Urbanization and Interethnic Relations in Africa*

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Abstract

How does urbanization affect interethnic relations? I address this question by leveraging evidence from economics games conducted in urban and rural Kenya, complementing prior research that focuses on urban populations alone or that characterizes interethnic relations via survey-based measures. While I find evidence that both urban and rural participants expect outgroup members to harbor ethnic biases, I find no evidence for differential altruism or cooperation toward ethnic outgroups, either among urban or rural participants—including when coding those participants using a measure that captures the continuous nature of urban-rural connections. I do, however, find evidence of cross-group bias along a dimension of interethnic relations rooted in like/dislike, captured via a different measurement strategy. Moreover, I find that this bias differs across urban and rural domains, with stronger coethnic preferences among rural participants. The results point to a potential weakening of (at least one dimension of) cross-group bias due to urbanization.

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Introduction

The movement of populations from rural to urban areas is one of the central elements of development, with wide-ranging impacts on economic behavior, political participation, and social preferences. This paper focuses on the effect of urbanization on social preferences—in particular, its impact on interethnic relations. It asks: how are orientations toward non-coethnics (as reflected in altruism toward members of other ethnic groups, willingness to cooperate, expectations of generosity, and like/dislike of ingroup versus outgroup members—summarized here under the label "ethnic bias") shaped by exposure to urban life? It addresses this question by contrasting evidence for these orientations across urban and rural dwellers in Kenya, an ethnically heterogeneous country undergoing rapid urbanization.

There are many reasons to think that orientations toward non-coethnics may be affected by moving to an urban environment, although the directions of these effects are in many cases unclear and contradictory. Urban environments are more diverse than rural areas, implying that urban residents are in much more frequent contact with members of other ethnic groups.[1] Such contact has been shown to reduce social distance (Pettigrew and Tropp 2006; Kasara 2013; Scacco and Warren 2018; Mousa 2020; Grady et al. 2023) but also to generate intergroup conflict and outgroup bias (Young 1976; Bates 1983; Enos 2014; Condra and Linardi 2019). Urban residents tend to be wealthier, better educated, and more exposed to the media than their rural counterparts. These factors again have an ambiguous impact on cross-group relations: they may promote the emergence of cosmopolitan identities and make ethnic affiliations less salient (Lerner 1958; Deutsch 1961; Brooks 2014) or they may heighten the importance of ethnic group memberships by increasing competition over jobs and public resources (Melson and Wolpe 1970; Bates 1983). Differences in the principal modes of economic activity in urban and rural areas (Green 2023), the proximity and salience of traditional leaders (Koter 2013), the depth of attachment to political parties (Harding and Michelitch 2021), and the strategies that elites use to mobilize political coalitions (Bleck and van de Walle 2018; Kim and Horowitz 2022) have also been hypothesized to generate differences in the strength of ethnic ties and the tenor of intergroup relations in urban and rural settings.[2]

The variety of channels through which urban life may impact cross-group interactions, along with the inconsistency in the theoretical expectations about the direction of these effects, has given rise to a number of investigations into the impact of urbanization on interethnic relations. These studies

[1] Intergroup contact can be both broad (i.e., exposure to non-coethnics in one's neighborhood) and deep (i.e., inter-ethnic marriage), with likely different impacts on outgroup bias (Chakraborty et al. 2024).

[2] Urbanization may also have the effect of neither heightening or reducing the salience of ethnic differences but shifting the dimension of ethnic identity that matters, from narrower "tribal" identities to broader regional or linguistic identities (Gluckman 1960; Posner 2005; Green 2023).

vary in the empirical approaches they employ, the specific outcomes they consider, and the findings they report. For example, Robinson (2014) and Kramon et al. (2022) draw on survey data to study the importance that urban and rural dwellers attach to their ethnic identities, finding weaker ethnic identification among urban residents. Nathan (2016) uses survey and administrative data to test whether ethnic voting diminishes with urban location, finding that the tendency for urban voters to support coethnic candidates and parties varies with local demography and socioeconomic status. Kim and Horowitz (2022) show that urban voters penalize politicians for making overt ethnically-based appeals and that this tendency increases with length of residence in the city, suggesting that rural voters may be less likely to punish ethnic pandering. Kramon et al. (2022) and Lyon (2023) employ survey data to show that interethnic trust is lower for city dwellers than for individuals living in rural areas. Barriga et al. (2023) and Peterson (2016), whose approach is the closest to the one adopted here, employ economics games to study some combination of inter-ethnic trust, inter-ethnic cooperation, and differential generosity to ingroup and outgroup members among rural and urban Africans. Whereas Barriga et al. (2023) find no evidence for ethnic bias in either urban or rural populations, Peterson (2016) reports evidence of cross-group bias in locations ranging from rural villages to large cities. Berge et al. (2020) also use economics games to investigate ethnic bias in Nairobi, Kenya. Although they do not explicitly study differences across urban and rural areas, they do find evidence that recent urban migrants display more cross-group bias than longer-term urban residents—a finding suggestive of stronger ethnic bias in rural areas, and of the potentially bias-reducing effects of moving to an urban location.

The present study joins these efforts to understand the impact of urbanization on interethnic relations. Like some of the papers just discussed, I use economics games—including a Dictator Game (DG) and a Public Goods Game (PGG)—to study differential generosity and cooperation across coethnic and non-coethnic partners. Economics games have an advantage over survey-based approaches for capturing interethnic relations because they provide incentivized, behavior-based measures of cross-group interaction rather than self-reported affinities, beliefs, and attitudes, often about hypothetical situations. Beyond the use of such games, the study departs from prior research in several noteworthy ways.

First, in measuring the character of interethnic relations, I supplement the findings from the economics games with the results from an Affect Misattribution Procedure (AMP), an experimental task borrowed from social psychology that captures affect or like/dislike (Payne et al. 2005), a distinct and important aspect of a person's orientation toward others (Russell 2003). While the affect felt toward

ingroup members and/or the dislike felt toward outgroup members is likely correlated with differential generosity or cooperation across ethnic lines (as captured in the DG and PGG), these are different dimensions of bias, and expanding the types of cross-group bias we investigate allows for a deeper understanding of interethnic relations.

Beyond the additional outcome that the AMP allows us to capture, including it alongside the DG and PGG also makes it possible to investigate whether social desirability bias (in the context of the present study, relating to the desire to try to hide one's favoritism toward and/or greater willingness to cooperate with coethnics) may be affecting the results in the economics games. This is because the AMP, as a speeded task, makes it harder to conceal one's biases than in the DG or PGG, which provide opportunities for participants to reflect on their choices before conveying their decisions (Hazlett and Berinsky 2018; Blum, Hazlett and Posner 2021).[3] A finding of ethnic bias in the AMP alongside a finding of no ethnic bias in the DG or PGG would suggest that participants may be taking advantage of the opportunity to censor their responses in the two behavioral games.

Comparing the DG/PGG and AMP results across urban and rural participants can also provide insight into whether social desirability bias varies across urban and rural populations. A finding that it does would hold potentially important implications for our understanding of how urbanization shapes intergroup relations. For example, it might point to the impact of urbanization lying less in the reduction of cross-group prejudices than on the socialization to trying to hide such prejudices in public interactions. The net effects of these two scenarios would be the same in terms of expressed prejudice, but the implications for behavior when one's actions are not publicly visible—for example, in the voting booth, in private business transactions, or in other non-public settings—would be quite different.

A second important departure from prior work involves moving beyond a dichotomous distinction between urban and rural. Recent evidence suggests that 70 to 90 percent of household heads in Nairobi were born in a rural area (Emina et al. 2011; Onyango, Cruch and Owour 2021). While some of these urban migrants can be considered fully "urban," in the sense of having completely disconnected themselves from rural life, others retain strong links to their rural homes (Gugler 2002; Robinson and Riedl 2025). Many engage in frequent contacts with their rural kin, plan to return someday to live in their village, and associate mainly with coethnics from their home region. Fully 76 percent of urban participants in the present study say they have a specific place outside of Nairobi they consider home. Such migrants are likely less "urban" in their orientations and behavior than individuals born in an

[3]In this respect, the AMP is similar to the more familiar Implicit Association Test (IAT), which also uses speeded responses to uncover biases that are more easily concealed in more traditional measurement approaches (Peréz 2013). I use the AMP because it is easier to implement than the IAT and has been shown to compare similarly in terms of effect size and reliability (Cameron, Brown-Iannuzzi and Payne 2012).

urban area who marry someone from a different ethnic group and maintain few connections to their ancestral village—despite the fact that all such participants would be coded as urban in most prior analyses. The same is true for the "ruralness" of participants recruited from rural areas. A resident of a rural district who frequently visits and communicates with urban-based family members or who previously lived in an urban part of the country will likely be less "rural" in their orientations and behavior than someone who never traveled out of their village and has few connections with urban-based kin—even as both would be treated as rural in most studies.[4] To better capture the non-binary nature of "urbanness" and "ruralness," I draw on information about the connections that urban and rural participants maintain with their rural and urban kin to create continuous measures of these characteristics, which I use to supplement the dichotomous coding of participants based on the places where they were recruited. Among urban participants, I also create a measure of "exposure" that captures the extent to which the participant has contact with members of other ethnic groups. As I demonstrate below, these measures capture significant variation among urban and rural participants that is lost in analyses that assume a simple urban/rural dichotomy. I re-analyze all of my results using these continuous measures of urban and rural life.

