantly poor, this possibility is omitted from our experimental design and, consequently, from the analysis as well.

are highly mobile (Dupas et al., 2021, 2024), we rule out learning, repeated interaction between parties, and information sharing between observers. We assume that the observer's payoff depends on a hidden attribute of the target that the observer must infer from a single continuous informative signal a that, given our experimental design, we call the target's *appearance*. The payoffs of both the observer and the target may include both material elements (e.g., the wage or the productivity of the target) as well as immaterial elements (e.g., self-image and social image considerations). We assume that the target always benefits from being selected, but the observer incurs a cost when choosing a low-attribute target and a benefit when selecting a high-attribute target. Examples of such situations include the decision to interview or hire a job candidate, to admit someone to a social group or study program, to (not) punish someone for an action, to (not) arrest someone on the suspicion of a crime and, more generally, to treat someone with respect or not.[2]

Provided some reasonable assumptions are made—e.g., observers have a well defined prior on the distribution of hidden attributes – the optimal decision for the observer is to set a cutoff or threshold signal value Z above which the target is selected (e.g., Coate and Loury, 1993; Abebe et al., 2023). This prediction holds even if the target can manipulate the signal by incurring a cost. A signaling equilibrium value of Z is one at which the observer is just indifferent between selecting or not, taking into account the incentive effect that threshold Z has on targets' investment in signal a and the joint distribution of signals and attributes that this investment produces (e.g., Ghiglino and Goyal, 2010; Bramoullé and Ghiglino, 2022).

In our experiment, we do not create conditions under which the *equilibrium* value of Z may change. For this reason, we focus our conceptual discussion on individual decisions by observers or targets. We nonetheless recognize that the equilibrium value of Z is likely to vary with the types of decision the observer makes—which is why we examine different types of selection decisions in our experimental design.

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[2]This rules out situations in which misrepresenting one's social group or status constitutes a serious violation of the social order, i.e. situations where 'lower' classes must 'know their place'. In such cases, the option to 'blend in' is unacceptable to the observer, and anyone who is found to have misrepresented their identity faces strong social punishment. This possibility is ruled by design in our experiment and is therefore ignored here.

2.2 Pure signaling

We start with a simple version of the model in which the signal is the target's perceived socio-economic status or income. Let V represents the target's expected loss of not-being selected, which we call *victimization* (i.e., being offered a less favorable treatment and fewer opportunities). Let Z be the threshold of observable income below which a target is victimized. Observable income is denoted a for appearances and is determined by $a_i = y_i + s_i$ where y_i is actual income and s_i is a signal or improvement in appearances that individual i can purchase at unit cost p. In other words, targets must keep up appearances to avoid victimization by observers.

The target solves an optimization problem of the form:

$$max_{s \geq 0}\, U(c) - V.I(y + s < Z) \text{ subject to } c = y - ps \tag{1}$$

where $U(c)$ is an increasing concave function with $lim_{c \to 0} U(c) = -\infty$ and where $I(.)$ is an indicator function equal to 1 if $y + s < Z$. By construction, the sole benefit the target derives from appearances is a reduction in victimization. Since there is no benefit from exceeding the minimum appearance Z that avoids victimization, it follows that, conditional on devoting resources to appearances, the optimal value of $s = Z - y$.

Let y^* be the level of income satisfying $U(y) - V = U(y - p(Z - y))$. By concavity of the utility function, y^* exists and is unique. The solution to (1) is thus of the following form:

- If $y \geq Z$ then $s = 0$ since the likelihood of victimization (for appearing poor) is 0. Targets who are sufficiently rich do not need to invest in appearances to avoid victimization.

- If $y < y^*$, then $s = 0$. For these targets, the utility cost of appearances is higher than the expected cost of victimization.f

- Targets for whom $y^* < y < Z$ invest $s = Z - y$ in appearances.

To summarize, the model predicts that individuals below an income level of Z invest an increasing amount of resources to preserve appearances as their income falls, down to an income level y^* below which they give up on appearances and get victimized. This implies that, for these individuals, the share ps/y of their consumption budget spent on the signal s falls with

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income y: the signal has an income elasticity less than 1 and thus behaves like a necessity.[3] It also follows from this model that the fraction of victimized targets in the population increases with Z, the cutoff in appearances, and p, the cost of reaching that threshold.

We now endogenize Z using a simple principal-agent model. We assume that the principal (i.e., the observer) derives a negative payoff $\pi^- < 0$ from interacting with a target i with income $y_i < \hat{y}$, and $\pi^+ > 0$ otherwise. The principal's expected payoff W of interacting with target i is given by:

$$W = Prob(y_i < \hat{y})\pi^- + Prob(y_i \geq \hat{y})\pi^+$$

and is maximized by choosing not to interact with i if $y_i < \hat{y}$. While y_i is not directly observed by the principal, this objective can nonetheless be achieved by setting Z such that only targets with $y_i \geq \hat{y}$ choose to invest in the signal s. It follows that the optimal cutoff Z^* is the value that solves the following equation:

$$U(\hat{y}) - V = U(\hat{y} - p(Z^* - \hat{y}))$$

The observer interacts with individuals whose appearances Z is higher or equal to Z^*, and excludes them otherwise. This is true even though there is bunching of targets at Z^* since, by buying the signal s to arrive at Z^*, all of them have revealed having an income greater or equal to $\hat{y}$. With linear utility, the above equation has the closed-form solution:

$$Z^* = \frac{V}{p} + \hat{y}$$

which shows that, as intuition suggests, the signal required by the observer falls with the cost p of acquiring the signal, and it increases with the target's cost of victimization V and the income threshold $\hat{y}$ above which interaction with i is beneficial for the observer.

It follows that different types of interactions—e.g., social event vs job hiring—are likely to

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[3]Similar qualitative predictions are obtained if the model is generalized by making the likelihood (or severity) of victimization a sharply decreasing function of appearances a in the vicinity of Z. This is because of $lim_{c \to 0}U(c) = -\infty$: no matter how costly victimization is, the fact that consumption is needed for survival makes very poor individuals choose victimization over starving, and thus $y^* > 0$ for any $\{V, p, Z\}$. In other words, for any cumulative distribution of income $F(y)$ with some mass below y^*, a fraction $F(y^*)$ of the population will be victimized, with an intensity of victimization V that may increase by how much their income falls below y^*.

have different signal levels Z^*. There are indeed many examples from everyday life in which (1) a minimum standard of appearance is required to participate in a given activity and (2) this minimum varies with the type of interaction. For instance, until recently, it was common to require that white-collar male employees wear a suit and tie everyday to work.[4] Even when formal wear is no longer required at work, it is still recommended at job interviews. Higher dress codes exist as well. Exclusive black tie events, for instance, keep at bay those who cannot afford to buy or rent one, and dress codes are common in historically upper-class sports, either for players (e.g., golf, tennis, polo) or for spectators (e.g., the Royal Ascot and the Henley Royal Regatta). Dress code restrictions are also commonly used at the lower end of the spectrum, e.g., ruling out hoodies in shopping malls, shorts and flip-flops in restaurants, jeans in discos, and pajamas in schools and grocery stores.[5] Equivalent restrictions exist for women as well, e.g., regarding hairstyle or headdress, and similar examples can be found in non-Western cultures. From this we conclude that there is ample observational evidence that participation in many human activities is conditioned on a minimum threshold of appearance, and that this threshold depends on the activity – e.g., appearance requirements are more costly for activities aimed at wealthier social strata. In all these examples, dress codes serve to exclude poorer individuals, a feature that is central to our model.

2.3   Luxury consumption

The model can be modified to make s an unnecessary expenditure on an observable luxury good, that is, a good that generates utility for rich consumers. The utility function is now $U(c,s)$, where c is regular consumption and s is luxury consumption. Here signaling operates by inferring someone's income y from their luxury expenditure s. To illustrate with a linear example, let x be the expenditure that targets would spend on the luxury in the absence of signaling, and let the optimal x^* be given by $x^* = max(0, -\alpha + \beta y)$ where α and β are parameters. We see that $x^*=0$ if $y \leq \alpha/\beta$ and $x^* = -\alpha + \beta y$ otherwise. It follows that, if observers see x, they can infer y as long as $x > 0$.

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[4]The World Bank, for instance, only allowed its employees to report to work without a suit and tie in 1999, in which year a 'dress casual' Friday was introduced. A suit and tie remained compulsory other days of the week.

[5]The astute reader will recognize that many of these examples originate from the UK, which is particularly strict about dress codes. But they exist in one form or another in most Western societies.

Let us now assume that there is a cutoff level of luxury consumption Z below which the target is victimized. This is because a low consumption of the luxury good signals poverty. With this assumption, some targets choose to spend an excess amount s on top of the optimal amount x^*, such that $s = Z - x^*$ and they avoid victimization. In this modified model, it is excess spending on the luxury good that serves the role of signal, as in Veblen and Howells (1899) and Duesenberry (1949). For simplicity of exposition, we assume that individuals derive no utility from consuming more than their desired luxury consumption x^*: $U(c, s > x^*) = U(c, x^*)$. With these modifications, we have the following predictions:

- There is a level of income y^* under which the target prefers to incur the victimization cost $VI(s < Z)$ rather than waste their limited budget on a good that gives them little or no utility. This cutoff income value is the solution to equation $U(y^*, 0) - U(y^* - Z, x(y^*)) = V$ where $x^*(y^*) \geq 0$.

- Individuals with income $y > y^*$ and a desired luxury consumption between $x(y^*)$ and $x(y) < Z$ spend $s = Z - x(y)$ extra on the luxury good, which leaves $y - Z$ for non-luxury consumption expenditures c. For these individuals, luxury consumption has an income elasticity less than 1, i.e., it behaves like a necessity.

- Individuals with $x^* > Z$ [and thus $y > \frac{Z+a}{b}$] spend more than Z on the luxury good. For these individuals, luxury consumption behaves normally.

- The optimal Z^* for the principal is the value that solves the following equality:

$$U(\hat{y}, 0) - U(\hat{y} - Z^*, x(\hat{y})) = V$$

where, as before, $\hat{y}$ is the level of income of the target below which the utility of the principal turns negative.

This model is similar to our pure signaling model, except that the rich spend on the signal good more than the minimum Z that is required to avoid victimization. We also note that, in this model, many individuals spend something (x^*) on the luxury good *even when* that expenditure is not observed and does not serve as signal of SES status. This is different from our earlier model where the signal good generated no utility. We also note that if the poor could costlessly

signal a higher social status, they would do so—e.g., to an enumerator (e.g., Martinelli and Parker, 2009).

Model predictions do not require that observers be unaware that the targets are manipulating their perceived rank. This is because signals remain partially informative: in equilibrium, the ultra-poor are identified and victimized; and the very rich are identified because they spend more than the minimum on luxury goods. But there exist intervals of true income values over which targets are observationally equivalent: they are 'like the others', they blend in. Here too, the fraction of victimized targets increases with Z and p, implying variation in victimization with the decision context.

The luxury good can be a material consumption item, such as a more expensive attire or hair style. It can also be an expenditure on a good that generates no material benefit for the target but signals a high income. In the context of our study, making a charitable contribution often is, in many ways, the ultimate signal of high income since it costs dearly to low-income individuals and only serves to improve the target's self- and social image. Hence, given the choice between a luxury item and charitable giving, a target would send a stronger signal by spending additional resources on the latter rather than the former.

