Keeping Up Appearances:
An Experimental Investigation of Socioeconomic Status Signaling to Avoid Discrimination*

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Abstract

We investigate the welfare cost of relative rank considerations, using a series of vignettes and lab-in-the-field experiments with over 2,000 individuals in Abidjan, Côte d'Ivoire. We show that: (1) people perceived to be of very low rank are considered more likely to be sidelined from beneficial opportunities in many aspects of life; and (2) in response, people invest in their appearance and distort consumption choices in order to appear of higher rank. These effects are economically significant. As predicted by a simple signaling model, the distortion concerns people with low (but not too low) socio-economic status and the SES range concerned varies across types of beneficial opportunities.

JEL codes: D12, D63, J71, O12

Keywords: conspicuous consumption, relative rank, poverty, stigma

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

1   Introduction

There is abundant anecdotal evidence that the *appearance* of economic poverty can attract scorn, ridicule, social exclusion, and even moral condemnation (e.g., Chase and Bantebya-Kyomuhendo, 2014). Sociologists refer to this phenomenon as 'poverty stigma' and scholars, using survey data, have documented discrimination tied to perceived poverty (Peterman, 2018), as well as the lack of respect the poor receive from others (e.g., Inglis et al., 2019, for the UK and Link et al., 2024, for the US).

This paper investigates two questions central to the welfare cost of poverty stigma: (1) is poverty stigma *expected*, i.e., are people perceived to be of relatively low socio-economic status (SES) considered more likely to be discriminated against across hiring, policing, and everyday social life; and (2) as a result, do people with relatively low SES *distort* their consumption 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 2,000 respondents recruited among the poorer segments of urban and peri-urban dwellers of Abidjan, the largest city in Côte d'Ivoire, West Africa. We also offer a simple signaling model showing that, in the presence of relative rank considerations, the distortion in consumption is larger for people with low, but not too low, SES—but the range varies across domains or type of interactions.

The idea that relative rank matters for how one is treated, and as a result influences economic behavior, is not new. Veblen and Howells (1899) observed that people often engage in conspicuous consumption to increase 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. Following the seminal paper of Bénabou and Tirole (2006), these ideas have recently regained some prominence. Contributions have been both theoretical (e.g., Ghiglino and Goyal, 2010; Bramoullé and Ghiglino, 2022; Langtry, 2023) and empirical (e.g., Bursztyn et al., 2018; DellaVigna et al., 2012; Charles et al., 2009).[1] In lower-income countries, these theories have been

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[1]Using observational data from 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

brought to the fore to account for lavish weddings (Rao, 2001; Bloch et al., 2004) and funerals (Jindra and Noret, 2011; Case et al., 2013). By themselves, however, the correlation patterns documented in this literature do not demonstrate social signaling. For this to be the case, the evidence would have to show not only that people distort their choices or consumption, but also that these distortions are undertaken with the purpose of social signaling, and that they have social or economic benefits. This paper provides evidence of all three in a single social context.

The literature so far has been primarily concerned with the middle or upper class. Our study focuses instead on the idea that for people close to the bottom of the socio-economic ladder, signaling that they are not *at the very bottom* can have important returns.

We first present a signaling model where "observers" set a threshold level of observable status (Z) below which individuals face discrimination, and "targets" can invest in costly signals to raise their apparent status above this threshold. The model predicts a non-monotonic response: the very poorest individuals (below income y*) cannot afford to signal and accept discrimination, while those with intermediate incomes (between y* and Z) invest in appearance to reach the minimum threshold Z, and the wealthy (above Z) need not signal at all. The threshold Z is endogenous: observers set it at the level where only individuals above some income threshold find it worthwhile to signal, making the signal informative despite manipulation. Different contexts (e.g., job interviews vs. social gatherings) will have different thresholds. The model further predicts that signaling expenditure as a share of income decreases with income—making status goods behave like necessities for the poor.

A key premise of the model is that there is discrimination against the poor, and people expect it. We employ a randomized vignette approach to identify people's expectations about discrimination toward low-SES individuals. Respondents view photographs of a person dressed in high, medium, or low-SES attire. We find that the same person photographed in high-SES clothing is judged as less likely to be suspected of committing theft or burglary, and more likely to be invited to social gatherings, compared to when wearing medium or low-SES attire.[2] 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

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that this explains most of the racial gap in consumption patterns.

[2]We did not ask respondents to assess competence, but clothing has been shown to influence perceived competence in the United States context (Oh et al., 2020).

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 confirms that potential signals of economic rank matter for important social and economic opportunities, and demonstrates that respondents are aware of this.

Our key contribution is a suite of incentivized choice experiments designed to test the hypothesis that, given the discrimination risk, people invest in their appearance and, more generally, use their consumption choices to differentiate themselves from those at the very bottom. 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, as found among *favela* dwellers in Brazil by Angeli et al. (2024). We find some evidence of such self-censoring in the aggregate, but not within our (relatively poor) sample. We then investigate whether respondents modify their appearance to improve their perceived SES rank when they know they are being evaluated for an opportunity. This is achieved by taking a photograph of willing participants and then offering them the opportunity to have a photo "retake" 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 retake when the photograph matters for selection, but as predicted by the model, this effect is non-monotonic in SES: we observe the effect only for those in the middle tercile of the SES distribution. 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 peers as less likely to be discriminated when the panel is shown the prepared photograph, compared to when they are shown the unprepared photograph of that same individual. We also confirm that prepared photographs are rated differently by survey firm staff selecting participants for documentaries, confirming that changing one's appearance is consequential. Together, these results suggest that people who are neither too low nor too high SHS, deliberately seek to signal a higher SES to increase chances of a favorable outcome.

Next, we 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

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be transmitted to a selection committee. Applicants were given the choice between a free meal or a pen, as a perk they would receive (in addition to the wage) if hired for the job. 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 pre-order 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 much less likely to pre-order the free meal than those in the random group. Here again, the effect is non-monotonic in income: the very poorest cannot afford to signal, and the relatively wealthier individuals do not need to.

Last, we conduct a field experiment to test whether individuals distort their consumption towards a status item when their consumption choice is publicly observable. We offer respondents a choice between (a) receiving a basket of household goods and (b) making a contribution to a charity. These gifts are presented as a reward for participating in 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, in addition to the enumerator. 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, when their choice is observable, respondents above a certain SES threshold are more likely to select the charitable contribution . We then show that, as predicted by the model, the share of one's total budget spent on the charitable contribution decreases as the total budget increases.