Third, I go beyond documenting cross-group altruism (via the DG) to investigate *expectations* of altruism using an additional economics game, the Choose Your Dictator (CYD) game, introduced in Berge et al. (2020). In the CYD, participants play the role of receiver and choose between a coethnic and a non-coethnic to be the dictator, with their choice indicating their expectations about the relative generosity of in-group and outgroup dictators. Analyzing expectations of outgroup members' generosity is important insofar as the salience of ethnicity in politics and social life may depend as much or more on how one thinks non-coethnics will behave than on one's own predispositions toward non-coethnics. For example, voters disinclined to support ingroup candidates simply because they share an ethnic tie may nonetheless vote along ethnic lines if they believe that others will only support coethnics or that politicians will favor members of their own groups (Posner 2005). Nathan (2016) makes precisely this argument in his study of urban Ghana, where he shows that ethnic voting is driven by expectations of ethnically biased allocations of goods rather than by the importance people attach to ethnic differences themselves.[5] Incorporating the results from the CYD game allows me to evaluate whether urban and rural populations differ in their expectations of ethnic bias by non-coethnics. If they do, it will suggest that an important impact of urbanization may lie in how it alters expectations

[4]This is not just an African phenomenon. Many US survey respondents identify as "rural" even though they do not live in rural areas (Trujillo 2024).

[5]Drawing on data from urban India, Spater (2022) shows similarly that outgroup prejudice is unrelated to coethnic voting.

of how outgroup members will behave rather than simply in changes in one's own attitudes or behavior toward non-coethnics.

To preview my findings, I find little evidence for differential altruism or willingness to cooperate across ethnic lines among either urban or rural populations—both when treating those populations as distinct monolithic blocks based on the locations from which participants were recruited and when I adjust the urban/rural coding to account for the connections that urban participants maintain with rural kin, and vice versa. These null findings validate the results reported in prior work using similar economics games to measure ethnic bias in urban Kenya (e.g., Berge et al. 2020; Blum, Hazlett and Posner 2021) but, as in Barriga et al. (2023), broaden them to show that they extend to rural populations as well.

I do, however, find evidence for expectations of in-group favoritism in the CYD, even though study participants are not in fact making more generous allocations to coethnics in the DG. This finding, which holds equally among urban and rural participants, points to an important aspect of how ethnicity affects social and political life: by shaping expectations of how others will behave as much, and perhaps more, than influencing how others actually do act.

Notwithstanding the absence of evidence for cross-group bias in the DG and PGG, I find consistent evidence for ethnic bias in the AMP, with stronger dislike for outgroup members among rural than urban participants. As noted, there are two different interpretations for these results. The first is that cross-group biases exist in the study population but are hidden in the traditional economics games-based measurement tools, which permit participants to conceal their biases. The second is that urbanization may be associated with a lessening in outgroup bias for the particular dimension of bias that the AMP captures, which is different from the aspects of bias measured in the DG and PGG. To the extent that the former interpretation holds, the finding of stronger out-group bias in the AMP among rural than urban participants, combined with the null results in the DG and PGG in both settings, suggests that the inclination to hide one's positive bias toward coethnics and/or negative bias toward non-coethnics may actually be stronger in rural areas, running against the hypothesis of urban socialization to conceal distasteful outgroup biases.

Taken together these results provide insight into how interethnic relations differ in urban and rural Africa and, by extension, into the impact of urbanization. It would appear that exposure to urban life does not alter altruism toward coethnics versus non-coethnics, willingness to cooperate along ethnic lines, or expectations about the generosity of ingroup versus outgroup members. But it does seem to be associated with changes in the like or dislike that individuals feel toward ingroup versus outgroup

members. The fact that such ingroup bias is weaker in urban areas suggests that urbanization may reduce social distance and foster intergroup cohesion—even if not through the outcomes that prior studies have measured.

Research sites and sample

Estimating the causal effect of urban migration is challenging because the kinds of people who migrate from rural to urban areas are likely different from the kinds of people who do not. This means that any differences we observe across urban and rural populations could stem either from the impact of exposure to urban life or from the distinctive individual characteristics that vary across people who chose to migrate and people who choose to remain in their rural homes.[6] Researchers seeking to estimate the impact of urbanization have dealt with this selection problem in several ways.

Perhaps the strongest response in the political science literature is offered by Kramon et al. (2022), who leverage data from the Kenya Life Panel Survey to follow individuals over time as they migrate from rural to urban areas, using individual-level fixed effects to partially address concerns about selection into migration. However, even this strategy has its drawbacks. First, it requires longitudinal data gathered over many years that, in the case of Kramon et al. (2022) and most studies like it, is collected within a single rural setting and then used to follow the subset of survey respondents who migrate.[7] This limits the ability to generalize to migrants from other rural areas, for whom the impact of urban exposure may differ due to ethnic affiliation, socioeconomic status, distance from the city to which they have migrated, or other factors. A second drawback is that the individual fixed-effects approach employed in such studies does not allow for estimating effects among non-movers, whose response to the urban environment may have been different from the responses of those who selected into migration.

An alternative approach, employed in Berge et al. (2020), Kim and Horowitz (2022), Lyon (2023), and Robinson and Riedl (2025), is to study urban populations alone and make inferences about the effects of urban life by comparing recent migrants and long-term urban residents. While this approach sidesteps the problem of selection into migration (as everyone in the study, or their parents, has already chosen to migrate), it shares the drawback of the longitudinal approach in omitting non-movers from the analysis. It also assumes that length of residence in the urban domain is correlated with the

[6] For a particularly compelling demonstration of the consequences of selection bias in studies of urban migration, see Hicks et al. (2020).

[7] Because the leverage in such studies comes from the subset of original survey respondents who migrate, the effective sample size tends to be small. The Kramon et al. (2022) study is an exception because its sample is so large. Thus, the 23 percent of respondents who moved to one of the country's two major urban areas across survey rounds comprise nearly 1,600 individuals. Beegle, De Weerdt and Dercon (2011), another prominent study using a similar panel approach, is more typical: only 138 individuals are observed in both rural and urban areas.

intensity of the urban "treatment."

The strategy adopted here (as in Bannon, Miguel and Posner (2004), Green (2013), Robinson (2014), Peterson (2016), Barriga et al. (2023), and Lyon (2023)) is to compare outcomes across urban and rural populations. While selection bias can be addressed to some degree in this approach by controlling for the individual characteristics that differ across urban and rural participants (as I do in the analyses that follow), this is an imperfect solution, and the results presented below should be interpreted with this caveat in mind. The contribution here, therefore, is not to overcome the challenges to estimating the causal effects of urbanization on interethnic relations but to offer insight into those effects by, for one of the first times, using economics games to measure ethnic bias across urban and rural populations in an African setting.[8]

To sharpen the focus on politically relevant inter-ethnic differences, and also to maximize study power, I limit the sample to members of the Kikuyu and Luo ethnic groups, the two most politically relevant and historically antagonistic groups in Kenyan national politics (Hornsby 2013; Africa Report 2018). All out-group partners presented to Kikuyu participants in the economics games are Luo, and all out-group partners presented to Luo participants are Kikuyu. The study was conducted a decade after the violence surrounding Kenya's disputed 2007 presidential election and shortly before another election in which the leading candidates were again Kikuyu and Luo. Ethnic tensions between members of these groups were thus high at the time of the laboratory sessions.

I recruited participants from Nairobi (specifically, the multi-ethnic compounds of Kibera and Kawangware, N=318) and the rural districts of Kirinyaga (N=171) and Siaya (N=164), which are overwhelmingly populated by Kikuyus and Luos, respectively. The locations of the sites are shown in Figure 1. This sampling strategy makes it possible to compare relations between members of the same two ethnic groups living in both urban and rural settings.