2.4 Heterogeneity

We have noted that the targets' decision to spend resources on signaling depends on the victimization threshold Z: the higher Z is, the larger the fraction of targets who are discouraged from trying to 'blend in'. It is also the case that observers with different payoffs choose different values of Z. For instance, an observer may set a lower Z when inviting someone to a social gathering than when hiring this person for a demanding job. Payoff functions may also differ across observers, resulting in different Z's even for the same selection task. Consequently, if payoffs are heterogeneous among observers, we should observe a gradual—instead of sharp—increase in victimization as appearances deteriorate.

It follows that targets will choose different levels of investment in signaling depending on what the selection is for and their assumption about the observer's Z. In particular, poor individuals are predicted to invest in signaling only if they believe the observer's Z is low enough. Hence, heterogeneity in beliefs about Z across targets can generate bunching on

several levels of Z. Importantly for our purpose, we expect an increase in signaling effort when the signal becomes visible to the observer, something we can manipulate experimentally.

3   Study Setting and Data

3.1   Sample

The sample for this study is the Abidjan sample from the African Urban Development Research Initiative (AUDRI) at Stanford University. In 2019, AUDRI started an effort to collect representative data of urban and peri-urban populations in the Greater Abidjan, the capital city of Côte d'Ivoire. The sample includes 84 "semi-rural" Enumeration Areas (EAs) and 622 urban EAs across 16 sub-districts around the capital city of Abidjan.[6]

After a listing exercise in the selected EAs, AUDRI selected adults for a panel study that started with a first in-person questionnaire from November 2019 to February 2020, followed by two Covid-round phone surveys in 2020, and a second in-person questionnaire from November 2022 to February 2023. The AUDRI data has been used so far to document the impacts of Covid (Dupas et al., 2023b) and test a novel methodology to elicit ranking from peers (Dupas et al., 2023a).

The survey experiments used in this paper were conducted during the second in-person survey, and in its immediate aftermath. All the fieldwork was conducted with the assistance of IHfRA, a multi-country survey outfit based in West Africa.

3.2   Data

We combine an array of data to test the hypotheses laid out above.

Survey Data   We have detailed survey data on the 2276 individuals that participated in the second in-person survey. Table A.1 summarizes some of the sample's characteristics, including information on their occupation, income, and education level. Notably, half of the sample is comprised of women, the average age stands at 40 years old, and 76% of the participants speak French at home to some extent. Education levels reveal that one third of the sample lack formal

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[6]See Dupas et al. (2023b) for full details on the sampling strategy.

schooling, with a noteworthy disparity between women (41%) and men (23%). Self-employment is the main source of income among our sample (26%), followed by salaried (19%), and casual workers (16%). Respondents report an average monthly income of 128,000 FCFA (206 USD) with a median of 65,000 FCFA (105 USD). In terms of ownership of assets, Table A.1 shows that 21% of the sample own the dwelling where they reside, 24% own land, and 12% own livestock. While our sample is poorer than the average Abidjan dweller, there is substantial heterogeneity within our sample, as shown in Figure A.1.

Experimental Vignettes   We embedded vignette experiments in the survey, exposing respondents to randomly selected scenarios and eliciting their beliefs about the likelihood of specific outcomes such as the individual in the scenario being invited for an interview, suspected of a crime, etc. The scripts for the vignettes are shown in Appendix E.

Real-stake experiments   Our key outcomes include *choices* made by study participants. We conducted 4 choice experiments with real stakes. The first three were embedded in the survey: a *documentary choice* experiment in which participants had to select what documentary to volunteer for, if any;[7] a *photo experiment* in which participants had to choose whether to invest time in providing a second photo (the photos were used to select documentary participants); and a *raffle experiment* in which participants had to decide in advance what prize to get in the event of winning (we gave prizes worth USD 85 to 2.7% of participants). The fourth choice experiment was conducted once everyone had been surveyed: a *mini-job experiment* in which participants choose what signal to send to a selection committee (we ultimately hired 102 (7.5%) applicants for a one-day job).

Scored Photographs   There is evidence to suggests that humans value seeing who they interact with because they can extract valuable information from appearance (e.g., Eckel and Petrie, 2011). We took 1230 photos from 1115 respondents. We then hired 102 individuals to rate the photos. We call these hired individuals "judges". They were hired via the "mini-job" experiment described above.

A given judge was given 100 photos to rate and rank. Each photo was rated (scored) on

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[7]We produced three documentary films now posted on the web.

many dimensions, and by at least two judges on any dimension; but a given judge was asked to rate photos on only one dimension.[8] The dimensions rated were: (i) socio-economic status, (ii) beauty, (iii) corpulence, (iv) likelihood of victimization. Beauty is included because it is known to predict a higher probability of being selected (e.g., Hamermesh, 2013; Scholz and Sicinski, 2015) and it is potentially correlated with SES status (e.g., Mobius and Rosenblat, 2006; Mocan and Tekin, 2010). Corpulence is included because, in our study population, it is commonly associated with high SES status (Manafe et al., 2022; Macchi, 2023). For (i)-(iii), the ranking was conducted in two stages. In the first stage, judges gave a rating from 1 to 10 to each photograph. The 100 photos were then divided into groups of at most ten pictures given the same rating. The judge was then asked to rank the pictures from 1 to 10 within each group. This process produces a near-complete ranking of all 100 pictures for each judge. For (iv), judges had to answer eight of our vignette questions on the likelihood that the person in the photograph would be victimized—e.g., suspected of burglary by the police, excluded from a social gathering, or passed on for a job interview.

We also asked 10 IHfRA enumerators to select potential documentary participants based on 634 photos. Each enumerator was given between 120 and 180 photos and asked to give it a score from 0 to 10, and told that that top-scorers would be invited to participate. Each photo was scored by two different enumerators.

Ultimately, our photo-level dataset has 12 variables (each showing averages across all judges who rated that variable for that question): SES, Beauty, Corpulence, 8 Victimization questions, and documentary-score by enumerator.

3.3 Experimental Design

Table 1 presents the key elements of the study, organized by hypotheses, and provides information on the timeline. It is easier to describe each component of the study as we discuss its results, so the detailed explanations for each component of the experimental design are embedded in the empirical results below. For a full account of the procedures done in chronological order, and for sample sizes, we refer the reader to Appendix D.

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[8]For respondents who provided two photographs, their two photos were never assigned to the same judge.

Table 1: Components of the study, by hypothesis

	Hypothesis	Experiment	Outcome
H1	1.1: People who look poor have fewer opportunities.	Vignettes	1. Probability of being victimized or socially excluded based on appearance
		Photo	2. Selection of job candidates based on candidate photo
	1.2: People with "poor" consumption patterns have fewer opportunities.	Vignettes	3. Selection of job candidates based on candidate choice between free meal and gift
H2	2.1: People distort their appearance to appear less poor.	Photo	4. Choice to take a second photograph to enhance physical appearance
			5. Probability of second photograph being perceived as less poor
	2.2: People are more likely to distort their consumption as observability increases.	Raffle	6. Choice between contributing to charity or receiving baskets of goods under different observability conditions
		Mini-job application	7. Choice between free meal and gift in a job application, varied by selection process
H$_{aux}$	People refrain from status-inconsistent behavior for fear of rejection.	Documentary choice	8. Match between chosen documentary type and actual socioeconomic status

4 Empirical results

We now present our empirical findings, not in the chronological order in which the questions and experiments were conducted, but in their logical order for the purpose of hypothesis testing.

4.1 Beliefs about the role of appearance

We start by investigating our first hypothesis, namely, that people *believe* that people who look poor receive a worse treatment in various human interactions. For this, we rely primarily on three distinct series of vignettes in which respondents were asked to report their beliefs about possible outcomes.

In the first series of vignettes, each respondent was presented with photographs of a person and asked the likelihood that the person would be: suspected of burglary by the police, suspected of stealing by the community, invited to a social gathering, and evicted by their landlord. There were 18 photographs of 6 different individuals (three men and three women), each photographed three times: with an outfit and environment suggesting either a low, medium, or high socio-economic status (SES)—an approach inspired by the empirical evidence provided by Athey et al. (2023).[9] Each respondent saw and answered the vignette question for 3 out of the 18 pictures (but was never shown the same model in different outfits). Pictures were randomized across respondents so as to net out model fixed effects.

Table 2: Appearances and social acceptance: Experimental Vignettes

	(1) Suspected by the police of burglarizing	(2) Suspected by the community of stealing	(3) Evicted by landlord	(4) Invited to social gathering
High SES picture	-0.30***	-0.72***	-0.72***	1.45***
	(0.08)	(0.08)	(0.08)	(0.08)
Low SES picture	0.03	0.07	0.17**	-0.56***
	(0.08)	(0.08)	(0.08)	(0.08)
Photo actor FE	Yes	Yes	Yes	Yes
Observations	6828	6828	6828	6828

Notes: An observation is a vignette depicting an individual in either a Low, Medium or High SES outfit and background. The omitted category is "Medium SES" photos. Respondents were asked to rate the likelihood of the outcome on a scale of 1 to 10. To facilitate interpretation, we standardize these responses to have a mean 0 and a standard deviation of 1 in the omitted category. Clustered standard errors at the respondent level.

Table 2 shows how answers to these vignettes depend on whether the respondent saw a high, medium, or low SES picture. Fixed effects are included for each of the six models. In the first three columns, we see that respondents predict a significantly lower probability of being suspected of a crime or evicted by a landlord when the photograph shows the model with a high SES attire and environment, compared to the same model in the medium SES condition (the omitted category). We also find that landlords are believed to be significantly more likely to evict a model in the low SES condition. In the last column, we find the opposite result for invitation to a social gathering: high SES pictures have a large and significant positive coefficient, indicating that respondents believe the person more likely to be invited, while the

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[9]See Appendix B for examples of photographs.

opposite is true for low SES pictures. These findings are robust by gender and by wealth terciles (see Table A.2) and are similar to those reported by Athey et al. (2023).

Perceived SES depends not only on what people chose to wear and what objects they surround themselves with. It can also be affected by behavior or consumption decision that are associated with low SES status. We test this in a second series of vignettes in which each respondent is presented with three hypothetical recruitment situations: a supervisor job in a supermarket chain; a paid job in a local NGO; and a position on the committee of a charitable organization. Respondents were provided with short fact sheets about hypothetical applicants and asked to rate the likelihood that a given applicant would be selected to interview for a given position. The fact sheet provides the applicant's education level, current occupation, and father's occupation. It also indicates what the applicant had chosen as compensation for their interview time: (a) a free meal or (b) a key-chain and tote bag. Information about the applicant's choice of the free meal is randomized across respondents: since poor applicants would presumably be more interested in the meal than the rather superfluous tote bag and key-chain, this choice signals low SES status. But conditional on education, occupation, and parental background, it should not be predictive of work productivity and, as such, should be dismissed as irrelevant for the hiring process.

In Table 3 we regress the respondents' predicted likelihood of getting interviewed on applicant characteristics. Education does not appear as regressor since all vignettes reported the same level of schooling (secondary school diploma). We find strong evidence that, for all the three jobs, choosing the free meal is regarded as a large negative signal: it decreases the perceived likelihood of being selected by between 0.35 standard deviations for the NGO interview and 0.42 standard deviation for the charity committee. These are large effects in absolute terms, but also compared to occupation and family background, which are not statistically significant except in the second column. Even there, the effect of choosing the meal is much stronger than that of being unemployed or in casual work. These findings are robust by gender and by wealth terciles (see Table A.3).