Our findings contribute to various literatures. First, we contribute to the experimental literature on social stigma and social signaling. In his seminal contribution, Moffitt (1983) introduced 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. In Uganda, Macchi and Stalder (2025) experimentally document how individuals have a high willingness to pay for redistribution via work rather than cash handouts. This is consistent with a model where accepting free money signals extreme poverty. Social signaling has also been discussed in other contexts. Bursztyn and Jensen (2015) and Bursztyn et al. (2019) show that social image concerns influ-

ence whether high-school students in Los Angeles sign up for SAT prep course. Karing (2024) shows that embedding a social signal in a vaccination intervention in Sierra Leone raises take-up. Oh (2023) documents workers reluctance to accept better paying jobs occupied by lower castes in India. And Macchi (2023) shows that obesity is a reliable signal of wealth that loan officers use to make high-stakes lending decisions in Uganda, and documents that people are well aware (in fact, they overestimate) the wealth-signaling value of obesity.

While some of these studies provide experimental evidence that individuals undertake costly actions, it does not demonstrate that these actions are taken to avoid discrimination: people may undertake these actions simply because of self-image concerns, or for other reasons such as conformism, life-style choices, or erroneous beliefs. For these actions to constitute strategic social signaling, they must have real consequences, such as reducing social or economic discrimination and other forms of social exclusion, and those who undertake these actions must be aware of these benefits. Our paper is the first to overcome this shortcoming by combining evidence that individuals undertake costly actions to modify how they are perceived by others *and* that this social signaling has real economic and social benefits, such as increasing the likelihood of being called for a job interview. We also show that the distortions induced by social signaling are large, especially relative to people's income. To the best of our knowledge, ours is the first study to document these distortions with real-stakes choices among a very poor population, and the economic benefits that they produce.[3]

Second, our paper contributes to the literature on discrimination. We show that respondents expect others to be treated badly if their appearance signals low SES and we provide evidence of actual discrimination based on signals of low SES. This evidence is reminiscent of taste-based labor market discrimination documented in various settings (e.g., Bertrand and Mullainathan, 2004; Charles and Guryan, 2008) as well as discrimination due to biased beliefs (Bohren et al., 2019). Discrimination based on SES appearance can contribute to what Bohren et al. (2025) call "systemtic discrimination". We also show that, when given the opportunity, people with low SES modify their appearance and consumption pattern to receive better treatment. These

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[3]Our results are consistent with lab experiments conducted on richer populations. In a lab experiment with students at a US college, Clingingsmith and Sheremeta (2018) show that making consumption of gourmet chocolate truffle visible leads to a large increase in demand when income brings status, but not otherwise. In an experiment on charitable behavior Butera et al. (2022) show that public recognition motivates giving but also creates unequal image payoffs, with significant utility losses for those who do not give. Relative rank and social image concerns have also been shown to affect voting (Gerber et al., 2016; DellaVigna et al., 2016).

findings resonate with papers showing status-seeking behavior motivated by the desire to gain social recognition (e.g., Charles et al., 2009; Bursztyn et al., 2018).

Third, 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 low the earnings of low paid workers are. Using Danish data, Hvidberg et al. (2023) document how those with higher incomes tend to overestimate the incomes of others. 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. Together with a growing literature documenting incentives for individuals *at the top* to hide their earnings to avoid redistributive pressure (see e.g., Baland et al. (2011) and Baseler (2023) among others), the process investigated here with individuals *at the bottom* offers a possible explanation for these findings, namely, that efforts by individuals to manipulate their perceived social status compresses the distribution of perceived income relative to the truth.

Fourth, both our conceptual and empirical analyses suggest that surveying individuals about their assets, income level or other proxies of social status may be affected by relative rank considerations. While the risk of strategic *under-reporting* in the context of government surveys used to target social programs has been previously discussed (e.g., Camacho and Conover, 2011), the pattern in our raffle experiment is the opposite: respondents sought to signal a higher socio-economic status to the enumerator by choosing the charitable contribution over the basket of goods. This finding is consistent with Martinelli and Parker (2009), which found evidence that individuals over-report assets owned by the majority, a phenomenon they explain through an 'embarrassment' effect.

Finally, our findings have implications for 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 being 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). But 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

Building on the work of Bénabou and Tirole (2006) and Clingingsmith and Sheremeta (2018), 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, 2014). Whatever the reason, looking poor may lead to loss of respect, 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 form the basis of our experimental design and they can be formalized in a model in which individuals who appear poor are discriminated, and the fear of being

discriminated induces poor people to engage in behaviors that reduce their immediate material welfare but protects them from discrimination.[4]

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 are highly mobile, 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.[5]

Provided that 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., 2025). 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).

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[4]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.

[5]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 out by design in our experiment and is therefore ignored here.

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. As we examine different types of selection decisions in our experimental design, we do not expect the threshold to be the same across experiments. What's more, since our sampling strategy likely excluded the most destitute, we may not always observe in our data the segment of the population that is below the threshold.

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 discrimination, i.e., of being offered a less favorable treatment and fewer opportunities. Let Z be the threshold of observable income below which a target is discriminated. 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 discrimination by observers.

The target solves an optimization problem of the form:

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

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 discrimination. Since there is no benefit from exceeding the minimum appearance Z that avoids discrimination, 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, illustrated in Figure 1:

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

Figure 1: Model illustration: Optimal signal across income levels
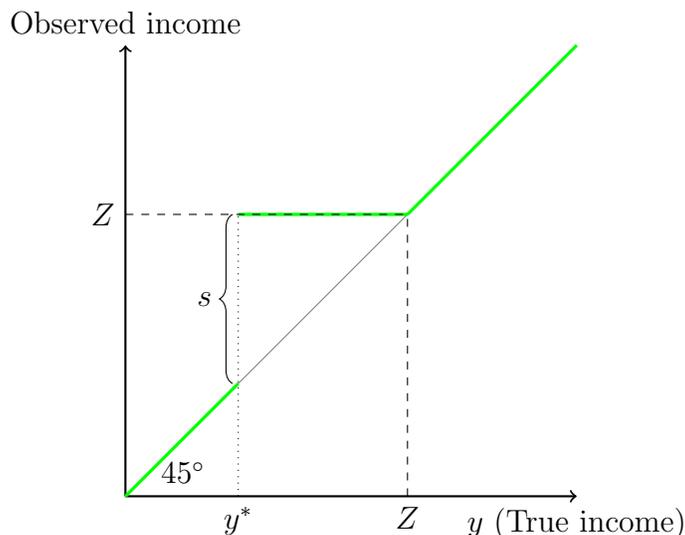
- If $y < y^*$, then $s = 0$. For these targets, the utility cost of appearances is higher than the expected cost of discrimination.

