ETHNICALLY BIASED? EXPERIMENTAL EVIDENCE FROM KENYA

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Abstract
Ethnicity has been shown to shape political, social, and economic behavior in Africa, but the underlying mechanisms remain contested. We utilize lab experiments to isolate one mechanism—an individual's bias in favor of coethnics and against non-coethnics—that has been central in both theory and in the conventional wisdom about the impact of ethnicity. We employ an unusually rich research design involving a large sample of 1300 participants from Nairobi, Kenya; the collection of multiple rounds of experimental data with varying proximity to national elections; within-lab priming conditions; both standard and novel experimental measures of coethnic bias; and an implicit association test (IAT). We find very little evidence of an ethnic bias in the behavioral games, which

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runs against the common presumption of extensive coethnic bias among ordinary Africans and suggests that mechanisms other than a coethnic bias in preferences must account for the associations we see in the region between ethnicity and political, social, and economic outcomes. (JEL: D71, O15)

1. Introduction

Ethnicity has been shown to affect a range of political, social, and economic outcomes in Africa, from voting patterns (Horowitz 1985; Posner 2005; Ichino and Nathan 2013; Conroy-Krutz 2013) to trade and investment decisions (Fafchamps 2000; Robinson 2016) to workplace productivity (Hjort 2014) to public goods provision (Miguel and Gugerty 2005) to favoritism in the distribution of jobs and development resources (Franck and Rainer 2012; Burgess et al. 2015; Kramon and Posner 2016).

Although the role of ethnicity in shaping politics and policy in Africa is undisputed, the underlying mechanisms remain contested. In particular, the role played by differences in preferences across ethnic groups—either in terms of direct coethnic favoritism, antipathy toward others, or more subtle variation in tastes for certain outcomes (Alesina, Baqir, and Easterly 1999; Alesina and LaFerrara 2005; Vigdor 2002)—is controversial. Explanations based on *ethnic preferences* contrast with theories that emphasize different technologies of cooperation within groups due to superior ability to communicate or to impose social sanctions (Miguel and Gugerty 2005), different norms of behavior in within-group and cross-group interactions (Habyarimana et al. 2007), or other *institutional considerations* (e.g., Fearon 1999; Spolaore and Wacziarg 2009; Robinson 2016).

This paper assesses the degree of coethnic bias in preferences, defined as a preference for discriminating in favor of coethnics and against non-coethnics (Becker 1957; Horowitz 1985; Vigdor 2002; Hjort 2014). The investigation is carried out in Kenya, an African country with well-documented and politically salient ethnic divisions. Presidential vote shares in Kenya largely fall along ethnic lines during national elections; and in the aftermath of the 2007 contest more than one thousand people died and hundreds of thousands were displaced from politically motivated ethnic violence (Human Rights Watch 2008; Gibson and Long 2009). Moreover, there is a history of ethnic bias in policies, including large-scale distortions in public roads investment favoring the president's ethnic group (Barkan and Chege 1989; Burgess et al. 2015).

To isolate the presence of coethnic bias from other potential mechanisms, we employ lab experiments in an unusually rich research design that involves multiple rounds of experimental data and a large sample of over 1300 subjects. We measure coethnic bias using both standard experimental games (e.g., Dictator and Public-good games) and a more novel lab activity (the Choose-Your-Dictator game), which captures expectations about the biases of others. In our theoretical model, we explain how each game captures a different aspect of coethnic bias. Then, as a further test, we employ an implicit association test (IAT), which can be seen as a test of unconscious prejudice

(Amodio 2014; Pérez 2016) and thereby contrasts with the behavioral games that measure a conscious bias in preferences.

In addition to measuring the strength of coethnic bias, we explore whether, as an influential literature suggests it might (Bates 1983; Eifert et al. 2010; Hjort 2014), such bias varies with proximity to political competition. To address this issue, we draw our sample from two different lab sessions: one scheduled immediately before Kenya's 2013 general elections, and the other roughly seven months earlier. We also supplement this variation in real-world timing with within-lab priming designed to increase the situational salience of political competition and various dimensions of ethnicity.