Table 3: Consumption choices as signals: Experimental Vignettes

	(1) Invited to join charity committee	(2) Interview for supermarket job	(3) Interview for NGO job
Selected free meal	-0.42***	-0.39***	-0.35***
	(0.03)	(0.03)	(0.03)
Occupation: Unemployed	0.04	0.28***	0.02
	(0.03)	(0.03)	(0.03)
Occupation: Casual Worker	0.03	0.18***	-0.03
	(0.03)	(0.03)	(0.04)
Father is civil servant	0.04	-0.01	0.03
	(0.03)	(0.02)	(0.03)
Observations	6828	6828	6828

Notes: An observation is the written profile of a hypothetical job applicant. The omitted categories are: selected tote bag and key-chain as compensation for interview time; occupation: bank employee; father's profession: farmer. Respondents were asked to rate the likelihood of the outcome on a scale of 1 to 10. To facilitate interpretation, we standardize these responses to have a mean 0 and a standard deviation of 1 for observations in the omitted categories. Clustered standard errors at the respondent level.

To get a sense for the economic magnitude of the coefficient reported in Table 3, we compare them with the estimated coefficients obtained in a series of vignettes we call the "benchmarking vignettes". In these vignettes, respondents are presented with a hypothetical individual CV, and asked the likelihood that the person with this CV would be selected for a specific position. There are six possible CVs and four positions: receptionist at the Abidjan Sofitel (a luxury hotel); committee member of a charitable organization; video contributor to an Abidjan documentary on 'Voices of the poor'; and video contributor to an Abidjan documentary on 'Voices of the mighty'. The positions and CVs were randomized across respondents so as to net out individual effects. In Table A.4 we regress the 1 to 10 likelihood rating reported by respondents on the three pieces of information provided to them: whether the applicant has completed secondary education or not; and whether their primary occupation is salaried, unemployed/housewife, or self-employed. The omitted category is salaried with no formal education. Even though none of the situations we consider require secondary education, we find that respondents with secondary education are systematically favored for all four posts. In contrast, those who are unemployed are viewed as less likely candidates for the hotel job, the charity committee, and

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the 'Voices of the Mighty' video, but seen as more appropriate for the 'Voices of the Poor' video. Self-employed candidates are similarly seen as less likely for the charity committee and the 'Voices of the Mighty' video, but more likely candidates for 'Voices of the Poor'—a finding that accords well with the fact that, in Abidjan, salaried workers earn, on average, a higher income than those who are self-employed.

We can now gauge the economic magnitude of the estimated coefficients in Table 3 by comparing them to those reported in Table A.4. For the committee position, having chosen the key-chain and tote bag over the free meal increases the applicant's perceived likelihood of being selected by 0.42 standard deviation, which is close to the effect of having secondary education (+0.50 standard deviation). It is thus a large, economically significant effect.

To summarize, the evidence we have provided so far demonstrates that appearances are believed to matter for a wide range of social and economic outcomes, and that the effect of appearances on predicted outcomes is economically significant.

4.2   Appearance and actual job opportunities

The vignettes experiments above have shown that respondents *believe* that people who look poor receive a worse treatment in various human interactions. Are they right? To test this, we conducted a consequential experiment in which enumerators from our partner survey firms were asked to *screen* applicants for video documentaries. Specifically, enumerators were given photos submitted by applicants, and asked to score applicants on a scale of 0 to 10 regarding their suitability for one of three documentaries. This was consequential because we ultimately invited the top-scorers to submit a one-minute video recorded on a phone. These short videos were subsequently combined to produce three documentary videos entitled 'Babi la joie' (about fun opportunities in Abidjan), 'Babi la dure' (about the difficulties of life in Abidjan), and 'Abidjan terre d'opportunité' (about the economic opportunities offered by Abidjan).[10]   As promised to respondents, these short documentaries have since been made public on the Stanford AUDRI website and on the IHfRA website. Given this, the enumerators' screening process mimics the recruitment for a short job or 'gig'. Each of ten enumerators scored 120 randomly selected photographs. Because these were photographs from the AUDRI sample, we have de-

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[10] "Babi" is the nickname that Abidjan's inhabitants have for their city.

tailed information on the SES status of each individual. More importantly, we separately and independently hired people from the community (we call them "judges") to rank all photographs for SES, beauty, corpulence, and victimization risk.

Table 4: Appearances and Documentary Opportunities: Suitability scores given by enumerators

	(1)	(2)	(3)
	Dep. Var: Selection score given by enumerators		
	Babi la joie	Terre d'opportunité	Babi la dure
SES score	0.068	0.190**	0.011
	(0.073)	(0.086)	(0.065)
Beauty score	0.229***	0.096	0.057
	(0.087)	(0.084)	(0.074)
Corpulence score	0.069	0.335***	0.009
	(0.074)	(0.099)	(0.071)
Female	-0.333	-0.582**	-0.168
	(0.233)	(0.276)	(0.208)
Log(age)	-1.948***	-1.269***	0.253
	(0.342)	(0.481)	(0.337)
Mean score	6.16	6.37	7.02
Observations	224	150	259

Notes: An observation is a photo of a study participant who agreed to have her photo rated. Each column shows a regression for a specific subsample. The dependent variable is the average selection score (from 1, lowest, to 10, highest) given by enumerators to the photograph for a specific documentary. SES, beauty, and corpulence scores are average scores (between 0 and 1) given to photograph by a separate panel of judges drawn from the study population. Column 1 focuses on the sample of individuals randomly selected for the "photo" experiment; their photo was rated for the documentary "Babi la joie", about fun in Abidjan. Column 2 focuses on the sample of individuals randomly selected for the "documentary choice" experiment, and who signed up for the documentary "Terre d'Opportunité", about Abidjan as a land of opportunity. Column 3 focuses on the sample of individuals randomly selected for the "documentary choice" experiment, and who signed up for the documentary "Babi la dure", about hardship in Abidjan.

In Table 4, we regress the enumerator scores about the applicant's suitability for each documentary, on the applicant's appearance scores separately assigned by the judges on the basis of their photograph. Gender and age (in logs) are included as controls, although not including them does not qualitatively change the findings. We see that appearance plays an important role for being selected to two of the documentaries. In the first column, the dependent

variable is the enumerator score given for the first documentary on having fun in Abidjan. Photographs that were ranked higher in terms of beauty are significantly more likely to be scored highly by enumerators, together with being young. This conform with expectations: 'fun' is associated with youth and beauty. In the second column, the selection is for the documentary on Abidjan Land of Opportunity. As could be anticipated, photographs ranking higher in terms of socio-economic status and corpulence (an indicator of wealth) are more likely to be selected for this documentary. Men and young individuals are also more likely to be selected— suggesting social stereotypes emphasizing the entrepreneurial dynamism of men. The surprise is the third column where the selection is for a documentary on economic difficulties faced by Abidjan residents. Here we would expect a low SES to be associated with a higher probability of selection, but it is not: the SES coefficient is nowhere near statistical significance.[11]

4.3   Keeping up appearances

Having established the importance of perceived social rank in people's access to opportunities, we now turn to the question of how this influences individuals' behavior.

4.3.1   No evidence of resignation

We begin by checking whether respondents internalize the effect that their SES and appearance has on their chance of success if they apply to a position or task. If respondents anticipate to be rejected with high probability, they may opt not to apply, as was shown in a recent study: Brazilian job seekers studied by Angeli et al. (2024) refrain from applying to jobs they feel discriminated for because of their address. To investigate this auxiliary hypothesis (H_{aux} in Table 1), we run a "documentary choice" experiment with a random third of the sample. We first ask permission to take a picture of the respondent for our own records. We then ask respondents whether they would like to be considered for a documentary on the hardships of life in Abidjan ('Babi la dure'), or for a documentary on the opportunities that Abidjan offers ('Abidjan terre d'opportunité'), or neither. We randomize whether their choice is made in private (directly on a tablet, without the enumerator observing) or whether it is observed by the enumerator. This design allows us to test whether respondents choose the documentary

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[11]This pattern remains if we drop the beauty and corpulence scores, and if we estimate the regression separately for men and women.

that best reflects their SES status, i.e., whether they apply for the 'job' for which they are most likely to be selected—and whether this is more pronounced when their choice is visible to the enumerator, at the tail end of a long survey in which their income, assets, and other economic opportunities were discussed.

In Table A.5, we regress whether their documentary choice on the 'Private' treatment, as well as on the average SES, beauty and corpulence scores given to their photograph by judges drawn from the AUDRI sample. While overall respondents were much less likely to volunteer for the "opportunity" documentary than for the "hardship" one, possibly owing to the fact that our sample is drawn from the lower segment of the SES distribution within Abidjan, we do no find any evidence that respondents disproportionately refrain from applying if their photograph is poorly ranked in terms of SES. This indicates that, while the fear of rejection may overall induce respondents in our sample to self-censor, this self-censoring is not tied to perceived SES *within* our sample. We also do not observe any difference when the documentary choice is made in private, i.e., hidden from the enumerator.

4.3.2   Manipulation of physical appearance

We now turn to our second key hypothesis, namely, that people change their appearance or consumption to avoid being victimized for looking poor. Put another way, we experimentally test whether people attempt to, and succeed at, "improving" their perceived SES when they have incentives to do so. For this, we run a second photo choice experiment with a random third of respondents.

We first ask permission to take a picture of the respondent for our own records. We then ask a random half of respondents whether they consent to their picture being shown to a local panel that will select volunteers for a documentary about how people of Abidjan have fun. All respondents are then offered the possibility to have the enumerator come back later to take another picture of them, i.e., after they have had time to prepare themselves.[12] Although this service was free, it nonetheless involves a time cost. We also gave respondents the option to send us an alternative photo digitally. Presumably, those who avail themselves of either opportunity do so to improve their appearance.

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[12]The scripts used are shown in subsection E.2.

Table 5 examines whether respondents schedule a second photo shoot, and whether a second photo is produced.[13] We see that, when the photo will not be used for screening (control condition), only 17% of people plan to, and only 16% of people do, allocate some time to have a second photograph taken. However, when respondents know that the photo will be used in selecting volunteers for a documentary on Abidjan, these figure increases by 6.6 and 8.1 percentage points, respectively. The share of people who invest in producing a second photo increases by 50% (column 6 of Table 5). This is consistent with the hypothesis that respondents are willing to incur a cost to improve their appearance so as to increase their chances of an opportunity they regard as valuable.

Table 5: Photo Experiment: Results

| | (1) | (2) | (3) | (4) | (5) | (6) |
	Scheduled second photoshoot			Second photo was produced		
Screening	0.075**	0.063*	0.066**	0.090***	0.083**	0.081**
	(0.034)	(0.033)	(0.033)	(0.034)	(0.033)	(0.032)
Mean control	0.17	0.17	0.17	0.16	0.16	0.16
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sous-prefecture FE	No	Yes	Yes	No	Yes	Yes
Enumerator FE	No	No	Yes	No	No	Yes
Observations	563	563	563	563	563	563

An observation is a study participant selected for the documentary experiment. Columns (1) through (3) display the results of regressing the variable "the respondent scheduled a second photo shoot" on a dummy equal to 1 if the respondent was assigned to the screening treatment, controlling for the following characteristics of the respondents: female, older than 50 years old, speaks French at home, Ivorian, followed higher studies, and tercile of wealth distribution within sous-prefecture. Columns (4) through (6) display the results of regressing the variable "a second photo was produced", controlling for the same variables. Sous-prefecture fixed effects and enumerator fixed effects included as indicated at the bottom of the table.