The urban sample, in addition to being limited to equal numbers of Kikuyu and Luo participants, was stratified by age, gender, and length of residence in Nairobi.[9] The two rural samples were stratified by age, gender, and frequency of contact with Nairobi. As expected, the urban sample is better educated and somewhat younger than the rural sample (see Appendix Table A1.) For this reason, as pre-registered, I control for age and education in the analyses that follow. The balance table also reveals differences across urban and rural participants in income, media exposure, and religious

[8]The only prior studies that employ economics games to measure ethnic bias across both urban and rural populations are Peterson (2016) and Barriga et al. (2023). Bannon, Miguel and Posner (2004), Green (2013), Robinson (2014), and Lyon (2023) draw on survey data to compare the strength of ethnic identification among urban and rural Africans, but this is a much narrower aspect of inter-ethnic relations than the outcomes studied here.

[9]With respect to length of residence in Nairobi, the sample was stratified into three equally sized groups: long-term residents ($>$ 20 years), mid-term residents (10-20 years) and short-term residents ($<$10 years).

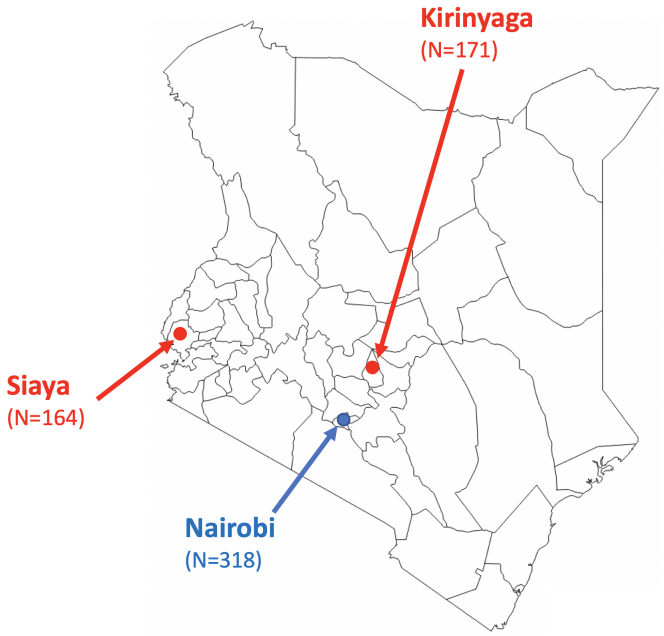


Figure 1: Research sites

affiliation. Although not pre-specified, I also report analyses that include these additional covariates.

Subjects participated in the study activities in all three research sites using a mobile lab facility run by the Busara Center for Behavioral Economics. In Nairobi, participants were recruited from the Busara Center's pre-existing subject pool.[10] In the two rural districts, participants were recruited each day from different non-adjacent villages in order to reduce the likelihood (otherwise high) that people in the community would talk with one another, which might have caused subjects in later sessions to have been briefed regarding the purpose of the research.

The lab sessions took place in July 2017. Given research indicating that proximity to national elections may affect the salience of ethnic identity (Eifert, Miguel and Posner 2010), the lab sessions were scheduled more or less simultaneously in all three sites to equalize the distance from the coming general elections, which were held on August 8.[11] Participants were paid Ksh 200 (about $2) for attending the session, plus an additional 50 KSh if they arrived on time. They also received a reimbursement for their transport expenses, as well as whatever they earned from their play in the experimental games. Payments were made via MPESA mobile money transfer after the conclusion of the lab session, both to reduce opportunities for learning during the course of the experiment and to prevent participants' knowledge of their accumulating earnings from altering their risk preferences as the trials progressed.

[10]To ensure comparability with the two rural sites, where the Busara Center had not previously operated, only participants who had not participated in a prior experimental study were admitted.

[11]The Nairobi lab sessions took place from July 7-19; the Kirinyaga sessions from July 7-17; and the Siaya sessions from July 8-15.

Measuring interethnic relations

My main measure of interethnic relations is the difference in the mean contributions across coethnic and non-coethnic partners in the DG and PGG. Economics games like the DG and PGG offer powerful tools for measuring cross-group bias and have been increasingly adopted by researchers studying intergroup relations in Africa and beyond.[12] In the DG, participants are given a sum of money (in this case KSh 100) and asked to divide it between themselves and a receiver, keeping whatever portion they choose. The share of the endowment they voluntarily give away serves as a measure of altruism (Kahneman, Knetsch and Thaler 1986). In the PGG, participants are paired with two other players. They are given a sum of money (again, KSh 100) and asked to decide how much to keep for themselves and how much to give to a group fund in which contributions are doubled and then shared equally among all three players. Each player gets to keep their share of the group fund, plus whatever they decide not to contribute. Because contributions in the PGG depend on expectations of what the two other players will contribute, they serve as a measure of cooperation.[13] Ethnic bias can be measured using these games by observing whether participants are more altruistic in the DG when paired with a coethnic versus a non-coethnic partner, and more cooperative in the PGG when the two other players are coethnics versus non-coethnics.

Each lab session began with an on-screen literacy test, which involved distinguishing between positive and negative words in the participant's local language. Participants then completed a set of Raven's matrices, designed to measure cognitive ability (Raven 2008). Instructions for the games and tasks were provided on the computer screens in both English and Swahili (in Nairobi) and in Kikuyu and Dholuo (in Kirinyaga and Siaya, respectively), in addition to being read aloud in the appropriate language by a trained lab leader. For parts of the session where subsets of participants received different instructions or where they completed games or tasks at their own pace, participants put on headphones and received instructions via recordings in Swahili, Kikiyu, or Dholuo, as appropriate. A flowchart indicating the ordering of the tasks in the lab sessions is provided in Appendix Figure A1.

In the DG, participants were shown a photograph of the receiver and given his first name, age, education, and hometown, with the hometown selected so as to provide an indication of the receiver's likely Kikuyu or Luo origins (see Figure 2).[14] Hometown was used as a cue for ethnicity rather than

[12] In addition to the already cited studies, prominent examples in the African context include Habyarimana et al. (2009), McCauley (2014), Burbidge and Cheeseman (2017), Jeon, Johnson and Robinson (2017), and Haushofer et al. (2023).

[13] The incentives facing players in the PGG, as well as the interpretation of its results, are complex. For useful discussions, see Ledyard (1995); Camerer and Fehr (2004) and Habyarimana et al. (2009).

[14] All of the images used in the DG and other games were of men in order to reduce the number of factors that might affect participants' response to the stimulus. The photographs were pre-screened to be ethnically ambiguous so

the labels "Kikuyu" or "Luo" because using overt ethnic labels was potentially inflammatory in the pre-election context in which the study was conducted.[15] Using such labels also risked signaling that the experiment was about ethnic discrimination, which may have heightened social desirability bias. The inclusion of information about the receiver's age and education was meant to further obscure the ethnic focus of the study.

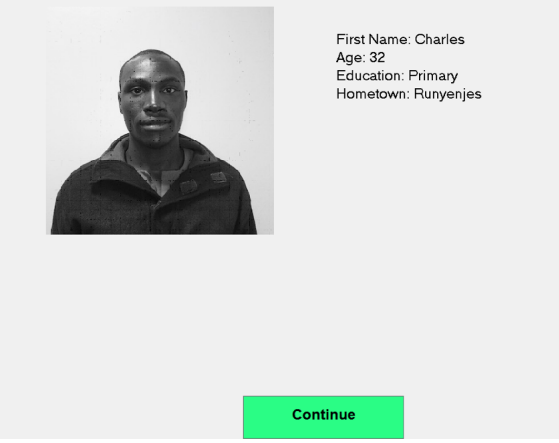


Figure 2: Information Provided About the Other Players in the DG and PGG

The DG was played twice: once with a receiver from a coethnic hometown and once with a receiver from a non-coethnic hometown, with the order randomized. Participants were informed that the receiver was an actual person to whom the money they allocated would be given, and money was in fact transferred to the person shown in the picture at the end of the experiment.

As in the DG, participants in the PGG were shown photos and provided the names, ages, education levels, and hometowns of the two other players, one at a time, as shown in Figure 2. Participants played three rounds of the PGG: one with a coethnic group in which both of the other players were from coethnic hometowns, one with a non-coethnic group in which both of the other players were from non-coethnics hometowns, and one with a mixed group. The order in which these three rounds were played was randomized. As in the DG, the other players were real and were paid based on the outcome of the game, as determined by the participant's contribution and contributions made by the other players in anonymous PGGs they played at the time their photos were taken, prior to the experimental sessions.