- 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 discriminated. This implies that, for these individuals, the share ps/y of their consumption budget spent on the signal s falls with income y: the signal has an income elasticity less than 1 and thus behaves like a necessity.[6] It also follows from this model that the fraction of discriminated targets in the population increases with Z, the cutoff in appearances, and p, the cost of reaching that threshold.

2.3 Endogenizing the signal level

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

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[6]Similar qualitative predictions are obtained if the model is generalized by making the likelihood (or severity) of discrimination 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 discrimination is, the fact that consumption is needed for survival makes very poor individuals choose to incur the cost of discrimination 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 discriminated, with an intensity of discrimination V that may increase by how much their income falls below y^*.

income $y_i < \hat{y}$, and $\pi^+ > 0$ otherwise. This could be either because the principal correctly anticipates fewer benefits from interacting with someone whose income falls below $\hat{y}$ (e.g., because it signals lower productivity or fewer useful social contacts), or because the principal receives negative utility from interacting with such person (e.g., taste for discrimination). To avoid such interaction, the principal sets a threshold signal s such that only those with $y_i \geq \hat{y}$ invest in the costly signal. This ensures that the signal s is informative of i's true income.

Formally, 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:

$$Z^* = \frac{V}{p} + \hat{y} \tag{2}$$

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 discrimination 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 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 example, a formal suit is typically expected from a job candidate. Higher dress codes exist as well. Exclusive black tie events, for

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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). 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.

These examples also serve to illustrate the fact that *knowing* that someone is of high SES does not, in general, dispense this person from following the dress code (or other signal). Since the code is essential to keeping at bay undesirable members of society, violations of the dress code by high SES individuals weaken the signal and threaten the exclusion equilibrium. For this reason, the dress code becomes self-enforcing: to belong to the group, individuals must show their support for the exclusionary code by abiding by it. Hence violating the code itself becomes a reason for exclusion. In such cases, providing financial information as evidence of socioeconomic status does not eliminate the need to follow the code.

2.4   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 $x > 0$.

Let us now assume that there is a cutoff level of luxury consumption Z below which the target is discriminated. 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 discrimination. In this modified model, it is excess

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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 discrimination 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 discrimination. 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.

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 discriminated against; 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 discriminated targets increases with Z and p, implying variation in discrimination 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.

2.5 Heterogeneity

We have noted that the targets' decision to spend resources on signaling depends on the discrimination 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 discrimination 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.[7]

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-19 (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. The data was collected with the assistance of Innovative Hub for Research in Africa (IHfRA).

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 2,276 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 the average household size is quite large, at around 6. Education levels reveal that one third of the sample lack formal 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 around 104,000 FCFA (187 USD) with a median of 60,000 FCFA (98 USD). In terms of ownership of assets, Table A.1

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

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;[8] 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 1,230 photos from 1,115 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 many dimensions, and by at least two judges on any dimension; but a given judge was asked to rate photos on only one dimension.[9] The dimensions rated were: (i) socio-economic status, (ii) beauty, (iii) corpulence, (iv) likelihood of facing discrimination. 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,

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[8]We produced three documentary films that were, at the time, posted on the Stanford and the IHfRA websites.

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

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 discriminated—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 632 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.

In summer 2024, we recruited enumerators to additionally rate the photos for skin-tone. Each photo was rated by 6 different raters, using 2 scales. We average the ratings to create a "skin-lightness" score. The fact that lighter skin tone increases perceived status has been well documented in many contexts (Dixon and Telles, 2017).

Ultimately, our photo-level dataset has 13 variables (each showing averages across all judges who rated that variable for that question): SES, Beauty, Corpulence, Skin tone, 8 discrimination questions, and documentary-score by enumerator.

3.3 Experimental Design

Table 1 presents the key elements of the study, organized by hypotheses. The hypotheses, and related heterogeneity tests, were all pre-specified in the AEA RCT registry entry (AEARCTR-0010773). 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 and the timeline, we refer the reader to Appendix D.

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

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 discriminated or socially excluded based on appearance
		Photo	2. Selection (by enumerators) of real applicants for documentary based on photos
	1.2: People with "poor" consumption patterns have fewer opportunities.	Vignettes	3. Selection of hypothetical job candidates based on candidate choice between free meal and gift
		Mini-job	4. Selection (by enumerators) of real job candidates based on candidate choice between free meal and gift
H2	2.1: People distort their appearance to appear less poor.	Photo	5. Choice to participate in photo retake to enhance physical appearance
			6. Probability of second photograph being perceived as less poor
	2.2: People distort their consumption to appear less poor.	Raffle	7. Choice between contributing to charity or receiving baskets of goods under different observability conditions
		Mini-job application	8. 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	9. Match between chosen documentary type and actual socioeconomic status

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).[10] 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)	(2)	(3)	(4)
	Evicted by landlord	Suspected by the police of burglarizing	Suspected by the community of stealing	Invited to social gathering
High SES picture	-0.721***	-0.303***	-0.723***	1.447***
	(0.083)	(0.078)	(0.077)	(0.080)
Low SES picture	0.167**	0.026	0.074	-0.556***
	(0.079)	(0.079)	(0.076)	(0.080)
Mean Medium SES	4.91	4.11	4.11	6.03
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. Clustered standard errors at the respondent level.

Table 2 shows how answers to these vignettes depend on which SES picture the respondent was shown (high, medium, or low). 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 evicted by a landlord or suspected of a crime 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

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[10]See Appendix B for examples of photographs.

coefficient, indicating that respondents believe the person more likely to be invited, while the 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.[11] Our focus is on whether choosing a free meal lowers the predicted likelihood of getting a job interview. This choice suits well our setting because it is customary to offer a meal to someone invited to a meeting to compensate them for the loss of earnings and the financial cost of traveling to the meeting place. Offering a meal is therefore considered normal in an environment where many people live hand-to-mouth. Given this, poor people living nearby may apply to the job just to get the free meal. This grants a strong signaling power to not taking the meal.

We indeed find strong evidence that, for the three jobs considered, choosing the free meal is regarded as a large negative signal: it decreases the perceived likelihood of being selected by between 1 point on a scale of 1 to 10 for the NGO interview, and by 1.2 points for the other two

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[11]Education does not appear as regressor since all vignettes reported the same level of schooling (secondary school diploma).

interviews.[12] 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	-1.213***	-1.189***	-0.986***
	(0.076)	(0.078)	(0.076)
Occupation: Unemployed	0.114	0.851***	0.050
	(0.081)	(0.088)	(0.081)
Occupation: Casual Worker	0.098	0.553***	-0.089
	(0.098)	(0.101)	(0.099)
Father is civil servant	0.110	-0.027	0.072
	(0.073)	(0.075)	(0.072)
Mean no free meal	6.81	6.49	6.64
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. 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

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[12]These effects correspond to a 0.35 standard deviations fall for the NGO interview and 0.42 standard deviation for the other two.