Table 6 tests for heterogeneity in these results by socio-economic status. To proxy for SES, we create a wealth-per-capita index that combines information about the respondents' self-reported income, household expenses, assets, and household size. We then use this index to define the wealth tercile of the respondent in their location (i.e., their sous-prefecture). We run the specification shown in column 6 of Table 5 separately by tercile of the wealth-per-capita index. In line with the predictions of our model, we find that the results are stronger and

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[13]70% of second photos were produced through a second photoshoot and 30% were sent digitally by the respondent. The results are unchanged if we focus on participation in the second photoshoot.

significant only for individuals in the middle of the wealth distribution: they are more likely to seek to modify their appearance to increase their chances at being selected for the documentary than those at the bottom (too poor to attempt to "pool") and those at the top (who do not need to signal). The magnitude of the effect for the middle group is quite large economically: Individuals told about the screening are 15 percentage points (+150%) more likely to have participated in the second photo shoot than those who were not.

Table 6: Heterogeneity photo experiment

	(1)	(2)	(3)	(4)	(5)
	Dep. Var: Second photo was produced				
	Men	Women	Bottom wealth tercile	Middle wealth tercile	Top wealth tercile
Screening	0.09*	0.05	0.04	0.15**	0.08
	(0.05)	(0.05)	(0.06)	(0.06)	(0.07)
Mean control arm	0.12	0.19	0.23	0.10	0.12
Observations	285	278	210	175	178

Notes: The table display the results of regressing the variable "a second photo was produced" on a dummy equal to 1 if the respondent was assigned to the screening treatment, for the samples at the top of each column. All regressions include sous-prefecture and enumerator fixed effects.

Figure 1 uses the SES ratings of the photographs themselves (rather than the survey-based measured of SES) to study who selects into submitting a second photo. The figure shows that the SES-rating distribution of photos among those who elect to submit a second photo is clearly to the left (lower perceived SES) of the distribution of photos for those who do not submit a second photo. This confirms the results in Table 6.

We then verify that the second photos are indeed "better", i.e., ranked higher. We check this in three ways. First, we check whether the second photographs are ranked "higher" by judges. This can be seen graphically in Figure 1. The distribution of SES ratings of second photos is clearly to the right of the distribution of first photos, closing about half of the gap with those who do not submit a second photo.

Figure 1: Selection into Second Photoshoot, and Rating of Second Photos
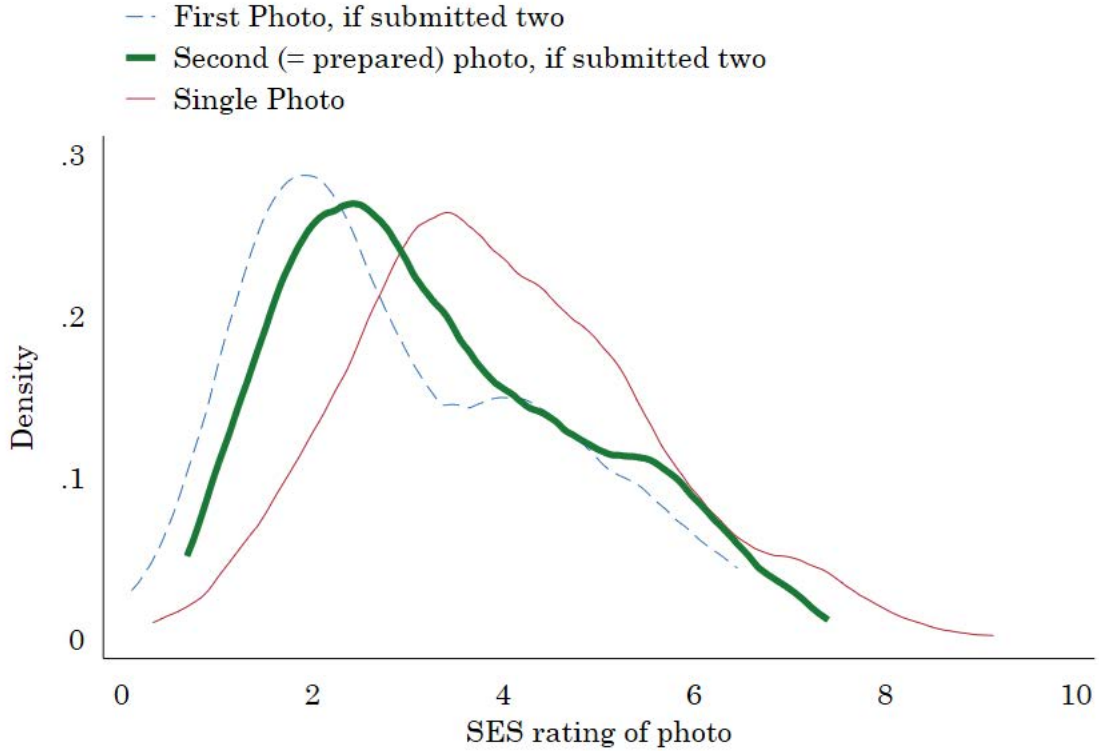
In Table 7, we regress the average SES score given to a photo on a dummy equal to 1 if the photograph shown is a "prepared" (= second, scheduled photo), restricting the sample to photos from participants who chose to submit a second photo, so that we can include participant fixed effects. Second photos are systematically associated with higher perceived SES and beauty, suggesting that respondents are indeed able to improve their appearance along these dimensions. Reassuringly, we do not see a difference in corpulence rankings, which is something that individuals could not possibly have manipulated between two photoshoots.

Table 7: Manipulation of rankings between photo-shoots

	(1) SES rank	(2) Beauty rank	(3) Corpulence rank
Prepared photo	0.397**	0.602***	0.165
	(0.179)	(0.115)	(0.146)
Mean	3.66	5.10	4.46
Observations	230	230	230
Respondent FE	Yes	Yes	Yes

An observation is a respondent-photograph. Only photographs of respondents who provided two photographs are included. The table shows the results of a respondent fixed effect regression of SES, beauty, and corpulence average ranks reported by independent judges on a dummy equal to 1 for the second (prepared) photograph. Ranks are measured on a scale from 0 to 10, with higher values assigned to individuals associated with higher socioeconomic status (SES)/greater attractiveness/corpulence.

Second, we test whether the "prepared photos" are associated with better employment opportunities and social acceptability. For this, we asked our hired judges to answer the vignette questions we used in section 4.1 about the photos. Results are presented in Table 8. We see that prepared photographs predict a higher probability of being interviewed for a desirable receptionist job, invited to a charity committee, selected for a documentary entitled 'Voices of the mighty', or invited to a social gathering. Together with the findings from Table 7, these results confirm that survey respondents *can* readily modify their appearance to increase their perceived SES, and in so doing they are less likely to be excluded from valuable opportunities.

Table 8: Does manipulation of physical appearance improve likely outcomes?

	(1) Receptionist Sofitel	(2) Charity committee	(3) Voice of the poor	(4) Voice of the mighty	(5) Suspected by the police of burglariz-ing	(6) Suspected by the commu-nity of stealing	(7) Evicted by landlord	(8) Invited to social gathering
Prepared Photo	0.59***	0.54***	0.27	0.63***	0.30	0.23	-0.04	1.16***
	(0.21)	(0.19)	(0.16)	(0.16)	(0.23)	(0.21)	(0.21)	(0.23)
Mean first picture	4.37	4.19	4.58	3.87	4.11	4.18	4.75	4.26
Respondent FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	230	230	230	230	230	230	230	230

An observation is a respondent-photograph. The table shows the results of the second picture on the perceived likelihood of facing the events indicated on each column. The likelihood is measured on a scale from 1 to 10, with higher values associated to a higher likelihood. Respondent FE (photographed individuals) are included as indicated at the bottom.

4.3.3 Signaling via consumption

The results so far suggests that individuals can manipulate their appearance to successfully improve their perceived SES, which, in turn, reduces their risk of victimization and increases the likelihood of getting selected for a position. We next test whether they also distort their *consumption* choices in order to signal a higher SES. For this, we conducted two choice experiments with the entire sample: a "mini-job" experiment and a "raffle" experiment.

Mini-job experiment The mini-job consists in ranking photographs of other respondents to produce data that we have used in previous sections. This 3-4 hour job was well paid (14,000 FCFA, which is equivalent to USD 23) and therefore attractive to our study population.

To the respondents who expressed interest in applying, we explained that, in addition to the wage payment, selected applicants would receive, at the time of performing the work, one of two complimentary gifts of equal value. They were then asked to choose which one they would prefer: either a complimentary meal, or a pen of equal monetary value. Respondents were randomly assigned to one of two treatment groups, varying what respondents knew about the hiring selection process. In the control group, respondents were told that, since the job was likely to be oversubscribed (only 102 applicants (7.5%) ended up being hired), selection for the mini-job would be random. In contrast, applicants in the treatment group were told that selection would be based on their suitability for the job, which would be determined based on their answers to a small set of questions.[14] These questions were then immediately followed by the choice between the meal or the pen.[15]

This design allows us to test whether respondents distort their choice when it is potentially observable by the selection committee (H2). We hypothesize that in the control group, when respondents know for sure that their meal-or-pen choice will not influence their probability of being selected, they will opt for what genuinely appeals to them. In contrast, we anticipate that applicants in the treatment group are more likely to choose the status good (pen) if they internalize the fact, shown in section 4.1, that selecting the free meal is regarded as a negative signal.

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[14]The questions were: (a) Do you have any experience with tablets, computers, or smartphones; and (b) What is your availability for the job? These questions were only asked to applicants assigned to the treatment ("screening") group.

[15]The scripts used for this experiment are given in Appendix E.

The results are shown in Table 9. The majority (57%) of respondents in the control group chose the free meal. This probability decreases by 8 percentage points (significant at the 5% level) when respondents are led to believe that their choice will be observed by the hiring committee. Adding controls and a battery of fixed effects reduces the coefficient to 6 percentage points, but it remains significant both statistically and economically.

Table 9: Distortion in Consumption Choices

	(1)	(2)	(3)	(4)
	Dep. variable: Respondent selected free meal			
Screening	-0.08***	-0.07***	-0.07**	-0.06**
	(0.03)	(0.03)	(0.03)	(0.03)
Mean random arm	0.57	0.57	0.57	0.57
Controls	No	Yes	Yes	Yes
Sous-prefecture FE	No	No	Yes	Yes
Enumerator FE	No	No	No	Yes
Observations	1343	1343	1343	1343

An observation is a respondent selected for the "mini-job" choice experiment. The table displays the coefficients of regressing "selected free meal" (rather than the non-necessity good) on a dummy equal to 1 if the respondent was assigned to the screening treatment (i.e., they were told hires would not be made at random but based on their answers). Control variables include dummies for: female; older than 50 years old; speaks French at home; Ivorian; followed higher studies; casual worker; and tercile of wealth distribution within the sous-prefecture.