Given the frequency with which outcomes in Africa are linked to ethnic preferences, our results are striking. Our tests in the behavioral games yield little to no evidence of meaningful coethnic bias. This lack of coethnic bias holds across multiple experimental measures and well-powered statistical tests. Figure 1 summarizes evidence from the Dictator game and the Public-good game. In both cases, the average level of contribution to coethnic and non-coethnic partners is within one percentage point: 35.6% versus 35.4% in the Dictator game (panel (a)), and 46.2% versus 46.4% in the Public-good game (panel (c)). Participants' beliefs about the contributions of others in these games are similarly unaffected by their ethnic backgrounds, at 49.3%–48.4% (panel (b)) and 53.9%–53.1% (panel (d)), respectively. Even though our sample is unusually large, none of these differences are statistically distinguishable from zero.

Although the main message is one of no coethnic bias in preferences, it is worth being clear that this result should not be taken to suggest that ethnicity plays no role in our study setting: ethnicity remains a prominent feature of contemporary life in Nairobi—as in Kenya more generally, and in many parts of Africa. This is evident from survey data collected at the end of our experiment, where the large majority of respondents reported they would support their own coethnic frontrunner in the upcoming elections.[1] Our findings simply suggest that the salience of ethnicity is likely due to mechanisms other than coethnic bias in preferences.

Indeed, notwithstanding the centrality of coethnic bias in preferences in both the scientific and broader public debates about African politics, existing studies that examine such bias in the African context present mixed results (Carlson 2015; Michelitch 2015; Dionne 2015; Hjort 2014; Marx et al. 2016).[2] The current study contributes to this emerging literature along multiple dimensions. First, we examine coethnic bias across a range of experimental measures, using both standard games (Dictator and Public-good game), and a novel Choose-Your-Dictator game. Second,

1. Eighty-four percent of ethnic Luos reported that they would support their coethnic frontrunner Raila Odinga (against only 8% of ethnic Kikuyus), whereas 65% of Kikuyus stated that they would support their coethnic frontrunner Uhuru Kenyatta (vs. just 1% of Luos).

2. In contrast to the mixed findings in the African literature, the findings in the broader body of work on social identity—largely built on studies conducted with students in university-based experimental laboratories (e.g., Tajfel and Turner 1986; Chen and Li 2009) but also including work in more natural settings (e.g., Gil-White 2004; Bernhard, Fehr, and Fischbacher 2006; Whitt and Wilson 2007)—present consistent evidence of intergroup bias.