The outcome of interest in the DG is the percentage of the endowment transferred to the partner;

that the hometown cue, rather than phenotypical characteristics, would influence the participant's perception of the receiver's ethnic group membership. The name, age, and education were fixed to each photograph, but the hometown was randomized across participants. Each participant saw each photograph and associated profile just once. Across all of the sessions, each photograph was presented equally often as belonging to someone from a Kikuyu or Luo hometown.

[15] Berge et al. (2020) employ a similar strategy of cueing ethnicity via hometown and report that the vast majority of participants could correctly identify the intended ethnic backgrounds of their partners based on the hometowns ascribed to them.

in the PGG it is the percentage contributed to the group fund. I estimate the following pre-registered ordinary least squares model:

$$Y_i = \beta_0 + \beta_1 Coethnic_i + \beta_2 X_i + \epsilon_i \quad (1)$$

where Y_i is the transfer (in percent of the endowment) by participant i to his/her partner(s). $Coethnic_i$ is an indictor variable for being paired with a coethnic partner (or, in the PGG, being in a coethnic group),[16] X_i is a vector of control variables, and ϵ_i is the error term, clustered by individual. The main test of ethnic bias is the hypothesis $\beta_1 \neq 0$.

To test for the hypothesis that ethnic bias varies across urban and rural settings, I estimate the following equation:

$$Y_i = \beta_0 + \beta_1 Coethnic_i + \beta_2 Urban_i + \beta_3 Coethnic_i * Urban_i + \beta_4 X_i + \epsilon_i \quad (2)$$

where $Urban_i$ indicates whether participant i was recruited from an urban location (in this study, from Nairobi). The hypothesis of interest is that $\beta_3 \neq 0$.[17]

Main Results

The results for the DG are reported in Table 1. On average, DG participants give about a third of their endowment to their partner. However, there is no statistically significant difference in the amount they give to coethnics versus non-coethnics (column 2)—i.e., no evidence of differential altruism along ethnic lines. Nor is there evidence that urban participants are more generous than rural participants or that ethnic bias is present in either the urban or rural domains alone (column 3). Adding the pre-registered controls for gender, ethnic group, education, age, and Raven's score does not change these null findings (column 4), nor does adding the additional controls to account for other imbalances across the urban and rural samples (column 5).

We see similarly null results for the PGG (Table 2). On average, participants contribute about half of their endowment to the group fund. However, we see no differences in average contributions when the two other group members are coethnics versus non coethnics (column 2), and no differences in cooperation across urban or rural participants (column 3). Including controls leaves these null findings

[16] The omitted category in the DG is being paired with a non-coethnic partner. In the PGG the omitted category is being in a non-coethnic group (rounds with mixed groups are not analyzed).

[17] In the pre-analysis plan, I also pre-registered the intention to investigate whether priming participants to the possibility of election violence affected either levels of ethnic bias or differences in ethnic bias across urban and rural participants. I found no impact of priming to violence, so I do not report these analyses in the paper.

unchanged (columns 4-5).

Table 1: Dictator Game Results

	(1)	(2)	(3)	(4)	(5)
Intercept	0.328***	0.320***	0.319***	0.375***	0.431***
	(0.010)	(0.012)	(0.013)	(0.037)	(0.056)
Coethnic		-0.009	-0.008	-0.008	-0.008
		(0.014)	(0.019)	(0.019)	(0.019)
Urban			0.019	0.027	0.030
			(0.019)	(0.020)	(0.022)
Coethnic * Urban			-0.003	-0.003	-0.004
			(0.027)	(0.027)	(0.027)
N	1306	1306	1306	1306	1304
Pre-registered covariates				yes	yes
All covariates					yes

Outcome is the share of the endowment given to the other player. Pre-registered covariates are gender, ethnic group, education, age, and Raven's score (demeaned). All covariates also include log monthly income (Ksh), religion, and frequency of getting news from TV, newspapers, radio, and internet. *p<0.1; **p<0.05; ***p<0.01.

Table 2: Public Goods Game Results

	(1)	(2)	(3)	(4)	(5)
Intercept	0.469***	0.486***	0.489***	0.554***	0.516***
	(0.013)	(0.015)	(0.018)	(0.049)	(0.074)
Coethnic		-0.004	-0.011	-0.011	-0.011
		(0.018)	(0.025)	(0.025)	(0.025)
Urban			-0.041	-0.018	-0.010
			(0.026)	(0.026)	(0.029)
Coethnic * Urban			0.015	0.015	0.014
			(0.036)	(0.036)	0.036)
N	1306	1306	13060	1306	1304
Pre-registered covariates				yes	yes
All covariates					yes

Outcome is the share of the endowment contributed to the group fund. Pre-registered covariates are gender, ethnic group, education, age, and Raven's score (demeaned). All covariates also include log monthly income (Ksh), religion, and frequency of getting news from TV, newspapers, radio, and internet. *p<0.1; **p<0.05; ***p<0.01.

The design of the lab sessions, in which each participant played the DG and PGG both with coethnic and non-coethnic receivers/group members, makes it possible to reanalyze the data in a within-subjects framework. As reported in Appendix Table A4, this approach confirms the null findings presented in Tables 1 and 2, revealing no evidence of ethnic bias in either the DG or the PGG and no variation in effects across urban and rural participants.

The absence of ethnic bias in the urban sample in either the DG or the PGG accords with the findings reported in Berge et al. (2020), Blum, Hazlett and Posner (2021), and Barriga et al. (2023), who all find no evidence of cross-group bias in these games in similar, Nairobi-based samples. The

fact that these null results extend to the Kirinyaga and Siaya subsamples suggests that the absence of ethnic bias is not simply an urban phenomenon—notwithstanding the suggestive evidence in Berge et al. (2020) that it might be.[18] It would appear not to be the case that generosity toward non-coethnics and willingness cooperate with people from different—and even historically antagonistic—ethnic groups is something learned through exposure to the urban environment. The absence of these types of bias against outgroup members is equally apparent in both the urban and rural samples.

Social desirability bias

In a context, like Kenya, where social norms discourage discrimination across group lines, participants may have incentives to try to hide their prejudices toward outgroup members by altering their outwardly visible behaviors. These incentives will only be heightened in a laboratory setting where participants know that their allocation decisions are being observed. A weakness of economics games like the DG and PGG is that they provide opportunities for participants to reflect before making their decisions, allowing them to censor their responses so as not to violate social norms against outgroup discrimination (Cilliers, Dube and Siddiqi 2015; de Quidt, Haushofer and Roth 2018; Hamilton, Hazlett and Posner 2026). To the extent that participants took advantage of this opportunity, the null findings in Tables 1 and 2 may be misleading regarding the absence of differential altruism or the willingness to cooperate across group lines.

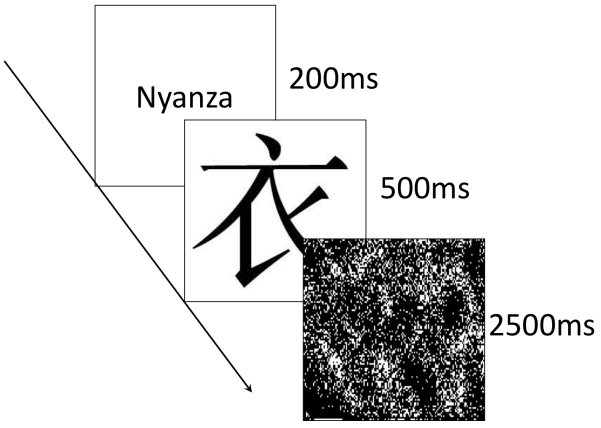
Investigating this possibility is important not only for confirming the validity of the results presented thus far but also for providing insight into whether social pressure against outgroup discrimination may vary in urban and rural areas. As noted earlier, a finding that it does would suggest that differences in bias across the two domains might be driven less by reductions in outgroup prejudice due to exposure to urban life than by socialization to try to conceal such prejudice in publicly visible behavior. That would be an important insight.