In Table 10, we examine whether the signaling value of this consumption choice varies with socio-economic status, using the same specification as in Table 6. The results suggest that the signaling value of consumption varies with socio-economic status: only individuals in the two bottom terciles distort their consumption for signaling purposes, and those in the middle tercile do so more than those in the bottom tercile. These findings fit our model predictions: poorer individuals are those most likely to distort their consumption choices for signaling purposes; and they distort their consumption choices in the direction of those taken by higher SES individuals, who choose the pen with a much higher probability (55 to 57%) irrespective of whether this choice is observed or not.

Raffle experiment   At the onset of the AUDRI wave 2 survey, respondents were informed that, as compensation for their participation to the survey, their name would be entered in a raffle to be held in May 2023. Each respondent was then asked to choose which of three possible prizes they would want to receive, were they to win the raffle a few months later. Each prize

Table 10: Distortion in Consumption Choices: Heterogeneity

	(1)	(2)	(3)	(4)	(5)
		Dep. Var: Respondent selected free meal			
	Men	Women	Bottom wealth tercile	Middle wealth tercile	Top wealth tercile
Screening	-0.06*	-0.09**	-0.11**	-0.12**	-0.01
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
Mean random arm	0.48	0.66	0.63	0.62	0.46
Observations	713	630	435	449	459

An observation is a respondent selected for the "mini-job" choice experiment. The table displays the coefficients of regressing "selected free meal" (rather than the non-necessity good) on a dummy equal to 1 if the respondent was assigned to the "Screening" condition for the sample indicated in each column.

had the same value of 50,000 CFA—around 85 USD, a non-negligible amount for our study population which, pre-Covid at baseline, had an average monthly income of 106 USD (median 54 USD). The three prizes were: (1) a basket of basic necessities; (2) a basket of non-necessities; and (3) contributions to three local NGOs.[16]

Respondents were randomized into three observability treatments. In the first treatment, respondents were asked to enter their prize choice privately onto the enumerator's tablet; in the second treatment, they reported their choice of prize to the enumerator, who recorded it; and in the third treatment, they recorded their choice by ticking one of three boxes in a sheet of paper containing their name on the top row, and those of other households in their neighborhood on blank rows below. This was designed to induce respondents to believe that their choice would be observed by others in the community, when those others are surveyed and asked to fill their own choice on the form—as in Bursztyn and Jensen (2015).

This design allows us to measure respondents' willingness either to deviate from their material self-interest by gifting their prize to others, or to opt for the more luxurious second basket. By comparing their private choices to their choices in the enumerator and public treatments, we can test whether and how respondents deviate from their optimal consumption plan when their choice is observed by others—as suggested by our model. The second hypothesis H2 predicts that, when observed, respondent either give more to charity—the ultimate signal since

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[16]See Appendix Figure A1 and A2 for photographs of the two consumption baskets. The three chosen NGOs were: an orphan and single mother care center in Abobo; a similar center in Yopougon; and the Drogba Foundation, a philanthropic organization founded by a local sport celebrity. A large fraction of our study sample resides in the low income neighborhoods of Abobo and Yopougon.

it generates no material utility for the giver—or choose the more luxurious basket to signal a higher income rank.

We start by verifying that luxury goods and charitable contributions are associated with high SES status. To this effect, we use respondents whose choice was private, since these observations are not affected by the public treatment/signaling effect of consumption. We start by non-parametrically regressing dummy variables for choosing the basic basket, the luxury basket and the charitable contribution on the (log of the) respondent's budget, defined as the reported monthly income of the respondent plus the value of the raffle prize (50,000 CFA). Results, shown in Figure A.2, indicate that the proportion of respondents who choose the charitable contribution increases dramatically with income, while the proportion of those who choose the basic basket drops. Contrary to our intentions, the luxury basket we designed does not appeal to higher income respondents—and can therefore not serve the role of signal good. With hindsight, this is perhaps unsurprising: since giving money away to charity generates no material benefit for the consumer, it is the perfect 'frivolous expenditure' and, as such, can serve the role of ultimate status symbol. Charitable giving is indeed commonly observed in our study population.

To confirm this result, we regress choosing charitable giving on an expanded list of regressors that capture other aspects of high SES: wealth (divided into terciles); education (a dummy for having more than secondary education); old age and being female (both of which are, in the context of our study, associated with a lower SES); and cultural identity (capture by a dummy for speaking French at home and for holding an Ivorian citizenship). Results are shown in Table A.6. We see that, as anticipated, respondents who are wealthier, better educated, and male are more likely to choose the charitable contribution. From this we conclude that this choice signals a higher socio-economic status. In the rest of the section, we therefore focus on the charitable contribution as the relevant signal good.

Next we examine whether respondents are more likely to choose the charitable contribution—the signal good—when their choice is de facto observed by the enumerator, or potentially observed by others in the neighborhood. In the context of our study, we expect the enumerator effect to be stronger for two reasons: (1) given our experimental design, observability by the enumerator is certain for the respondent while observability by neighbors is not; and (2) we know from other research (Dupas et al., 2023a) that our urban respondents know few of their

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neighbors and thus may be less concerned about making a bad impression on their peers.

Table 11: Signaling through Charity Contributions

	(1)	(2)
	Dep Var: Chose charity contribution over basket of goods	
Public (Enumerator)	0.037*	0.036*
	(0.020)	(0.020)
Public (Neighbors)	0.022	0.023
	(0.020)	(0.020)
Mean private	0.177	0.177
Controls	No	Yes
Sous-prefecture FE	Yes	Yes
Observations	2276	2276

An observation is a respondent in the raffle choice experiment. In the private treatment respondents select directly the item they want. In the enumerator treatment, respondents must tell the enumerator their choice. In the public treatment, respondents write down their choice in a list that contains the names of some of their neighbors. Controls include dummy variables indicating the following characteristics of the respondents: female; older than 50 years old; speaks French at home; Ivorian; more than secondary education; and terciles of wealth distribution within the sous-prefecture. Sous-prefecture fixed effects and enumerator fixed effects included as indicated at the bottom of the table.

The results, presented in Table 11, show that respondents are 3.4 percentage points more likely to choose a signal good when their choice is observed by the enumerator. This effect is large—19% of the control value—and statistically significant. A similar—but smaller and not statistically significant—coefficient is found when the choice may be observed by neighbors. Furthermore, as demonstrated in Table A.7, the behavior documented in Table 11 is correlated with the respondent's behavior outside the experiment: those who choose the signal good in the enumerator and public observability treatments tend to also report making gifts and transfers to other households. There is no response to treatment for those who do not otherwise make transfers to others.

4.3.4 Income elasticity of signaling consumption

We next test an important implication of our model, namely that for low SES individuals, the consumption of signal goods has an income elasticity less than 1, which implies that the *share* of consumption expenditures that they devote to the signal good *falls* with total expenditures. In contrast, for high SES individuals these goods have an income elasticity larger than 1 but they

do not serve as signal goods. We have already shown that *private* consumption of the signal good increases with SES. We now examine whether the budget share assigned to charitable giving falls with the total expenditure budget of the household.[17]

Formally, let Y_i denotes the total monthly income reported by respondent i in the survey questionnaire, which we take as the respondent's monthly consumption budget. Winning the lottery T adds 50,000 CFA Francs to this budget, creating a consumption budget $B_i = Y_i + T$. When the respondent gives away the lottery winnings to charity, this amounts to a share $S_i = T/B_i$ spent on signal goods. Regressing S_i on B_i shows whether the share of signal goods in consumption falls with total expenditures, as predicted by signal theory for poor households, or increases with it, as predicted by standard consumer demand theory. Since our study population is predominantly poor, we expect the negative effect to dominate if signal theory is true. Furthermore, we expect this negative effect to be magnified by the observability treatments.

To investigate this, we estimate three separate non-parametric regressions of S_i on the log of B_i, one for each of the observability treatments. The results are plotted in Figure 2. We see that, at the lower end of the total budget distribution, there is a definite increase in the average budget share devoted to the charitable contribution in the enumerator and public treatments, compared to the private treatment. These results conform with KUA theory: it is those respondents most desperate to appear richer than they are who switch from the basic necessities to charitable giving when this decision is observable. This difference is statistically significant.

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[17]Since the amount that could be given to charity was fixed, it is mechanical that, conditional on donating to charity, the donation will represent a decreasing share of income. But since donating is a choice, we can test whether, on average across all households in our sample, the poor end up donating a greater share of their income than the less poor.

Figure 2: Share of budget spent on charity, by poverty level and treatment group
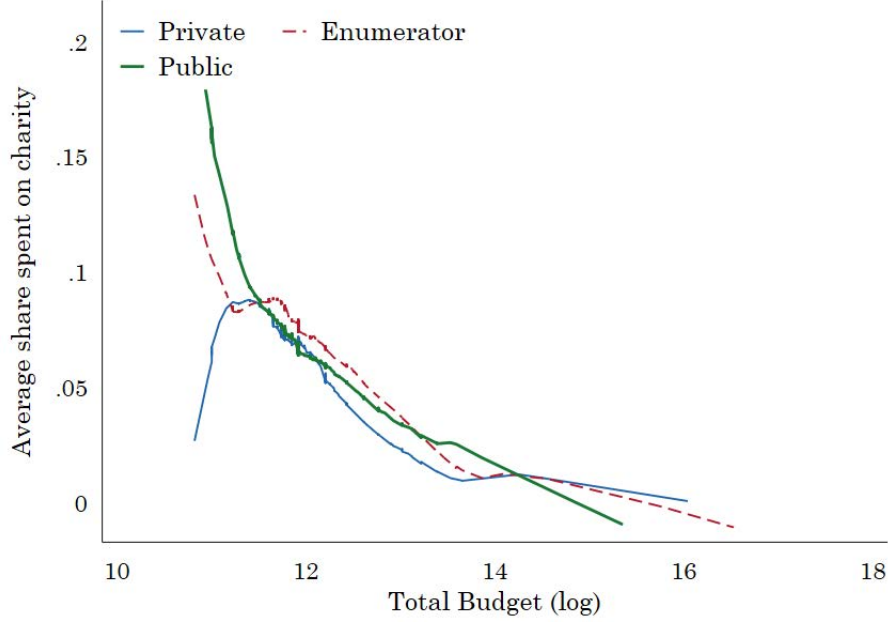
Each curve in the Figure was produced by the lowess command in Stata. The majority of the observations are concentrated in the 11-13 interval of total budget values. Since the lowess command does not generate confidence intervals, we also estimated each curve separately using a fractional polynomial approach. This approach reproduces the divergence at levels of total budget less than 11.5 that is observed in the above Figure, and the 95% confidence intervals for the Public and Private treatments do not overlap, implying that the difference is statistically significant.

5   Conclusion

This paper describes the results of a series of experiments and measurements aimed at assessing the role that appearances play in social victimization and the efforts that individuals undertake to avoid such victimization. We do so in an urban population that is relatively poor.

Our findings indicate that respondents believe that appearances affect outcomes, and they are willing to distort their consumption choices toward observable goods (e.g., attire, frivolous goods like the pen or tote bag, or charitable contributions) in order to signal a higher SES status. A benevolent external observer would see such distortions as welfare-reducing and may wish to intervene to restrict the choice of poor individuals to basic goods. By doing so, they would exhibit the kind of paternalism that is often embedded in welfare programs, and possibly reinforce social stigma.