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 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).[13] As promised to respondents, these short documentaries were 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 enumerator scored up to 180 randomly selected photographs, with a given photo scored by multiple enumerators. Because these were photographs from the AUDRI sample, we have information on the age of the person on the picture. More importantly, we separately and independently hired people from the community (we call them "judges") to rank all photographs for SES, beauty, corpulence, and discrimination risk. We also hired people from a different geographic area to rate the skin tone of the pictures.

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	0.118***	0.183***	-0.114***
	(0.040)	(0.041)	(0.042)
Beauty score	0.119***	0.054	-0.071
	(0.046)	(0.046)	(0.047)
Lightness of skin	0.108***	0.053***	-0.062***
	(0.019)	(0.019)	(0.020)
Corpulence score	0.077*	0.120***	-0.037
	(0.043)	(0.044)	(0.045)
Female	-0.702***	-0.639***	0.110
	(0.140)	(0.140)	(0.145)
Log(age)	-1.803***	-1.016***	0.568***
	(0.206)	(0.206)	(0.214)
Mean score	5.62	5.77	6.43
Observations	632	632	632

Notes: An observation is a photo of a study participant. 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 10) given to photograph by a separate panel of judges drawn from the study population. Column 1 concerns ratings for the documentary "Babi la joie", about fun in Abidjan. Column 2 concerns ratings for the documentary "Terre d'Opportunité", about Abidjan as a land of opportunity. Column 3 concerns ratings for the documentary "Babi la dure", about hardship in Abidjan.

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

In Table 4, we regress the average enumerator score 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 perceived SES plays an important role for the opportunities that people may receive. 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 SES are significantly more likely to be scored highly by enumerators. The coefficient on the SES score is of the same magnitude as the coefficients on the beauty and lightness of skin tone scores. In the second column, the selection is for the documentary on Abidjan Land of Opportunity. Photographs ranking higher in terms of SES and corpulence (an indicator of wealth) are more likely to be selected for this documentary. We note that men and young individuals are also more likely to be selected for the two documentaries in columns 1 and 2—suggesting social stereotypes. The third column shows that a higher SES rating is associated with a lower suitability rating for a documentary on economic difficulties faced by Abidjan residents. Here, in contrast with the first two columns, being young and male reduce the suitability rating.

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 of their SES and appearance on their chances of success in selective environments. If respondents anticipate being rejected, then they may opt not to apply. This was documented in a recent study in Brazil, where job seekers from poor neighborhoods refrained from applying to jobs they feel discriminated for because of their address (Angeli et al., 2024). 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 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 not 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 and/or consumption to avoid being discriminated 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 what we call the *photo 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.[14] 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.

Table 5: Photo Experiment: Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Scheduled second photoshoot			Second photo was produced		
Screening	0.076**	0.066**	0.063*	0.090***	0.085**	0.077**
	(0.034)	(0.033)	(0.033)	(0.034)	(0.033)	(0.032)
Mean control	0.167	0.167	0.167	0.156	0.156	0.156
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 an indicator variable 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, and tercile in the wealth distribution. 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 5 examines whether respondents schedule a second photo shoot, and whether a second photo is produced.[15] We see that, when the photo will not be used for screening (control condition), only 16.7% of people plan to, and only 15.6% 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 figures increase by 6.3 and 7.7 percentage points, respectively. The share of people who invest in producing a second photo increases by almost 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 6 tests for heterogeneity in these results by socio-economic status. To proxy for SES, we use a principal component analysis to create a wealth index that combines information

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[14]The scripts used are shown in subsection E.2 of the online appendix.

[15]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.

about the respondents' self-reported income, household expenses per capita, assets, and dwelling characteristics.[16] We then use this index to define where the respondent falls in the distribution. We run the specification shown in column 6 of Table 5 separately by tercile of the wealth index.

Table 6: Photo experiment: Heterogeneity by SES status

|	(1)	(2)	(3)
	Dep. Var: Second photo was produced		
	Bottom wealth tercile	Middle wealth tercile	Top wealth tercile
Screening	0.012	0.128*	0.085
	(0.060)	(0.068)	(0.072)
Mean control arm	0.202	0.132	0.126
Observations	206	185	172

Notes: The table display the results of regressing the variable "a second photo was produced" on an indicator variable 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 and control for respondent gender and age.

We find that the results are stronger and 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 and those at the top. The magnitude of the effect for the middle group is quite large economically: Individuals told about the screening are 13 percentage points (+100%) more likely to have participated in the second photo shoot than those who were not. While this finding is, *prima facie*, consistent with our model, we note that the probability of having a second photo taken in the control condition is higher among those in the bottom tercile (20% vs. 13% in the middle tercile). It could be due to a lower time cost for this group. Alternatively, it could be due to the very signaling mechanism our model highlights: since respondents in the control condition are told that the photo is for future survey tracking purposes, some respondents in the bottom tercile may want to have a nicer photo stored by the survey team if they expect enumerators to treat them more respectfully in the next survey round.

Figure 2 uses the SES ratings of the photographs themselves (rather than the survey-based

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[16]Specifically, the variables including in the PCA are: own income, household expenditure in the past 7 days divided by the number of household members, whether the household owns their dwelling, whether they have electricity, running water inside the dwelling, and the following assets: a bicycle, a motorbike, a car, a ventilator, a fridge, a bed, a mattress, a television, a computer, and livestock.