My strategy for addressing these questions is to supplement the evidence for ethnic bias collected via the DG and PGG with evidence from a third experimental task, the Affect Misattribution Procedure (AMP). Developed by Payne et al. (2005), the AMP is a speeded, priming-based task that is much less vulnerable to social desirability bias than economics games like the DG and PGG. The AMP works by leveraging the tendency for associations triggered by a particular stimulus to spill over and

[18]The divergence between these findings and the significant results reported in Peterson (2016) may stem from the modified version of the DG he employs, which involves asking participants to choose between two applicants for a small business loan. The set-up forces a binary choice between ingroup and outgroup applicants and prevents the dictator from keeping any of the endowment.

become associated with a second stimulus—in the case of the AMP task, an ambiguous image with no affective content. Because participants are unaware of the influence that the first stimulus has on their response to the second, researchers can learn about the participants' affective reaction to the first stimulus by asking them about their judgments about the second. This makes the AMP a valuable tool for measuring outgroup prejudice in a context where participant may wish to hide their biases.[19] Given the absence of cross-group bias found in the DG and PGG, a finding of ethnic bias in the AMP would be consistent with the presence of underlying biases that participants are able to hide in the two economics games. A finding of bias in the AMP in the urban but not rural samples would be consistent with differential motivations to hide one's prejudices in these two domains—potentially due to the socialization against open displays of ethnic bias in the urban setting.

In the version of the AMP employed in the present study, participants are first shown a priming word for 200 ms, which they are told to ignore, and then shown an innocuous image (a Chinese character) for 500 ms, followed by a white noise mask. The participant is asked to rate the image as pleasant or unpleasant by pressing the appropriate key on a keyboard. The priming word is a Kikuyu or Luo hometown in one version of the task (AMP-Town) and a region ("Central" or "Nyanza," the former strongly associated with Kikuyus; the latter with Luos) in the other (AMP-Region). The idea is that whatever positive or negative affect the participant may have toward coethnics or non-coethnics (as triggered by the priming word) will be reflected in their response to the Chinese character—all without the participant being aware that they are providing information about their disposition toward ingroup and outgroup members. A diagram depicting the flow of an AMP trial is provided in Figure 3.



Source: Blum, Hazlett and Posner (2021).

Figure 3: The Affect Misattribution Procedure (AMP)

Each version of the AMP contained 30 trials, with the priming words randomized. As pre-registered,

[19]For a further discussion of the AMP and other misattribution-based tasks as tools for measuring ethnic bias, as well as a comparison of results using the DG, PGG, and AMP in a similar setting, see Blum, Hazlett and Posner (2021).

the results presented here combine the town and region versions and report outcomes averaged across all 60 trials. For each participant, I calculate an "ethnic effect" measuring how responses differ across trials depending on whether the participant received a coethnic or non-coethnic prime.[20] I then take the average of these effects across all participants as my measure of ethnic bias.

The AMP results are presented in Table 3. Unlike in the DG and PGG, we see strong effects of ethnicity. The significant positive estimates for the intercept term indicate that participants are significantly more likely to say that they find the Chinese character pleasant when they are first primed to be thinking about a coethnic. Since the affect attached to the Chinese character is mediated by the priming to think about a Kikuyu or Luo hometown or region, the interpretation is that participants have more positive affect toward the ingroup than the outgroup.[21]

Table 3: Affect Misattribution Procedure Results

	(1)	(2)	(3)	(4)
Intercept	0.121***	0.121***	0.126***	0.180***
	(0.014)	(0.014)	(0.023)	(0.072)
Urban		-0.034	-0.057**	-0.066**
		(0.024)	(0.026)	(0.032)
N	442	442	442	441
Pre-registered covariates			yes	yes
All covariates				yes

Outcome is the "ethnic effect," the estimated probability of finding a Chinese character pleasant after exposure to a coethnic prime minus the estimated probability of finding the Chinese character pleasant after exposure to a non-coethnic prime. As pre-registered, results of the AMP-Town and AMP-Region are combined. Sample is limited to participants who correctly answered half or more of the questions in the six question literacy test administered at the start of the lab session and whose reaction times were $\leq$ 200ms, as pre-registered. Pre-registered covariates are gender, ethnic group, education, and age. All covariates also include log monthly income (Ksh), religion, and frequency of getting news from TV, newspapers, radio, and internet. *p<0.1; **p<0.05; ***p<0.01.

Viewed alongside the null results in Tables 1 and 2, these findings suggest that an underlying bias against non-coethnics exists but is not detected in the DG or PGG.[22] As noted, this could be either because the DG and PGG afford participants the ability to hide their biases whereas the AMP does not (in which case the findings suggest that social desirability bias may be affecting the results of the economics games) or because the AMP is simply measuring a different type of bias than the DG and PGG. In this latter case, the implication is that cross-group biases exist with respect to what the AMP measures (like/dislike) but not with respect to what the DG and PGG measure (altruism and

[20] Formally, $\text{EthnicEffect}_i = [r_i|\text{coethnic}] - [r_i|\text{non-coethnic}]$; r_i is an indicator for whether the target image (the Chinese character) was rated as pleasant.

[21] The underlying bias could be rooted in either positive bias towards coethnics or negative bias against non-coethnics. It is impossible to know from the experimental design which is driving the effects we see.

[22] These results echo those reported in Berge et al. (2020), who also find no evidence of ethnic bias in the DG and PGG but evidence of ethnic bias using the IAT.

cooperation, respectively).[23]

From the standpoint of understanding the impact of urbanization on interethnic relations, the more important result in Table 3 is the suggestive evidence for a weakening of the prejudice toward outgroup members among urban participants, as indicated by the negative sign on the *urban* variable in columns 2-4. Although the effect is only statistically significant in the analyses that include controls, this is reassuring, as these are the specifications that at least partially addresses concerns about selection into migration by controlling for individual characteristics that vary on average across urban and rural participants. The substantive size of these estimated effects is large, suggesting a roughly 40% decrease in ingroup bias among urban participants vis-a-vis their rural counterparts.

To the extent that the AMP findings can be interpreted as an indication of cross-group bias free from social desirability concerns, the results also speak to the question of whether urbanization may lead to socialization toward trying to hide one's biases against outgroup members. If that were the case, we would have expected to see stronger evidence of outgroup bias in urban areas in the AMP than in rural areas. The fact that we see the opposite weighs against this hypothesized effect of urban exposure.

Variation within the urban and rural domains

Thus far, my strategy for investigating whether urbanization is associated with changes in interethnic relations has been to compare outcomes among participants recruited from Nairobi and from the rural districts of Kirinyaga and Siaya. The assumption underlying this approach is that participants from Nairobi can be considered urban and participants from Kirinyaga and Siaya can be considered rural. However, as noted, this dichotomization belies significant variation in the linkages that urban dwellers maintain with rural family members, and vice versa.[24]

To better capture this within-category variation, I collected data on the connections that urban study participants maintain with their rural kin and the connections that rural study participants maintain with urban-based family members. These include questions asked of Nairobi participants about whether they have a place outside of Nairobi they consider home, how often they visit that place, how frequently they send money to family members living there, how well they know what is happening there, whether they have property or investments there, and whether they would someday like to be laid to rest there. These questions were inspired by survey items developed in Robinson and

[23] For an elaboration of this argument, see Blum, Hazlett and Posner (2021) and Hamilton, Hazlett and Posner (2026).

[24] The dichotomization also belies the fact that cities come in different sizes and contain distinct features, with different implications for what the urban experience entails, points emphasized in Lyon (2023) and Hoelscher et al. (2023).

Riedl (2025), who explore the multiple forms of rural connection that urban dwellers maintain with their rural kin. To get a sense of the connections that rural-based study participants have to urban areas (an issue that Robinson and Riedl (2025) do not address), I asked questions about whether they have family members living in Nairobi or another urban part of the country, how frequently they were visited by (or themselves visited) those urban-based family members, whether family members in Nairobi send them money, and whether they have ever lived in an urban area. I combine the responses to create pre-registered averaged z-score indexes of "urbanness" and "ruralness" for urban and rural study participants, respectively.[25] For participants recruited in Nairobi, higher values of the "urbanness" measure indicate weaker attachment to their rural home; for participants recruited from the two rural sites, higher values of the "ruralness" measure indicate weaker connections to urban life. The left and middle panels of Figure 4 plot the distributions of the two indexes. The broad variation we see in both figures conveys what is lost when we fail to acknowledge the variation in linkages that urban participants maintain with rural areas, and vice versa.[26]

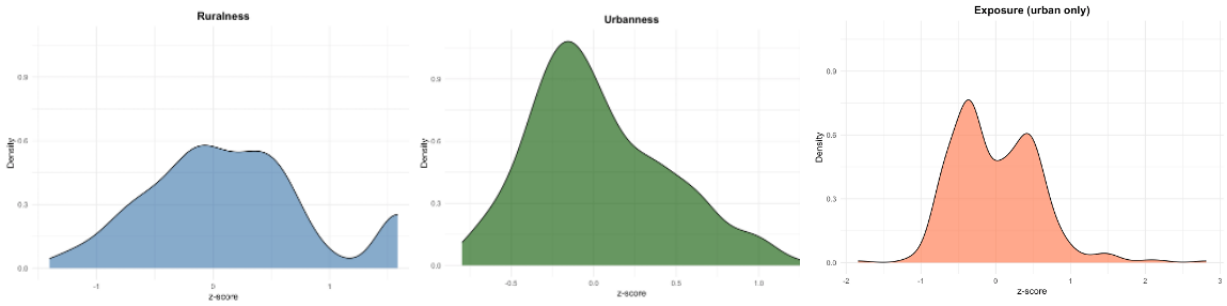


Figure 4: Variation in "Urbanness," "Ruralness" and "Exposure"

Accounting for these continuing connections is important for understanding the impact of urban or local location on people's attitudes and behavior—including bias toward non-coethnics. But, for urban migrants, moving from a rural to an urban place of residence is about more than just removing them from their rural communities. It also exposes them to a diverse environment in which interactions with people from other groups are much more frequent than in their rural homes. This is a critical feature of

[25]The full set of questions used in creating these indexes, along with descriptive statistics for each component, is provided in Appendix B.