These findings call for more work on assessing the magnitude of the distortions to material

welfare that low income households in an urban setting are willing to incur in order to protect themselves against social victimization. In particular, research is needed to ascertain whether these considerations reduce the take-up of social welfare programs even in low to middle-income countries. Our findings, however, need not apply to rural settings where households seem to know a lot more about others' material welfare (Alatas et al., 2012) and where collectivism often replaces the individualism that prevails in cities (Thomas et al., 2020). There, victimization of the poor may take other forms (e.g., Dasgupta, 1995; Walker and Bantebya-Kyomuhendo, 2014).

These findings also bring up important questions about the welfare implications of consumption smoothing (or lack thereof). Economists typically interpret a lack of consumption smoothing as an optimization failure. Our model and findings suggest that exhibiting uneven consumption patterns could be an optimal strategy: if consuming on par with my neighbors in the week that follows payday (or the harvest) enables me to keep up appearances and avoid victimization, it may well be worth the cost of skipping meals towards the end of the month (season), especially if skipping meals is not easily observable by neighbors. Such empirical patterns have been previously explained by impatience (Parker, 2017), behavioral traits such as lack of self-control, planning failures, limited attention (Reis, 2006; Augenblick et al., 2023), or savings constraints (Somville and Vandewalle, 2023). Our results suggest an additional explanation with much different implications for welfare.

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Appendix

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A Appendix Figures and Tables

Figure A.1: Heterogeneity in Income within the Study Sample
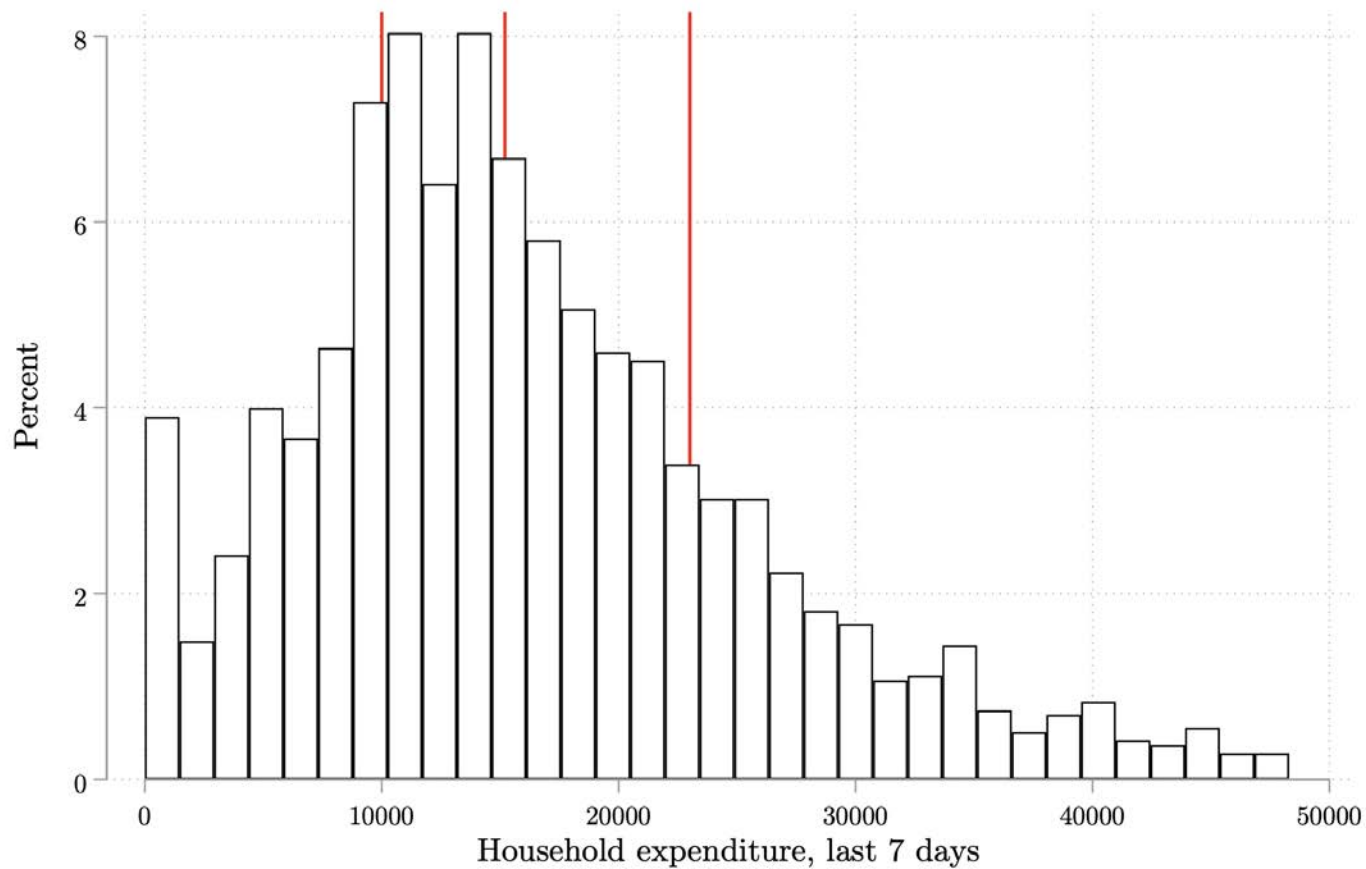
Notes: Red lines indicate quartiles.

Figure A.2: Raffle choice by income level
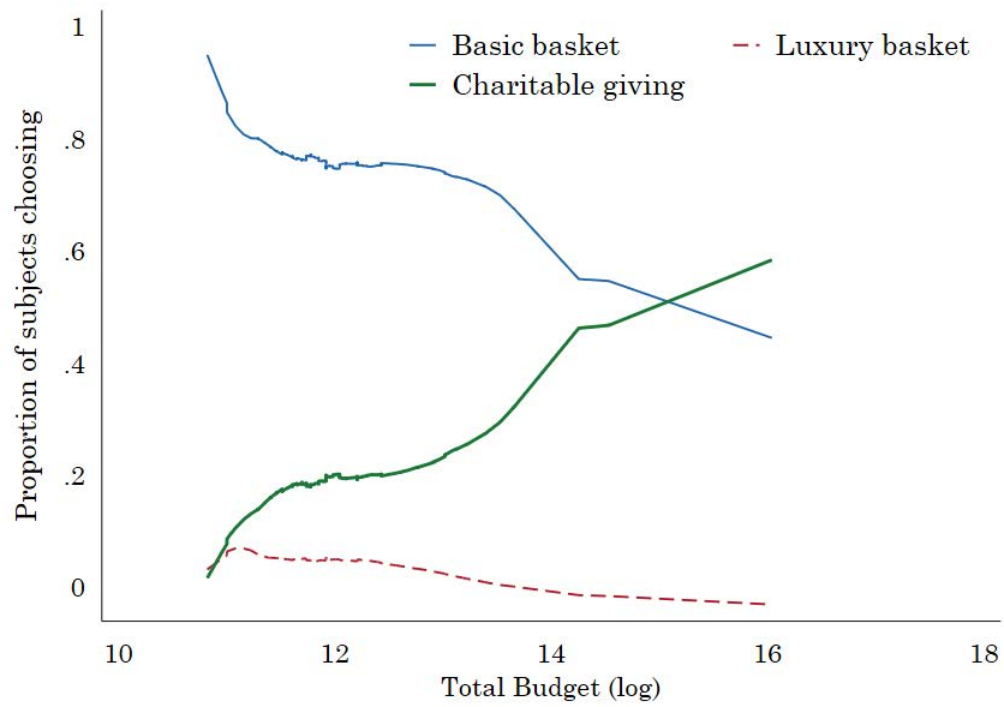
Notes: Sample restricted to respondents whose choice was private. The figure shows non-parametric (lowess) regressions of the dummy variables for choosing the basic basket, the luxury basket and the charitable contribution on the (log of the) respondent's budget, defined as the reported monthly income of the respondent plus the value of the raffle prize (50,000 CFA).

Table A.1: Summary statistics

	Mean	Std.Dev
Female	0.50	0.50
Age	40.27	12.89
Age<30	0.21	0.41
Age>50	0.20	0.40
Always lived in current residence	0.10	0.30
Years in current residence if born elsewhere	10.72	10.68
<5 years in current residence if born elsewhere	0.34	0.47
Always lived in current sous-prefecture	0.25	0.43
Speaks French at home	0.76	0.43
Ivorian	0.89	0.32
Marital status		
Single	0.30	0.46
Married/cohabiting	0.62	0.49
Highest education level		
No formal education	0.32	0.47
Some primary	0.26	0.44
Some secondary	0.33	0.47
Above secondary	0.10	0.30
Employment and income		
Self-employed	0.26	0.44
Salaried	0.19	0.39
Casual	0.16	0.36
Monthly income (x1000 FCFA)	128.47	522.65
HH expenditure, last 7 days (x1000 FCFA)	21.55	30.09
Assets		
Owns dwelling	0.21	0.41
Owns land	0.24	0.43
Owns livestock	0.12	0.33
Dwelling		
Has access to running water inside the dwelling	0.47	0.50
Has electricity	0.98	0.13
Has a toilet inside the dwelling	0.48	0.50
Transfers		
Charity contributions (x1000 FCFA)	3.86	14.50
Has given money to others	0.34	0.47
Has received money from others	0.32	0.47
Observations	2276	

Notes: Winsorized monthly income to the 99th percentile. Household expenditure over the last 7 days includes expenditure on food.

Table A.2: Appearances and social acceptance (Experimental Vignettes)

	(1)	(2)	(3)	(4)	(5)
	Dep. Var: Likelihood to be Invited to Social Gathering				
	Men	Women	Bottom wealth tercile	Middle wealth tercile	Top wealth tercile
High SES picture	1.40***	1.49***	1.44***	1.59***	1.30***
	(0.11)	(0.11)	(0.13)	(0.14)	(0.14)
Low SES picture	-0.62***	-0.49***	-0.53***	-0.54***	-0.61***
	(0.11)	(0.11)	(0.14)	(0.14)	(0.14)
Photo actor FE	Yes	Yes	Yes	Yes	Yes
Observations	3408	3420	2277	2277	2274

Notes: An observation is a vignette depicting an individual in either a Low, Medium or High SES outfit and background. The omitted category is "Medium SES" photos. Columns indicate characteristics of the *respondent*. The independent variables are characteristics of the picture that respondents rated. The wealth-per-capita index is computed using information on the respondents' self-reported income, household expenses, assets, and household size.

Table A.3: Appearances and social acceptance: Experimental Vignettes

	(1)	(2)	(3)	(4)	(5)
	Dep. Var: Profile Invited as Member of Charity Committee				
	Men	Women	Bottom wealth tercile	Middle wealth tercile	Top wealth tercile
Selected free meal	-0.40***	-0.45***	-0.36***	-0.46***	-0.44***
	(0.04)	(0.04)	(0.05)	(0.05)	(0.04)
Occupation: Unemployed	0.03	0.04	0.06	0.02	0.04
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
Occupation: Casual Worker	0.09*	-0.02	0.05	0.04	0.02
	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)
Father is civil servant	0.10***	-0.02	0.04	0.01	0.06
	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)
Observations	3408	3420	2277	2277	2274

Notes: Columns indicate the characteristics of the *respondent*. The independent variables are characteristics of the picture that respondents rated. The wealth-per-capita index is computed using information on the respondents' self-reported income, household expenses, assets, and household size.