Figure 2: Selection into Second Photoshoot, and Rating of Second Photos
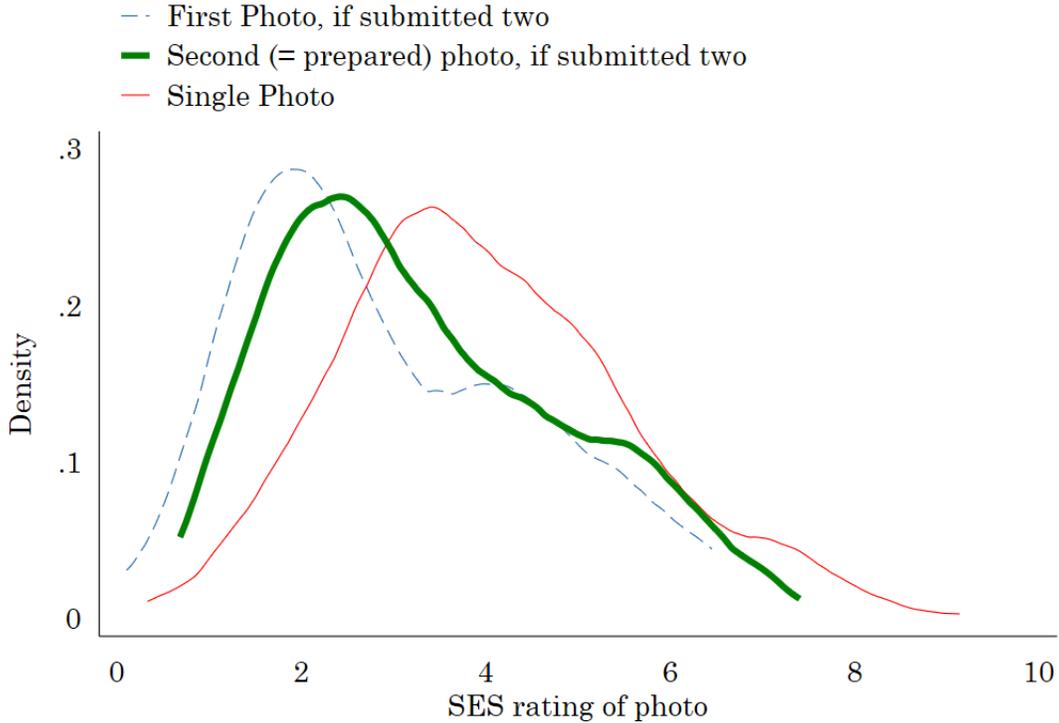
measured of SES) to study who selects into submitting a second photo. The figure shows that the SES-rating distribution of photos among those who elect to submit a second photo is clearly to the left (lower perceived SES) of the distribution of photos for those who do not submit a second photo. This confirms the results in Table 6.

We then verify that the second photos are indeed ranked higher. We do this in a number of ways. First, we check whether the second photographs are ranked higher SES by judges. This can be seen graphically in Figure 2. 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.

In Table 7, we regress the average SES score given to a photo on an indicator variable 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. Consistent with the existence of prejudice or discrimination against individuals with a darker skin tone, the prepared photos are rated as "lighter skin", suggesting that people invested in make-up and/or skin products in order to reduce their exposure to such colorism. 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) Skin lightness	(4) Corpulence rank
Prepared photo	0.397**	0.602***	1.261***	0.165
	(0.179)	(0.115)	(0.312)	(0.146)
Mean	3.660	5.102	8.067	4.463
Observations	230	230	230	230
Respondent FE	Yes	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 an indicator variable 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.

We then test whether the "prepared photos" are associated with better employment opportunities and social acceptability. For this, we ask our hired judges to answer the vignette questions we used in section 4.1 about the photos.

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 burglarizing	(6) Suspected by the community of stealing	(7) Evicted by landlord	(8) Invited to social gathering
Prepared Photo	0.45***	0.43***	0.23	0.57***	0.16	0.20	-0.03	1.17***
	(0.16)	(0.14)	(0.16)	(0.14)	(0.19)	(0.19)	(0.20)	(0.19)
Mean first picture	4.37	4.19	4.58	3.87	4.11	4.18	4.75	4.26
Pictured resp. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	460	460	460	460	460	460	460	460

An observation is a respondent-photograph-judge. The dependent variable in each column is the perceived likelihood of facing the event in the column title. The likelihood is measured on a scale from 1 to 10, with higher values associated to a higher likelihood. Photographed individual and judge fixed effects included.

Table 8 shows that, holding the photographed individual constant, prepared photographs predict a higher perceived 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—but no lower likelihood of being suspected of a crime or evicted.

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 facing discrimination 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.[17] These questions were then immediately followed by the choice between the meal or the pen.[18]

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

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[17]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.

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

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. The results are shown in Table 9.

Table 9: Distortion in Consumption Choices: Mini-job Experiment Results

	(1)	(2)	(3)	(4)
	Dep. variable: Respondent selected free meal			
Screening	-0.072***	-0.072***	-0.066**	-0.056**
	(0.027)	(0.027)	(0.027)	(0.026)
Mean random arm	0.570	0.570	0.570	0.570
Controls	No	Yes	Yes	Yes
Sous-prefecture FE	No	No	Yes	Yes
Enumerator FE	No	No	No	Yes
Observations	1341	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 an indicator variable 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.

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.

In Table 10, we examine whether signaling via this consumption choice varies with SES, using the same specification as in Table 6 but allowing the range of SES levels within which people signal to differ since the parameters of Equation 2 are different (the cost of the signal and the value of the beneficial state). For this application, we find that only individuals with wealth between the 10th and the 60th percentiles respond to the screening treatment by choosing the pen over the free meal. These findings fit our model predictions: the 10% poorest cannot afford to signal, but the near-poor 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 individuals in the top of the wealth distribution, who choose the pen with a much higher probability than the others (50% vs. 34% among the poorest) in the control condition.[19]

Table 10: Distortion in Consumption Choices: Heterogeneity

	(1)	(2)	(3)
	Dep. Var: Respondent selected free meal		
	Bottom 10% wealth	10 to 40 perc. wealth	Top 40% wealth
Screening	0.028	-0.095**	-0.050
	(0.097)	(0.038)	(0.042)
Mean random arm	0.661	0.610	0.496
Observations	115	681	547

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 an indicator variable equal to 1 if the respondent was assigned to the "Screening" condition for the sample indicated in each column. All specifications control for respondent gender and sous-prefecture fixed effects.

Finally, we check whether respondents are correct to *believe* (as evidenced in the vignettes experiment in Table 3) and to *act* on the belief (as evidenced in the mini-job experiment in Table 9) that selecting the free meal is interpreted as a undesirable trait by the "observer." For this, we asked enumerators to rate job applicants based on brief CVs. We put these brief CVs together for the respondents who (a) had applied for the mini-job and (b) were assigned to the screening treatment. These CVs included details on the applicant's gender, age, highest level of education, occupation at the time of the second wave survey, and familiarity with devices like laptops, tablets, or smartphones (as recorded in the screening questions). We also included the information about whether the applicant had chosen the free meal or the pen. In total, we compiled 283 unique profiles, which were each reviewed by 10 enumerators. Enumerators were asked to rate the profiles for suitability for the mini-job, and gave each CV a score from 1 to 10.