[26]Quite apart from the variance in urbanness and ruralness shown in Figure 4, the *levels* of urban-rural connections revealed in the data are striking. As reported in Appendix B (and as noted earlier), 76 percent of urban participants said they have a specific place outside Nairobi that they consider "home." Seventy percent report communicating with someone in that place at least once a week, and 66 percent report planning to live there permanently in the future. Seventy-nine percent characterize being in good standing as a member of their home village as important, very important, or essential. Meanwhile, among rural participants, 84 percent report having family members living in Nairobi or other urban areas, 87 percent report having been visited by these urban family members at least once in the past two years, and 50 percent say they communicate with them at least once a week. These results, which echo the findings in many other studies (e.g., Gugler (2002); Potts (2010); Robinson and Riedl (2025), among others) further underscore the folly of viewing urban and rural Africans as occupying distinct and disconnected worlds.

urban life that is not well captured in the "urbanness" measure. To test for the impact of this distinct implication of urban residence, I also create a separate index of "exposure" built from questions about the frequency of personal contact with members of the ethnic outgroup, the ethnic diversity of the neighborhood in which the participant lives, and whether they or their parents are married to a person from a different ethnic group.[27] The distribution of the "exposure" index is plotted in the right panel of Figure 4.

In Tables 4, 5, and 6, I revisit the results presented earlier for the DG, PGG, and AMP, replacing the dichotomous urban/rural coding with the continuous measures to test whether ethnic bias varies not just with urban or rural location but, within each of the urban and rural subsamples, with the strength of participants' urban/rural connections (and, within the urban sample, exposure to non-coethnics). Note that, because the "urbanness" and "ruralness" measures are built from different sub-sample-specific items, the coefficients on these variables are not directly comparable. Their purpose is to test whether, within each sub-sample, the degree of connection to the other domain is a source of variation in outgroup bias.

Although using a measure that captures the continuous nature of urbanness, ruralness, and exposure is almost certainly an improvement on prior efforts to investigate the impact of urbanization, the updated estimates continue to indicate no impact of urbanization on either altruism, as measured via the DG (Table 4), or cooperation, as measured via the PGG (Table 5).

We do, however, see more interesting results when we turn to the AMP. Unlike in Table 3, where I pooled the results for all participants and tested for differences across urban and rural individuals via a dummy variable, in Table 6, I analyze the urban and rural samples separately. This separation confirms that the ethnic effect shown in Table 3 was driven by rural participants, who are statistically significantly much more likely to demonstrate stronger affect toward coethnics than non-coethnics. There is no ethnic effect among urban participants. This finding reinforces the result highlighted earlier regarding the reduction in affect-related bias against outgroups (or the reduction in positive bias toward ingroups) that seems to occur with urbanization. The fact that increases in "urbanness" among urban participants are associated with further reductions in such outgroup bias (although not significantly so) and that increases in "ruralness" among rural participants are associated with further increases in outgroup bias (although, again, not significantly so), is consistent with this interpretation.

Table 6 also reveals another interesting result: that greater exposure to non-coethnics among urban residents is strongly associated with greater affect-related ethnic bias. As noted in the paper's

[27] See Appendix B for details. The "exposure" index was not pre-registered.

Table 4: Dictator Game Results, Revisited

	Urban sample				Rural sample	
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	0.404***	0.403***	0.406***	0.406***	0.442***	0.442***
	(0.069)	(0.069)	(0.069)	(0.069)	(0.099)	(0.099)
Coethnic	-0.011	-0.010	-0.011	-0.011	-0.008	-0.006
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.020)
"Urbanness"	0.015	0.043				
	(0.026)	(0.035)				
Coethnic * "Urbanness"		-0.057				
		(0.046)				
"Ruralness"					0.020	0.024
					(0.014)	(0.020)
Coethnic * "Ruralness"						-0.009
						(0.027)
"Exposure"			-0.015	-0.014		
			(0.017)	(0.024)		
Coethnic * "Exposure"				-0.001		
				(0.033)		
N	634	634	634	634	670	670

Outcome is the share of the endowment given to the other player. All models include the full set of covariates: gender, ethnic group, education, age, Raven's score (demeaned), log monthly income (Ksh), religion, and frequency of getting news from TV, newspapers, radio, and internet. *p<0.1; **p<0.05; ***p<0.01.

Table 5: Public Goods Game Results, Revisited

	Urban sample				Rural sample	
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	0.623***	0.623***	0.631***	0.631***	0.478***	0.477***
	(0.091)	(0.091)	(0.091)	(0.091)	(0.130)	(0.130)
Coethnic	0.004	0.003	0.004	0.004	-0.011	-0.009
	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.026)
"Urbanness"	0.044	0.042				
	(0.034)	(0.046)				
Coethnic * "Urbanness"		0.003				
		(0.061)				
"Ruralness"					-0.009	-0.004
					(0.019)	(0.026)
Coethnic * "Ruralness"						-0.009
						(0.035)
"Exposure"			0.011	0.006		
			(0.023)	(0.032)		
Coethnic * "Exposure"				0.009		
				(0.044)		
N	634	634	634	634	670	670

Outcome is the share of the endowment contributed to the group fund. All models include the full set of covariates: gender, ethnic group, education, age, Raven's score (demeaned), log monthly income (Ksh), religion, and frequency of getting news from TV, newspapers, radio, and internet. *p<0.1; **p<0.05; ***p<0.01.

Table 6: Affect Misattribution Procedure Results, Revisited

	Urban sample		Rural sample
	(1)	(2)	(3)
Intercept	0.088	0.030	0.283***
	(0.089)	(0.088)	(0.127)
"Urbanness"	-0.059		
	(0.050)		
"Ruralness"			0.016
			(0.022)
"Exposure"		0.089***	
		(0.031)	
N	160	160	281

See notes to Table 3. All models include the full set of covariates: gender, ethnic group, education, age, Raven's score (demeaned), log monthly income (Ksh), religion, and frequency of getting news from TV, newspapers, radio, and internet. *p<0.1; **p<0.05; ***p<0.01.

introduction, contact with ethnic others has been hypothesized to have quite different impacts. Some researchers have found contact to reduce social distance and prejudice (Kasara 2013; Scacco and Warren 2018; Mousa 2020; Grady et al. 2023). Others have pointed to its impact on fostering competition and conflict, and hence increasing social distance and cross-group bias (Bates 1983; Condra and Linardi 2019). The results reported in column 2 are consistent with the latter hypothesis. The estimates suggest that, whereas urban participants with no "exposure" display no ethnic bias on the AMP task (the estimated ethnic effect is a statistically insignificant 0.030), urban participants with the highest level of exposure are 9 percentage points more likely to prefer the Chinese character when prompted with an ingroup prime relative to an outgroup prime. The estimated effect for such high exposure urban participants (0.119) is about 42 percent that of the average rural participant (0.283).

Contra the bias-reducing impacts of urban location and the reduction in contacts with one's rural kin, exposure to non-coethnics in the urban domain would appear to drive groups apart—possibly because cross-ethnic interactions are often not among individuals with equal status, geared toward common objectives, in the context of cooperative tasks, or supported or encouraged by local authorities (the conditions specified by Allport (1954) as necessary for contact to reduce social distance). A challenge with interpreting these results is that urban dwellers can select into exposure to non-coethnics: those who dislike ethnic outsiders can choose to minimize their contact with them. However, such selection would lead to an expected negative relationship between exposure and out-group bias, which is the opposite of what we find here.