Table A.4: Perceived Returns to Social Rank (Benchmarking Vignettes)

	(1) Receptionist Sofitel	(2) Charity committee	(3) Voice of the poor	(4) Voice of the mighty
Secondary education	1.35***	0.50***	0.44***	0.76***
	(0.03)	(0.02)	(0.03)	(0.02)
Occupation: Unemployed	-0.06**	-0.35***	0.10***	-0.74***
	(0.03)	(0.03)	(0.03)	(0.03)
Occupation: Self-employed	0.01	-0.15***	0.06**	-0.24***
	(0.03)	(0.02)	(0.03)	(0.03)
Observations	6828	6828	6828	6828

Notes: The omitted category is "Salaried" with no education. Respondents were asked to rate the outcomes on a scale of 1 to 10. To facilitate interpretation, we standardize these responses to have a mean 0 and a standard deviation of 1 for observations in the omitted categories. Clustered standard errors at the respondent level.

Table A.5: Self-selection into documentaries

	(1) All	(2)	(3)	(4) Private Choice	(5)	(6)
	Babi la dure	Terre d'opportunité	None	Babi la dure	Terre d'opportunité	None
SES score	0.008	0.015	-0.023*	-0.018	0.009	0.009
	(0.014)	(0.013)	(0.013)	(0.025)	(0.022)	(0.023)
Beauty score	-0.016	-0.011	0.027**	0.002	-0.019	0.017
	(0.015)	(0.013)	(0.013)	(0.027)	(0.024)	(0.025)
Corpulence score	0.016	-0.007	-0.009	0.001	0.015	-0.016
	(0.016)	(0.014)	(0.014)	(0.029)	(0.026)	(0.027)
Female	-0.005	-0.070*	0.074*	-0.004	-0.098	0.103
	(0.045)	(0.040)	(0.040)	(0.080)	(0.070)	(0.074)
Log(age)	-0.043	-0.038	0.081	0.028	-0.116	0.087
	(0.071)	(0.063)	(0.063)	(0.130)	(0.114)	(0.121)
Mean	0.46	0.26	0.28	0.43	0.26	0.31
Observations	552	552	552	179	179	179

Notes: Sample limited to subsample selected for the documentary choice experiment. The dependent variable in column 1 equals one if the respondent volunteered for "Babi la dure". The dependent variable in column 2 equals one if the respondent volunteered for "Terre d'opportunité". The dependent variable in column 3 equals one if the respondent did not volunteer for a documentary. "Private" is a dummy equal one if the respondent was randomized into making her documentary choice in private (directly on the tablet, without the enumerator seeing the choice). SES, beauty, and corpulence scores are average scores (between 0 and 1) given to photograph by a separate panel of judges drawn from the study population.

Table A.6: Consumption and socio-economic status

	(1)	(2)
	Dep Var: Chose charity contribution over basket of goods	
	Private Treatment	Full Sample
Middle wealth tercile (PCA index)	-0.018	0.027
	(0.032)	(0.019)
Top wealth tercile (PCA index)	0.047	0.063***
	(0.035)	(0.020)
Above secondary	0.114**	0.103***
	(0.057)	(0.033)
Above 50 years old	-0.048	-0.029
	(0.035)	(0.020)
Female	-0.072***	-0.074***
	(0.028)	(0.017)
Speaks french at home	0.032	0.019
	(0.034)	(0.020)
Ivorian	0.012	-0.015
	(0.045)	(0.027)
Mean	0.177	0.195
Sous-prefecture FE	Yes	Yes
Observations	752	2276

Notes: Regression of the variable "the respondent chose charity contribution over basket of goods" on socio-economic characteristics. The sample in column 1 is restricted to the respondents in the private treatment.

Table A.7: Raffle results: Correlation between experimental and real-life behavior

	(1)	(2)
	Dep. Var: Chose charity contribution over basket of goods	
	Gave transfers	Didn't give transfers
Public (Enumerator)	0.09**	-0.00
	(0.04)	(0.02)
Public (Neighbors)	0.05	0.01
	(0.04)	(0.02)
Mean private	0.19	0.17
Observations	768	1508

The table displays the coefficients of regressing "The respondent chose charity over basket of goods" on the public treatment arms for the subsample of respondents who report giving transfers to others (column 1) and the subsample who do not (column 2).

B Example vignettes' photographs

Figure B.1: Example woman

(a) Low SES   (b) Medium SES   (c) High SES

Figure B.2: Example man

(a) Low SES   (b) Medium SES   (c) High SES

C Adding a Social Tax on the Rich

C.1 Social taxation

The conceptual framework presented in Section 2 can easily be extended to allow social taxation of individuals earning income $y > R$ with $R > Z$. With this addition, the model is capable of predicting both signaling "up" by some, as we document in this paper, and signaling "down" by others (as found in Baland et al. (2011) and Baseler (2023) among others).

Let the social tax imposed on individual i be a fraction τ of their apparent income a_i that exceeds R:

$$T_i = \tau(a_i - R)$$

Let us further assume that individuals can devote resources $q(d)$ to dissimulate some of their earnings so as reduce their apparent income $a_i = y_i - d_i$, with $q'(d) > 0$, $q''(d) > 0$, and $q(0) = 0$. Rich individuals thus solve an optimization problem of the form:

$$max_{r \geq 0} U(c) \text{ subject to } c = y - q(d) - \tau(y - d - R) \tag{2}$$

Since, by construction, $U'(.) > 0$ always, the first order condition boils down to:

$$\tau = q'(d)$$

which determines the optimal level of dissimulation as $d^* = (q')^{-1}(\tau)$.

In this simple model, optimal dissimulation is an increasing function of the social taxation rate τ but otherwise does not depend on income. However, because social taxation is 0 if $a = R$, there exist an interval of income values such that the individual devotes just enough resources to dissimulation so as to keep the apparent income equal to R.

The solution for rich individuals can thus be characterized as follows:

- When $y \leq R$, then $d = 0$

- When $R < y < R + d^*$, then $d = y - R$

- When $y > R + d^*$, then $d = d^* = (q')^{-1}(\tau)$

C.2 Overlap

If $R < Z$, the two models overlap in the sense that individuals have to contribute to the social tax to avoid being victimized for being too poor. In this case, it is probably best to think of contributions to the social tax as part of the signal s.

In the extreme case where $R = 0$ and $s_i = T_i$, individuals signal their income by offering a contribution T_i. In this case, keeping up appearances works entirely through social contributions.

C.3 Summary

The predictions of this extended conceptual framework can be summarized as follows:

- The poor have an incentive to invest in costly signals (e.g., conspicuous consumption, charitable contributions, 'proper' behavior) in order to achieve a minimum level of SES image to avoid being victimized – i.e., they want to appear 'average', 'like everybody', because this makes them 'respectable'.

- The ultra-poor cannot afford the cost of the signal and are victimized/despized/not-respected/ostracized. In a dynamic sense, there is a cliff edge to poverty, creating a poverty trap sometimes called destitution (Dasgupta, 1995). This is particularly true if victimization is driven by a negative moral judgement (Walker and Bantebya-Kyomuhendo, 2014).

- The rich have an incentive to hide some of their consumption so as to partly hide their high SES status and reduce or avoid a 'social tax'.

These predictions do not require that subjects be unaware that others are manipulating their perceived rank. This is because signals are partially informative: in equilibrium, the ultra-poor are identified and victimized; and the very rich are identified and taxed. But there exist intervals of true income values over which individuals are observationally equivalent. Rich individuals with $R < y < R + d^*$ all have the same $a = R$. Consequently, others cannot rank them relative to each other, even though they know appearances a are manipulated. But they can rank those whose income y exceeds $R + d^*$. Similarly, poor individuals for whom $y^* < y < Z$ all have the same $a = Z$ and thus their relative rank is not observable. But ultra-poor individuals with $y < y^*$ can still be identified.

D   Full Description of Experimental Design

We present here, in chronological order, the various activities conducted with respondents during the wave 2 of the AUDRI survey, which forms the core of the data in this paper. Table D.1, just like Table 1, organizes the activities by research hypothesis and the order in which the results are presented, but shows sampled size instead of outcomes.

Table D.1: Components of the study, by hypothesis

	Hypothesis	Experiment	Sample
H1	1.1: People who look poor have fewer opportunities.	Vignette	Full sample: 2276 respondents, 3 vignettes
		Photo	102 raters, 1230 photos
	1.2: People who make "poor" consumption choices have fewer opportunities.	Vignette	Full sample: 2276 respondents
H2	2.1: People distort their appearance to appear less poor.	Photo	Subsample D1: 758 participants
	2.2: People are more likely to distort their consumption as observability increases.	Raffle	Full sample: 2276 participants
		Mini-job application	1343 participants
H$_{aux}$	People refrain from status-inconsistent behavior for fear of rejection.	Documentary Choice	Subsample D2: 758 participants

D.1   Raffle

In the 2022-23 survey, the questionnaire starts by informing respondents that, as compensation for their participation to the survey, their name will be entered in a raffle that will take place at the end of the survey. Each respondent is then asked to choose one of three possible prizes for them to receive in case they are one of the raffle winners. Each prize has the same value of 50,000CFA—around 85 USD, a non-negligible amount for our study population which, pre-Covid at baseline, had an average monthly income of 106 USD (median 54 USD). The three prizes were: (1) a basket of basic necessities; (2) a basket of non-necessities; and (3) contributions to one of three local NGOs.[18]

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[18]See Appendix Figure A1 and A2 for photographs of the two consumption baskets. The three chosen NGOs were: an orphan and single mother care center in Abobo; a similar center in Yopougon; and the Drogba Foundation, a philanthropic organization founded by a local sport celebrity. A large fraction of our study sample resides in the low income neighborhoods of Abobo and Yopougon.

Respondents were randomized into three observability treatments. In the first treatment, respondents are asked to enter their prize choice privately onto the enumerator's PDA; in the second treatment, they report their choice of prize to the enumerator, who records it; and in the third treatment, they record their choice by ticking one of three boxes in a sheet of paper containing their name and those of other households in their neighborhood. The purpose of the third treatment is to induce respondents to believe that their choice will be observed by some of their neighbors.

D.2 Experimental Vignettes

Towards the end of the questionnaire, respondents are presented with three distinct series of vignettes and asked to report their beliefs about their possible outcomes. In the first series of vignettes (benchmark vignettes), respondents are presented with a hypothetical individual CV containing information about education level and occupation, and asked the likelihood that the person with this CV would be selected for a specific position. There are six possible CVs and four positions: receptionist at the Abidjan Sofitel (a luxury hotel); committee member of a charitable organization; video contributor to an Abidjan documentary on 'Voices of the poor'; and video contributor to an Abidjan documentary on 'Voices of the mighty'. The positions and CVs are randomized across respondents so as to net out individual effects. The purpose of this vignette is to ascertain which characteristics best predict suitability for different types of assignments. It is intended to be combined with experiment 7 (see below).