Results in Table 11 show that the average score received by a real-respondent profile is influenced by the choice of the free meal vs. the pen. In other words, their consumption choice is consequential. Of course, this choice is not exogenous, and we know from the earlier

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[19]The reader may wonder whether a test of our signaling model would have been to check whether job applicants in the near-poor category choose the free meal more often when they have a chance to disclose their financial situation (akin to Macchi (2023)'s experimental test of whether the obesity premium diminishes when additional financial information is made available to loan officers). The answer is no, because, as we discussed at the bottom of section 2.3, such information need not reduce the need for signaling.

analysis that it is correlated with covariates (poor respondents are more likely to select the free meal), but we can control for *all* the dimensions that were observable to the enumerators when rating (all the information on the CV). Doing this, in column 2, reduces the coefficient on "selected the free meal" somewhat, but the coefficient remains large (i.e., same order of magnitude as completing primary school) and statistically significant. This cannot be due to omitted variable bias since we can control for all the variables available to the scorers. Instead, it reflects discrimination (statistical or taste-based) based on markers of poverty. This shows that respondents' second order beliefs are grounded in reality, even if the magnitude of the effect is somewhat smaller than that predicted by respondents in Table 3, a finding that resonates with that of Angeli et al. (2024).

Table 11: Consumption choice and enumerator screening

	(1)	(2)
	Dep. Var: Score	
Selected free meal	-0.74***	-0.31***
	(0.22)	(0.06)
Female		-0.12**
		(0.06)
Education: Primary		0.45***
		(0.08)
Education: Secondary		1.63***
		(0.08)
Education: Post-secondary		2.65***
		(0.10)
Occupation: Unemployed		-0.22**
		(0.09)
Experience with computer/tablet/smartphone		2.36***
		(0.07)
Mean (if free meal not selected)	5.66	5.66
Controls	No	Yes
Age group dummies	No	Yes
Observations	283	283

An observation corresponds to a respondent-based CV. We created short CVs based on real respondents characteristics to elicit their suitability for the mini-job. The table displays the coefficients of regressing the enumerator scores reflecting the applicant's suitability for the job on an indicator variable equal to one if the profile indicates that the applicant selected the free meal (rather than the non-necessity good). Scores range from 1 to 10, with 10 indicating the highest suitability for the job. The Education dummies indicate the highest level completed.

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 had the same value of 50,000 CFA—around 85 USD, a non-negligible amount for our study population with an average monthly income of 187 USD (median 98 USD). The three prizes were: (1) a basket of basic necessities; (2) a basket of non-necessities; and (3) contributions to three local NGOs.[20]

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 not only by the enumerator, but also 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). Prior research has documented how the visibility of charitable giving boosts contributions (Alpizar et al., 2008; Butera et al., 2022), but to the best of our knowledge we are the first to test whether this applies even to individuals living at or near the poverty line. Such individuals may not risk "shame" from not contributing (since it may be well understood charitable giving is a form of redistribution, so they should be on the receiving end). But, as per our model, they may instead want to *hide* that they do not have the means to contribute, in order to wield respect.[21]

Our experimental 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

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[20]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.

[21]Enumerators are more educated and higher SES than the respondents, so respondents may have wanted to "signal up" to them. Doing so in a way that is visible to the neighbors may backfire, however, if charitable contribution is considered a luxury good that neighbors/ community members may consider extravagant.

plan when their choice is observed by others. Hypothesis H2 predicts that, when observed, respondent either give more to charity—the ultimate signal since 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 indicator 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 (an indicator variable 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 an indicator variable 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 observed by the enumerator, or potentially observed by others in the neighborhood in addition to the enumerator.

The results, presented in Table 12, show that respondents are 3.5 percentage points more

Table 12: Raffle Experiment: Signaling through Charity Contributions

	(1)	(2)
	Dep. Var: Chose charity contribution over basket of goods	
Public (Enumerator)	0.037*	0.035*
	(0.020)	(0.020)
Public (Enumerator+Neighbors)	0.022	0.022
	(0.020)	(0.020)
Mean private	0.177	0.177
P-value: Public (pooled)	0.117	0.117
Controls	No	Yes
Sous-prefecture FE	Yes	Yes
Observations	2276	2276

Notes: 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 on a sign-up sheet (handed over to the enumerator afterwards) that contains the names of some of their neighbors. Controls include respondent's gender and tercile in the wealth distribution.

likely to choose a signal good when their choice is observed by the enumerator. This effect is large—20% of the control value—and statistically significant. A similar—but smaller and not statistically significant—coefficient is found when the choice may additionally be observed by neighbors.

Table 13: Signaling through Charity Contribution: Heterogeneity

	(1)	(2)	(3)
	Dep. Var: Chose charity contribution over basket of goods		
	Bottom 50% Wealth	51-79th perc. Wealth	Top 20% Wealth
Public (Enumerator)	0.009	0.068*	0.037
	(0.027)	(0.037)	(0.050)
Public (Enumerator + Neighbors)	0.005	0.054	0.014
	(0.026)	(0.037)	(0.050)
Mean private	0.163	0.202	0.266
P-value: Public (Pooled)	0.760	0.055	0.548
Observations	1138	683	455

Notes: see Table 12 notes. Regressions control for gender of the respondent.

Table 13 shows the heterogeneity results by SES. Given the high cost of signaling in this experiment, we find that the threshold SES level at which we see an effect of the public treat-

ments is higher than in the previous two experiments: only people above the median react. Those in the top 20% of wealth *within our sample*, most of which are still likely below the SES level of the enumerators given the sampling frame, have a positive but insignificant treatment effect.

### 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.[22]

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.

To investigate this, Figure 3 plots the share of the budget assigned to the charity across budget levels using a binscatter plot. We observe a clearly downward sloping relationship, in line with the model prediction.