From ethnic bias to *expectations* of ethnic bias

The results presented thus far offer no evidence of differential altruism toward ingroup versus outgroup members and reveal no differences in willingness to cooperate along ethnic lines among either urban or rural participants. It would be wrong, however, to interpret the absence of evidence for ethnic bias in the DG and PGG as suggesting that ethnicity is not salient in social or political life. As noted, the salience of ethnicity can also stem from the suspicion that others harbor hidden ethnic prejudices, which then induces ethnically biased behavior as a response, even from individuals who do not harbor ethnic prejudices themselves. Assessing whether interethnic relations vary across urban and rural populations thus requires moving beyond measures of differential altruism and cooperation, or even affect, as measured by the AMP, to examine measures that capture *expectations* of ethnic bias in others. To do this, I use a third economics game, the Choose-Your-Dictator (CYD) Game, which provides a measure of expectations of differential altruism in coethnic and non-coethnic interactions.[28]

In the CYD, participants are presented with information about two different people and asked to choose one of them to be "their" dictator in a DG, or to let the computer decide. They are told that the dictator they choose (or the computer chooses) will be given KSh 100 and given the opportunity to divide that sum between themselves and the participant.[29] One of the two potential dictators is presented as a coethnic and the other is presented as a non-coethnic. As in the DG and PGG, participants are shown photos of the two potential dictators and provided with basic information about their age, education, and hometown, with the hometown serving as the cue about ethnic group membership (see Figure 5).[30]

The CYD is played twice. In the first, "anonymous," version, participants select a dictator after being told that the potential dictators do not have any information about them. This version of the CYD serves as a measure of expected differential altruism across the two dictators. In the present study, it tests whether participants generally expect Kikuyu dictators to be more or less generous than Luo dictators. In the second, "profiled," version, participants select a dictator after being told that the dictators will be provided with information about the participant's own basic profile (i.e., their age, years of education, and hometown). In this profiled version of the game, the participant's choice

[28]The PGG is also commonly interpreted as a measure of expectations of others' behavior, as it is only rational to contribute money to the communal pot when one expects others to also contribute. However, in standard versions of the PGG, including the version used here, the other players have no information about the participant, and thus cannot condition their contributions on the participant's ethnicity (similar to the anonymous CYD, described below). Hence, the PGG provides little insight into expectations of ethnically biased behavior.

[29]Because the dictator whose profile is selected is not present in the lab session, the amount the participant receives is determined by the amount the dictator gave to an anonymous receiver in a DG played at the time the dictator's profile photo was collected, at the beginning of the study.

[30]As in the DG and PGG, the photos are of men pre-screened to be ethnically ambiguous.

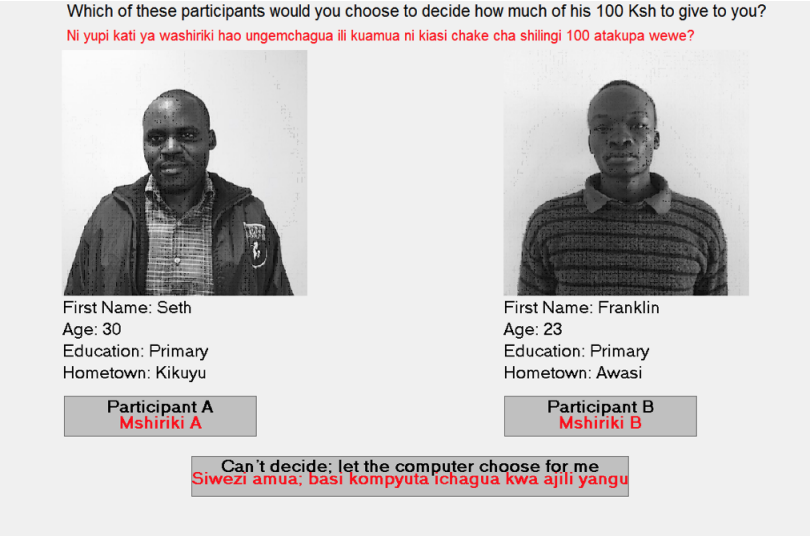


Figure 5: The Choose-Your-Dictator game

depends not just on the expected altruism of Kikuyu and Luo dictators *per se* but on the expected differential altruism of each type of dictator toward someone of their own ethnic type. This version of the CYD thus provides insight into expectations of coethnic bias by others.[31]

The CYD results are presented in Table 7, where the intercept term reflects the share of the total rounds in which participants select a coethnic to be their dictator, versus selecting a non-coethnic or letting the computer decide. Across all specifications, study participants are significantly more likely to choose a fellow group member to be their dictator. In the anonymous version, the interpretation is that participants expect that coethnic dictators will be more generous to receivers in general. In the profiled version, the interpretation is that Kikuyus expect fellow Kikuyus to be more generous to them than Luos, and Luos expect fellow Luos to be more generous to them than Kikuyus. The DG results presented earlier reveal that members of neither group are in fact more generous to receivers of their own group. But the expectation, as reflected in the choices in the profiled CYD, is that they will be. Notwithstanding this strong and consistent result, the insignificant coefficients on the *urban* dummy in columns 2-4 and 6-8 suggest that there is no difference in the strength of these expectations across urban and rural participants. While expectations of ethnic bias are present, they do not appear to weaken or intensify with exposure to urban life.

[31] Berge et al. (2020) provides a formalization.

Table 7: Choose-Your-Dictator Game Results

	Anonymous version				Profiled version			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	0.440***	0.460***	0.608***	0.857***	0.478***	0.493***	0.755***	0.979***
	(0.019)	(0.027)	(0.103)	(0.157)	(0.020)	(0.027)	(0.104)	(0.159)
Urban		-0.038	-0.033	-0.057		-0.027	-0.043	-0.075
		(0.039)	(0.041)	(0.049)		(0.039)	(0.041)	(0.050)
N	653	653	653	652	653	653	653	652
Pre-reg'd covariates			yes	yes			yes	yes
All covariates				yes				yes

Outcome is a dummy variable taking the value of 1 if the participant choose the coethnic to be their dictator rather than the non-coethnic or having the computer decide. Pre-registered covariates are gender, ethnic group, education, age, and Raven's score (demeaned). All covariates also include log monthly income (Ksh), religion, and frequency of getting news from TV, newspapers, radio, and the internet. *p<0.1; **p<0.05; ***p<0.01.

Conclusion

In this study, I have leveraged data collected among urban and rural populations in Kenya to learn about the impact of urbanization on interethnic relations, complementing prior work that has focused on urban populations alone or that has characterized interethnic relations via a narrower set of survey-based outcome measures. While I find evidence that both urban and rural participants expect outgroup members to be more generous to coethnics, I find no evidence for differential altruism (as measured via a DG) or cooperation (as measured via a PGG) across coethnics and non-coethnics, either among urban or rural participants—including when coding participants using a measure that captures the continuous nature of urban-rural connections. I do, however, find evidence of cross-group bias along a dimension of interethnic relations rooted in like/dislike, captured via a different measurement strategy, an AMP. Moreover, I find that this bias differs across urban and rural domains. Specifically, I find that the differential preference for coethnics over non-coethnics is stronger among rural participants, suggesting a potential weakening of ethnic differences due to urbanization. This finding reinforces earlier work (e.g., Robinson 2014; Kramon et al. 2022; Kim and Horowitz 2022; Berge et al. 2020) pointing to a reduction in the salience of ethnicity in urban life.

Given the challenges associated with estimating the impact of urbanization due to selection into migration, the findings reported here—like the findings in prior work—can only be suggestive of the effects of urban living on interethnic relations. Controlling for characteristics that differ across urban and rural participants goes some way toward generating valid estimates, but it cannot fully solve the selection problem. Even so, the use of multiple measures of cross-group bias and expectations of bias among members of the same politically relevant ethnic groups living in rural and urban domains

provides valuable insight into how moving from a rural to an urban area can alter how members of different communities interact with one another, with important implications for understanding development.