In the second series of vignettes, each respondent is presented with three photographs of a person and asked the likelihood that the person would be: suspected of burglary by the police; suspected of stealing by the community; invited to a social gathering; and evicted by their landlord. There are 18 photographs of 6 different volunteers, three men and three women. Each volunteer is pictured in an outfit and environment suggesting either a low, medium, or high socio-economic status (SES).[19] A respondent is never shown the same person in different outfits. Pictures are randomized so as to be able to net out fixed effects for each of the six volunteers. The purpose of this vignette is to test whether responses vary with the appearance (and gender) of the volunteer in the picture(H1).

In the third series of vignettes, each respondent is presented with three hypothetical recruitment situation: a supervisor job in a supermarket chain; a paid job in a local NGO; and a position on the committee of a charitable organization. Respondents are provided with short CVs documenting the applicant's education level (which is kept constant across vignettes), current occupation, and father's occupation. In addition, respondents are told that, at the end of the interview, the applicant will receive either a free meal or a key-chain and tote bag, depending on the applicant's stated preference at the time of applying. The respondent is asked how likely the applicant is to be selected for an interview, based on their CV and choice. In half of the cases, the hypothetical applicant has selected the meal; in the other half, the key-chain and totebag have been selected. This is randomized across respondents. The purpose of this vignette is to test whether respondents perceive consumption choices as signals of suitability for different jobs (H1)—the idea being that a low SES applicant is more likely to select the meal instead of the more luxurious but less immediately useful key-chain and tote bag.

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[19]See Appendix B for examples of photographs.

D.3 Documentaries and Photos

At the end of the questionnaire, respondents were randomly divided into three equal size groups: a "photo choice experiment" group, a "documentary choice" group, and third group for which the survey ended.

D.3.1 Photo experiment

In this group, an experiment examines respondents' willingness to exert effort (i.e., spend time) to enhance their physical appearance. Respondents are first asked whether they allow the enumerator to take a photograph of the respondent on his/her phone. A randomly selected half of these respondents is asked whether they consent to their picture being shown to a local panel that will select volunteers for a documentary about how people of Abidjan have fun ('Babi la joie'). Both sets of participants are then offered the possibility to have the enumerator come back later to taken another picture of them, i.e., after they have had time to prepare themselves. Alternatively, they could take a photo themselves and send it digitally to the research team digitally. The purpose of this treatment is to test whether participants are more likely to take the offer of a second, probably better photograph to improve their chance of being selected for the documentary. It is a test of hypothesis H2.

D.3.2 Documentary choice

In this group, respondents are first asked whether they allow the enumerator to take their picture. They are then offered the opportunity to volunteer for one of two documentaries: one on the difficulties of life in Abidjan; and another on the economic opportunities that Abidjan offers. They have to choose one—they cannot volunteer for both. We randomize whether their choice is made in private (directly on a tablet, without the enumerator observing) or whether it is observed by the enumerator. The purpose of this module is to test whether respondents choose the documentary that best reflects the SES status revealed by the photograph that the enumerator just took, i.e., whether they apply for the 'job' for which they are most likely to be selected. A small subset of respondents in the *documentary choice* experiment were given the option to schedule a second photo-shoot or submit a new photo digitally.

D.3.3 Documentary suitability scores

To select respondents for the documentaries, we asked ten enumerators to each view a random subset of 120 to 180 photographs of volunteers, and to score each of them on their suitability for the documentary they said being willing to be considered for (documentary choice group) or they had been assigned to (photo choice group). For those volunteers who provided two photographs, enumerators were randomized into receiving one or the other. The purpose of this experiment is to test whether the second photos are rated higher than the first photos, which is a way of testing whether respondents were able to alter their appearance to improve their chances of being selected (H2).

Based on these scores, a subset of volunteers were invited to submit a one-minute video recorded on a phone. These short videos were subsequently combined to produce three short videos entitled 'Babe la joie' (about the joy of living in Abidjan), 'Babi la dure' (about the difficulties of life in Abidjan), and 'Abidjan terre d'opportunité' (about the opportunities offered

by Abidjan). As promised to respondents, these short documentaries have since been made public on the Stanford AUDRI website and on the IHfRA website.

D.4 Mini-job application

All respondents who had not won the raffle and had not been included in pilot activities (N=2,068) were eligible for the mini-job experiment. For this, we invited eligible respondents we could reach by phone (95%) in March 2023 to a 'mini-job' consisting in ranking photographs of other respondents (we refer to these hirees as "judge" in the paper). This 3-4 hour job was well paid (14.000 FCFA, which is equivalent to USD 23) and thus attractive to our study population. Among respondents who expressed interest in applying (68.5%), we explained that, in addition to the wage payment, those selected for the job can choose either a complimentary meal or a pen of equal monetary value.

Respondents were randomly assigned to one of two treatments. In the first treatment, respondents were told that, since the job was likely to be oversubscribed (only 102 (7.5%) of those who expressed interest ended up being hired), selection for the mini-job would be random. In the second treatment, applicants were told that the selection of workers would be based on answers to a few questions on their suitability for the job.[20] These questions were then immediately followed by the choice between the meal or the pen.

The purpose of this experiment is to test whether respondents do distort their choice when this choice is potentially observable by a selection committee (H2). It is designed to be compared to respondents' earlier answers to the third series of vignettes. We hypothesize that in the first treatment, when respondents know for sure that their meal-or-pen choice will not influence their probability of being selected, they will opt for what genuinely appeals to them. In contrast, we anticipate that more individuals will choose the status good (pen) in the second treatment if they internalize the fact that choosing a free meal is considered as a negative signal.[21]

D.5 Mini-job tasks

Respondents invited to the mini-job were randomly divided into four equal-size groups. Three of these groups are tasked with ranking a random subset of 100 photographs according to one of three criteria: socio-economic status (T1); beauty (T2); and corpulence (T3). For respondents who provided two photographs, mini-jobbers are randomized into receiving one or the other—never both. The purpose of this experiment is to test whether the second photos are rated higher than the first photos, which is a way of testing whether respondents were able to alter their appearance to improve their chances of being selected (H2). This experiment also allows us to examine whether the ranks given by mini-jobbers correlate with SES data collected in the survey (which tests whether appearance predicts SES status), and whether these ranks predict the scores given by enumerators (which tests whether perceived SES status predicts the perceived suitability of volunteers for the documentary job). Ranks on beauty and corpulence serve as control variables.

Each mini-jobber was asked to rank a randomly assigned set of 100 photos on *one* of the three criteria only, to avoid inducing mechanical correlation between rankings across criteria.

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[20]The questions were: (a) Do you have any experience with tablets/ computers or smartphones; and (b) What is your availability for the job? These questions were only asked to applicants assigned to this treatment.

[21]The scripts used for this experiment used are given in Appendix E.

All pictures were scored and ranked by at least two mini-jobbers on each criteria.[22] Each mini-jobber ranked the 100 photos in two steps as follows. First they give each of their 100 photos a score from 1 to 10. On that basis, photos are divided into 10 batches, one for each score. In the second step, photos assigned the same score (i.e., in the same batch) are ranked within the batch by each mini-jobber. A combined score is then constructed by combining the two.[23] This procedure is used to improve the quality of the ranking by reducing the cognitive burden that having to rank 100 photographs in one go would impose. For each criteria, the variable used in most of the analysis is the average of the combined score of the two (or three) mini-jobbers.

The last fourth of the mini-jobbers were tasked (T4) with answering eight questions identical to the first and second series of vignettes, namely, questions on the likelihood that the person in the photograph would be selected for a job: receptionist at the Abidjan Sofitel (a luxury hotel); committee member of a charitable organization; video contributor to an Abidjan documentary on 'Voices of the poor'; and video contributor to an Abidjan documentary on 'Voices of the mighty'; and the probability that the person would be: suspected of burglary by the police; suspected of stealing by the community; invited to a social gathering; and evicted by their landlord. The purpose of this experiment is to link answers given to the two set of vignettes to actual people in our sample (H1). It also allows us to compare the magnitude of the effect of education and occupation—two relevant determinants of productivity—to that of SES status, beauty, and corpulence.

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[22]There were around 25 mini-jobber per dimension and a little less than 1250 photos, which means there are always at least two judges per photo, and sometimes three.

[23]Formally, let s_{ik} be the score from 1 to 10 assigned to photograph k by mini-jobber k, and let r_{iks} be the rank of that photograph in the batch of pictures assigned the same score s_{ik} by mini-jobber k. Since the number of photographs receiving score s_{ik} varies across scores and mini-jobbers, let m_{ks} be the number of pictures given score s by mini-jobber k. Let $r_{iks}/(m_{ks}+1)$ be the normalized within-score rank of picture i within the score batch s; it is a number between 0 and 1, adjusted for the size of the batch. The combined score c_{ik} is calculated as $c_{ik} = s_{ik} + r_{iks}/(m_{ks}+1) - 1$. It is a decimal number between 0 and 10.

E Scripts

E.1 Job Interview Experiment

Control group

We would like to ask if you are interested in coming to our office the week of March 20 to work 2-3 hours for us. The work consists in rating pictures on a tablet or computer. You would receive 14,000 FCFA, as a compensation for your work and to cover your transport costs, and either a prepared meal (to take away) or a pen of the same value. If you are interested in the job, your name will be entered into a raffle to determine who is invited to come. The reason for using a raffle is that we only need a certain number of workers to complete the task. We will randomly choose workers from those who are willing to participate. Are you interested?

[If the respondent is available/interested:] For planning purposes, please tell me now if you prefer to receive as an additional gift the prepared meal (to take away) or the pen. Your choice will not influence your probability of being chosen.

Screening group

We would like to ask if you are interested in coming to our office the week of March 20 to work 2-3 hours for us. The work consists in rating pictures on a tablet or computer. You would receive 14,000 FCFA, as a compensation for your work and to cover your transport costs. We only need a certain number of workers to complete the task. If you are interested in the job, we will ask a few questions that will help us select workers in case there are more interested people than necessary. Are you interested?

[If the respondent is available/interested:] Here are the screening questions: (a) do you have experience with computers/tablets or smartphones? (b) How sure are you of being available the week of March 20?
Finally, as an additional compensation for your time, would you prefer to receive a prepared meal (to take away) or a pen? Both have the same value.

E.2 Photo Experiment

Private condition

I would like to ask your consent to take a picture of you for our records. This is part of our survey quality control procedures – it will make it possible for my supervisor to verify that I truly interviewed you. The picture will be accessible to the research team.

Do you consent? [Take the picture if the respondent consents]

If you wish to have more time to prepare in order to have a more flattering photo, we can also come back to take a picture of you at a later time. Would you like us to come back today or tomorrow?

Screening

I would like to ask for your consent to take a picture of you for our records. This is part of our survey quality control procedures – it will make it possible for my supervisor to verify that I truly interviewed you. The digital picture will be accessible to the research team.

Do you consent? [Take the picture if the respondent consents]

We intend to produce a documentary called "Babi la joie" about the inhabitants of Abidjan when they have fun. A selection committee will evaluate the pictures to select the participants of the documentary . If you are selected, a member of our team will contact you and you will have the opportunity to express in a short video your personal experiences on the good life in Abidjan. Participation in this documentary is completely voluntary and you will have the opportunity to decline to participate if you wish. Do you give me your consent to show the picture that we just took to the selection committee?

If you wish to have more time to prepare in order to have a more flattering photo, we can also come back to take a picture of you at a later time. Would you like us to come back today or tomorrow?

Do you give me your consent to show this photo to the selection committee?