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[22]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 3: Share of budget spent on charity
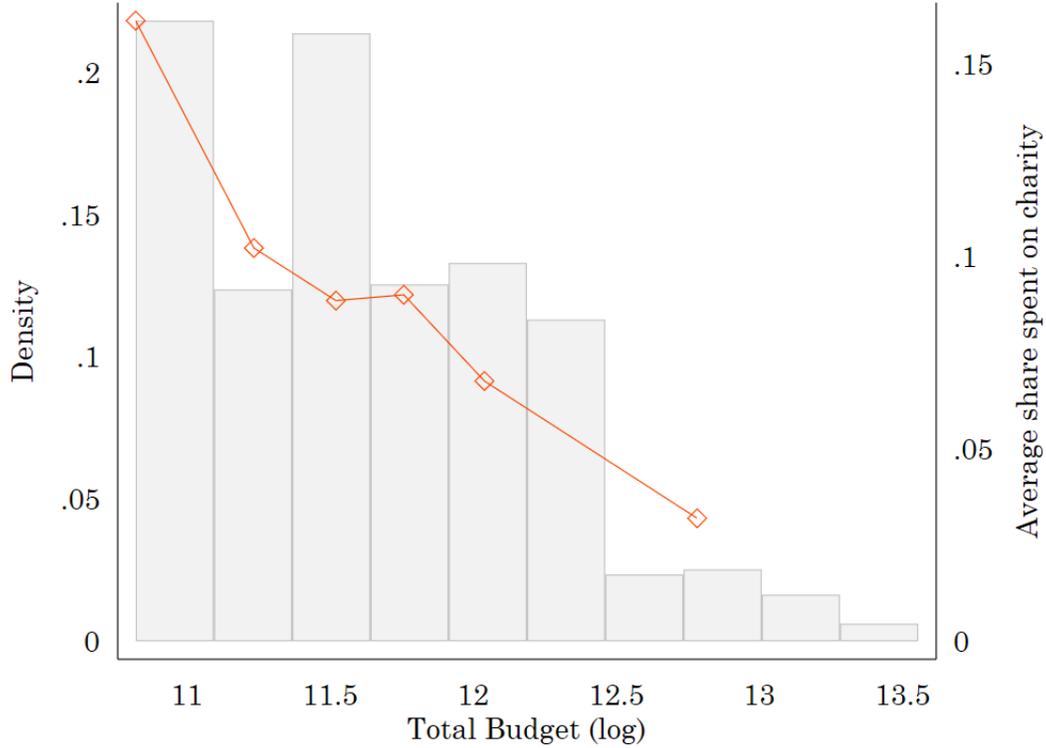
Notes: Binscatter plot overlaid on a histogram showing the distribution of the total budget variable. Each bin has the same number of observations.

5 Conclusion

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

Our findings indicate that respondents expect perceived SES to affect interactions, and they are willing to distort their consumption choices toward observable goods (attire, non-necessities, 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.

Our results call for more work on assessing the magnitude of the distortions to material

welfare that low income households are willing to incur in order to protect themselves against social discrimination. In particular, research is needed to ascertain whether these considerations reduce the take-up of social welfare programs in low- to middle-income countries. However, the phenomenon that we explore and find empirical evidence for in urban areas need not apply to rural settings, where households seem to know a lot more about the material welfare of others (Alatas et al., 2012) and where collectivism often replaces the individualism that prevails in cities (Thomas et al., 2020). There, discrimination against the poor may take other forms (e.g., Dasgupta, 1995; Walker, 2014).

Our findings raise 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 facing discrimination, 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 previously been 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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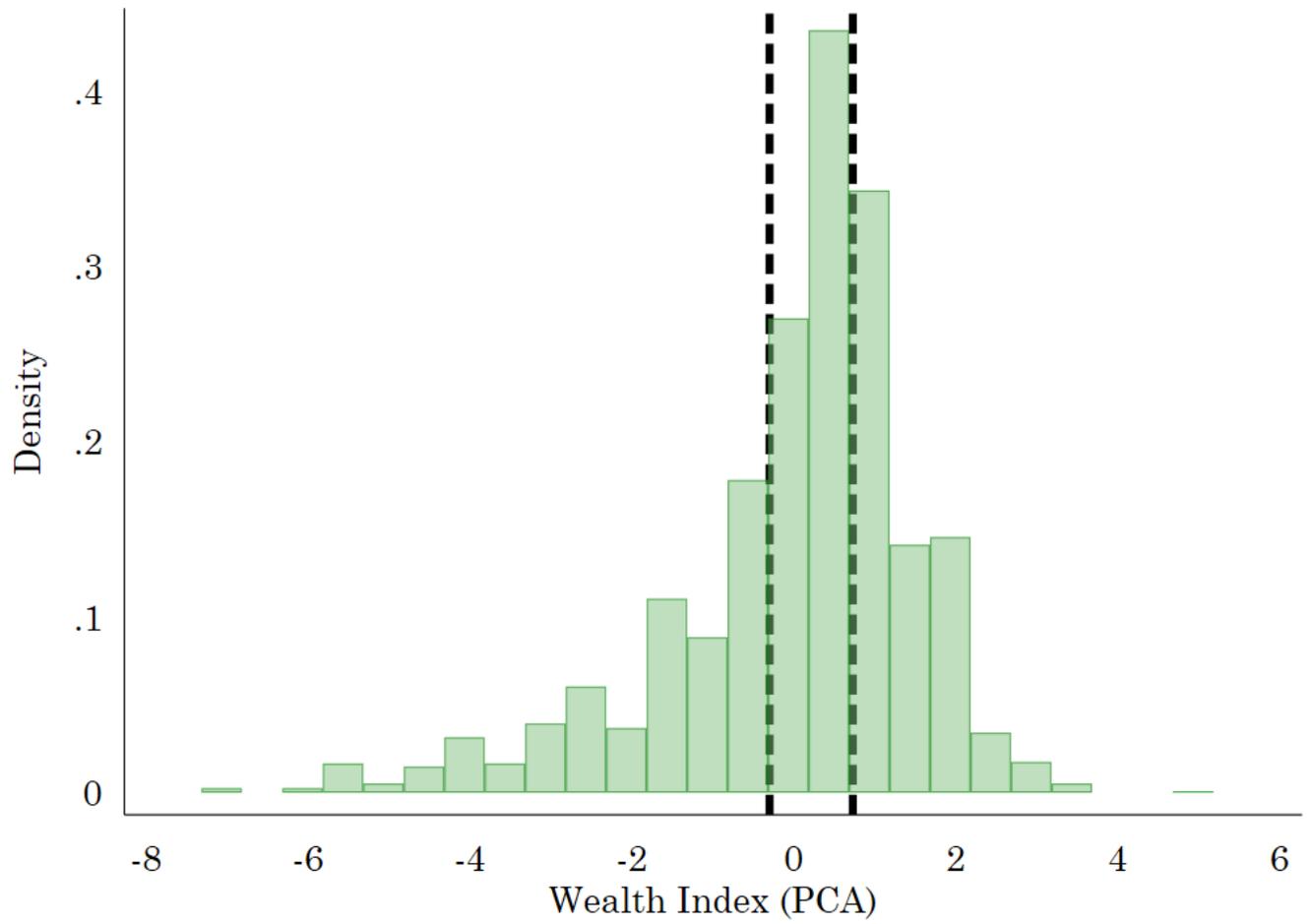**Walker, Robert**, *The Shame of Poverty*, Oxford University Press, 2014.