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Online Appendix

Urbanization and Interethnic Relations in Africa

A. Additional tables and figures.....1

B. Urban/rural connections.....4

A Additional tables and figures

Table A1: Descriptive Statistics

	Urban sample (Nairobi) (%)	Rural sample (Siaya/Kirinyaga) (%)
Gender (Male)	45.9	46.4
Education		
Some/completed primary	36.3	60.9
Some/completed secondary	47.9	33.4
Post-secondary	14.4	5.7
Speaks English fluently	67.6	44.3
Speaks Swahili fluently	31.3	37.8
Age		
Median	27	32
<= 25 years old	45	23
<= 35 years old	72	56
Monthly income (KSh)		
Median	5,000	5,750
<=1,000	20.3	5.4
<=5,000	54.1	49.5
<=10,000	74.4	79.5
<=15,000	86.5	90.1
Religion		
Catholic	33.2	30.8
Anglican	16.3	37.3
Pentecostal	22.5	7.3
Media		
Gets news from TV daily	51.6	26.4
Never gets news from TV	13.8	43.3
Gets news from newspapers daily	9.9	2.3
Never gets news from newspapers	43.4	78.8
Gets news from radio daily	65.4	82.9
Never gets news from radio	9.3	2.3
Gets news from internet daily	37.2	8.3
Never gets news from internet	44.2	82.1
Kikuyu-Luo relations		
Intermarriage, as a % of total marriages	19.2	1.8
No personal contacts with other group in past month	10.4	75.1
<= 5 personal contacts with other group in past month	28.7	94
Trust members of other group		
Not at all	12.7	29
A very great deal	19.2	4.9
Find members of other group		
Not at all peace-loving	21.8	39.4
Very peace-loving	35.6	15.5
People in neighborhood are from many different groups	98.9	n/a

Table A2: Dictator Game Results

	Mean offer	Median offer	Rate of positive offers	Average positive offer
Urban ($N = 320$)				
DG, coethnic	32.6	30	0.82	39.8
DG, non-coethnic	33.7	30	0.85	39.5
Difference[a]	-1.14	0	-0.03	0.26
Rural ($N = 335$)				
DG, coethnic	31.1	30	0.78	40.0
DG, non-coethnic	31.9	30	0.76	42.1
Difference[a]	-0.76	0	0.02	-2.10*
Urban-rural difference[b]	-0.39	0	-0.06*	2.36

[a] coethnic − non-coethnic
[b] (coethnic − non-coethnic)$_{urban}$ − (coethnic − non-coethnic)$_{rural}$
*p<0.1; **p<0.05; ***p<0.01.

Table A3: Public Goods Game Results

	Mean contribution	Median contribution	Rate of positive contributions	Average positive contribution
Urban ($N = 320$)				
PGG, coethnic group	45.2	40	0.85	53.3
PGG, non-coethnic group	44.8	45	0.85	52.5
Difference[a]	0.35	-5	-0.01	0.80
Rural ($N = 335$)				
PGG, coethnic group	47.9	50	0.83	57.9
PGG, non-coethnic group	48.9	50	0.82	59.6
Difference[a]	-1.07	0	0.01	-1.72
Urban-rural difference[b]	1.42	-5	-0.01	2.53

[a] coethnic group − non-coethnic group
[b] (coethnic group − non-coethnic group)$_{urban}$ − (coethnic group − non-coethnic group)$_{rural}$
*p<0.1; **p<0.05; ***p<0.01.

Table A4: Dictator and Public Goods Game Results, Within Subjects Design

	Dictator Game			Public Goods Game		
	(1)	(2)	(3)	(4)	(5)	(6)
Coethnic	-0.009	-0.008	-0.009	-0.004	-0.011	-0.11
	(0.008)	(0.012)	(0.012)	(0.010)	(0.014)	(0.014)
Coethnic * Urban		-0.003	-0.000		0.015	0.016
		(0.016)	(0.016)		(0.020)	(0.020)
N	1306	1306	1306	1306	1306	1306
Participant FEs	yes	yes	yes	yes	yes	yes
Round FEs			yes			yes

OLS. Outcome is the share of the endowment given to the other player (DG, columns 1-3) or contributed to the group fund (PGG, columns 4-6). Participant fixed effects absorb the *urban* main effect and all other time-invariant participant characteristics. Standard errors are clustered by participant. *p<0.1; **p<0.05; ***p<0.01.

Table A5: AMP Results

	Mean ethnic effect	SE	z-score	p-value
Urban ($N = 128$)	0.099	0.023	4.26	0.000
Rural ($N = 249$)	0.132	0.017	7.84	0.000
Urban-rural difference	-0.033			

Outcome is the "ethnic effect," the estimated probability of finding a Chinese character pleasant after exposure to a coethnic prime minus the estimated probability of finding the Chinese character pleasant after exposure to a non-coethnic prime. As pre-registered, results of the AMP-Town and AMP-Region are combined. Sample is limited to participants who correctly answered half or more of the questions in the six question literacy test administered at the start of the lab session and whose reaction times were $\leq$ 200ms, as pre-registered.
*p<0.1; **p<0.05; ***p<0.01.

Table A6: Choose-Your-Dictator Game Results

	% choosing coethnic	% choosing non-coethnic	% letting computer decide
Urban ($N = 320$)			
CYD, non-profiled	0.42	0.38	0.20
CYD, profiled	0.46	0.35	0.19
Rural ($N = 335$)			
CYD, non-profiled	0.46	0.36	0.18
CYD, profiled	0.49	0.34	0.17
Urban-rural difference			
CYD, non-profiled	-0.04	0.02	0.02
CYD, profiled	-0.03	0.01	0.02

*p<0.1; **p<0.05; ***p<0.01.

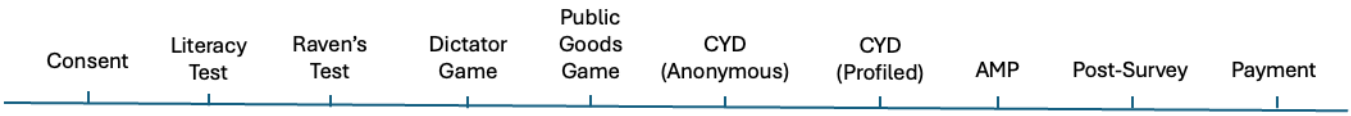


Figure A1: Timeline of the Lab Sessions

B Urban/rural connections

To investigate heterogeneity in within the urban and rural samples, I created three averaged z-score indexes:

- an index of ***ruralness***, capturing the extent to which rural residents have experience living in urban areas and/or have connections with urban kin
- an index of ***urbanness***, capturing the extent to which urban residents have maintained connections to their rural homes
- an index of ***exposure***, capturing the extent to which urban residents have contact with members of the ethnic outgroup

Tables B1, B2, and B3 provide the descriptive statistics for the components of each index. Components are reverse-coded where appropriate to ensure that all measures point in the direction of weaker urban connection in the "ruralness" index and weaker rural connection in the "urbanness" index.

Table B1: Components of the "Ruralness" Index

	%
Has family members living in Nairobi/other urban area	84
Were visited by these urban family members during past 2 yrs	
Never	13
Once	23
2-3 times	35
>3 times	29
Visited their urban family members during past 2 yrs	
Never	49
Once	20
2-3 times	19
>3 times	12
Communicate with urban family members	
Every day	5
At least once a week	45
At least once a month	78
Urban family members send them money	56
Have ever lived in Nairobi/other urban area	53
(Among those who did) for $\leq$ 1 yr	33
(Among those who did) for $\leq$ 5 yrs	79

Table B2: Components of the "Urbanness" Index

	%
Connection to Nairobi	
Length of residence in Nairobi	
$\leq$ 10 yrs	35
$\leq$ 20 yrs	67
Have a sense of belonging in Nairobi	96
Plan to vote in upcoming election in Nairobi	92
Connection to a rural place outside of Nairobi	
There is a specific place outside Nairobi they consider "home"	76
Length of residence in that place	
$\leq$ 5 yrs	32
$\leq$ 10 yrs	42
$\leq$ 20 yrs	82
Communicate with someone in that place	
Every day	22
$>$ once/wk	70
Visited that place in past 2 yrs	
Never	6
Once	19
2-3 times	33
$>$3 times	42
Someone from that place visited them in Nairobi in past 2 yrs	
Never	18
Once	31
2-3 times	28
$>$ 3 times	23
Send money to family members/others living in that place	
Never	29
Occasionally	57
Frequently	14
Know what is happening that place	
Almost everything	18
The most important things	41
Very little	35
Has property/investments in that place	38
Plans to live in that place permanently in future	66
After this life on earth, would like to be laid to rest	
In Nairobi	30
In their home village	53
In their partner's village	17
Being in good standing as a member of one's home village is	
Essential	8
Very important	25
Important	46
Not important	21

Table B3: Components of the "Exposure" Index (Urban Participants Only)

	%
Married to a spouse from another ethnic group*	18.9
Parents are intermarried**	6.3
Personal contact with member of other ethnic group during past month	
Never	9
1-5 times	17
6-10 times	23
11-20 times	9
$>$ 20 times	42
Ethnic diversity of neighborhood	
One group	1
Many different groups	99

*Among participants who are married. **Due to a data collection error, we do not have information about the father's ethnicity. This measure is based on whether the participant's stated ethnicity is different from their mother.