Online Appendix

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A   Appendix Figures and Tables

Figure A.1: Heterogeneity in SES within the Study Sample

Figure A.2: Raffle choice by income level
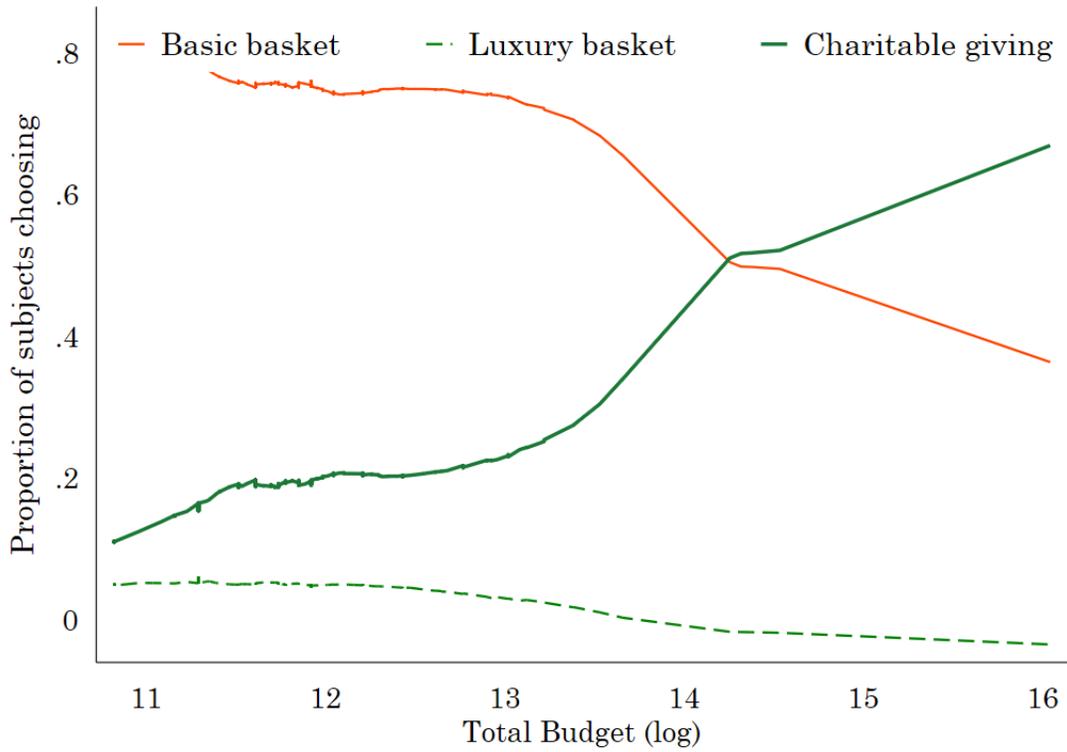
Notes: Sample restricted to respondents whose choice was private. The figure shows non-parametric (lowess) regressions of the indicator variables for choosing the basic basket, the luxury basket and the charitable contribution on the (log of the) respondent's budget, defined as the reported monthly income 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
HH size	6.12	3.39
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 (/1000 FCFA)	104.24	304.18
HH food expenditure, last 7 days (/1000 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 (/1000 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: Data source: AUDRI survey wave 2 (November 2022 to February 2023). 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***	1.34***	1.67***	1.32***
	(0.11)	(0.11)	(0.13)	(0.14)	(0.14)
Low SES picture	-0.62***	-0.49***	-0.61***	-0.47***	-0.59***
	(0.11)	(0.11)	(0.14)	(0.14)	(0.13)
Mean Medium SES	5.97	6.10	6.20	5.85	6.04
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	-1.132***	-1.294***	-1.087***	-1.193***	-1.354***
	(0.108)	(0.106)	(0.133)	(0.137)	(0.125)
Occupation: Unemployed	0.099	0.125	0.143	0.075	0.130
	(0.112)	(0.117)	(0.133)	(0.146)	(0.141)
Occupation: Casual Worker	0.257*	-0.065	0.161	0.086	0.056
	(0.142)	(0.135)	(0.165)	(0.173)	(0.172)
Father is civil servant	0.286***	-0.065	0.077	0.104	0.141
	(0.103)	(0.103)	(0.125)	(0.132)	(0.121)
Mean no free meal	6.75	6.87	6.78	6.79	6.86
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	3.67***	1.44***	1.32***	2.27***
	(0.08)	(0.07)	(0.07)	(0.07)
Occupation: Unemployed	-0.16**	-1.02***	0.30***	-2.23***
	(0.07)	(0.08)	(0.09)	(0.09)
Occupation: Self-employed	0.03	-0.43***	0.17**	-0.73***
	(0.07)	(0.07)	(0.08)	(0.08)
Mean score	5.13	6.45	6.16	5.42
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.Clustered standard errors at the respondent level.

Table A.5: Self-selection into documentaries

	(1) All		(3)	(4) Private Choice		(6)
	(1)	(2)	(3)	(4)	(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	0.27	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 an indicator variable 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)	0.016	0.018
	(0.032)	(0.020)
Top wealth tercile (PCA index)	0.052	0.054**
	(0.035)	(0.021)
Above secondary	0.113*	0.103***
	(0.058)	(0.033)
Above 50 years old	-0.045	-0.028
	(0.035)	(0.020)
Female	-0.071**	-0.073***
	(0.028)	(0.017)
Speaks french at home	0.033	0.018
	(0.034)	(0.020)
Ivorian	0.014	-0.014
	(0.044)	(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.

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:

$$max_{r \geq 0} U(c) \text{ subject to } c = y - q(d) - \tau(y - d - R) \tag{3}$$

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 discriminated 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 discriminated – 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 discriminated/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 discrimination is driven by a negative moral judgement (Walker, 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 discriminated; 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

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 distort their consumption to appear less poor.	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

In what follows, we present, **in chronological order**, the various activities conducted with respondents during the wave 2 of the AUDRI survey, which took place between late November 2022 and late February 2023, and which forms the core of the data in this paper. We also describe the additional choice experiment conducted in Spring 2023 (mini-job).

D.1 Raffle

In the AUDRI survey wave 2, 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 104 USD (median 60 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.[23]

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[23]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

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).[24] 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.

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.

[24]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 were made public on the Stanford AUDRI website and on the IHfRA website for about a year. Participants in the documentary were not remunerated for their participation.

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.[25] 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.[26]

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.

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[25]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.

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

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. All pictures were scored and ranked by at least two mini-jobbers on each criteria.[27] 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.[28] 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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[27]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.

[28]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?