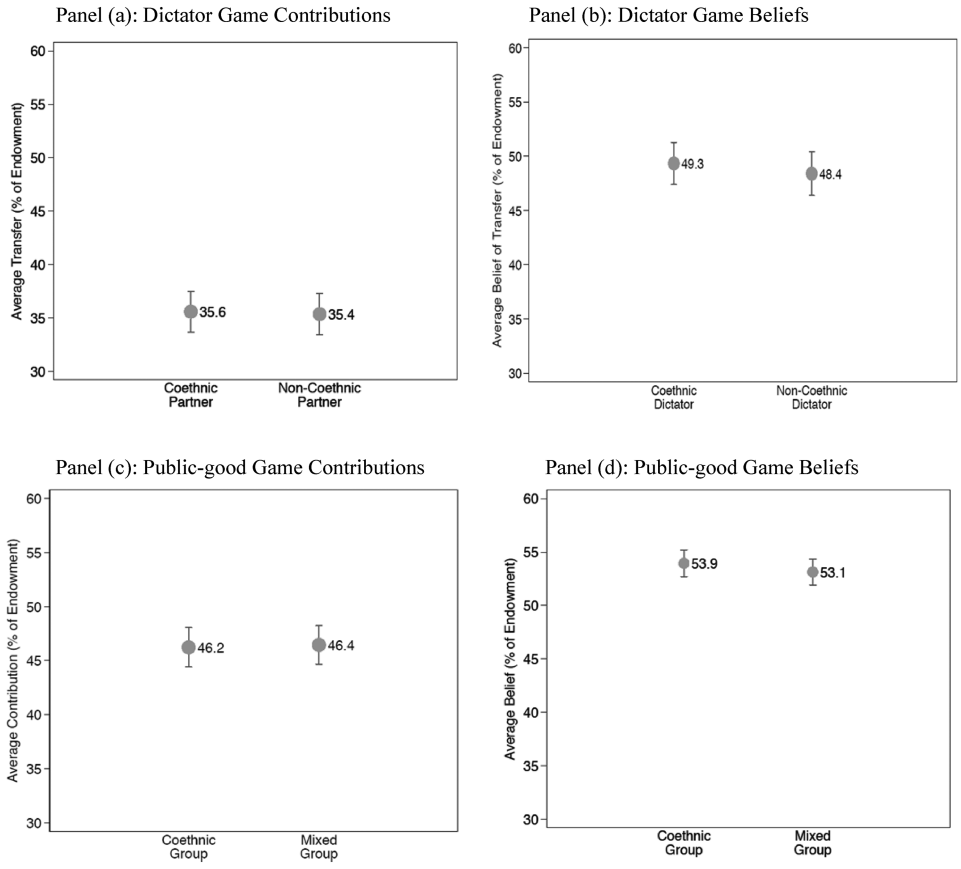

FIGURE 1. Coethnic bias in the Dictator game and Public-good game. Averages and 95% confidence intervals in the full sample for Dictator game contributions in profiled games for Coethnic versus Non-coethnic transfers (panel (a)), beliefs about Dictator game contributions from the profiled Choose-Your-Dictator game (panel (b)), for public-good game contributions in profiled games for Coethnic versus Mixed groups (panel (c)), and for public-good game beliefs about others' contributions (panel (d)). The Dictator game data in panels (a) and (b) is from the Election round (January–February 2013), the only time the complete profiled game data was collected. The Public-good game data is pooled from both the non-election round (July–August 2012) and the Election round, since the complete profiled game data was collected in both.

in previous studies on coethnic bias in preferences (e.g., Habyarimana et al. 2007) it remains unclear if results would differ if experiments had been conducted in a different setting or in closer proximity to national elections, when politicians are most likely to mobilize along ethnic cleavages and ethnicity has been shown to acquire greater salience (Eifert et al. 2010; Hjort 2014). To address this question, we amplify the situational salience of ethnicity and political competition both across lab rounds, by varying the proximity to Kenya's national election, and within the lab using priming treatments. Finally, to avoid selective presentation of results, our analysis follows a

pre-specified set of hypotheses, and we employ larger samples to ensure sufficient statistical power.[3]

Our results thus serve to crystallize and generalize the finding that coethnic bias in preferences in African societies is often less pronounced than is widely believed. This raises the question of why ethnicity often remains a divisive force in African societies. We believe a large part of the answer lies in the aforementioned institutionalized ethnic divisions, which need not correlate with individual-level coethnic bias in preferences (Habyarimana et al. 2007). Since this paper focuses exclusively on individual-level coethnic bias, we are not well positioned to further examine the role of institutions in driving ethnic divisions. Nevertheless, our results indicate two areas where individuals' coethnic bias may still play a role in shaping ethnic divisions, potentially in interplay with the broader societal context. First, there is one particular group where our robust set of null results in the behavioral games does not apply, namely among relatively recent migrants to Nairobi. These recent migrants are significantly less generous and cooperative toward non-coethnics than the longer-term residents of Nairobi. One interpretation of this result is that long-term immersion in the multi-ethnic setting of the capital leads to a reduction in ethnic bias, which would have intriguing implications for the future of increasingly urban countries like Kenya. Second, in the IAT we find a statistically significant ethnic bias, and the presence of this unconscious, implicit bias stands in contrast to the broad absence of bias in the behavioral games. Although the average implicit bias is small, our evidence suggests that this bias may be amplified in the context of political competition—a finding particularly interesting in light of the ethnic violence around Kenya's 2007 election.[4]

2. A Model of Ethnic Preferences and Behavior

In the empirical analysis, we employ lab games to measure individual-level ethnic biases. The model developed in what follows builds on Capellen et al. (2007, 2013) and describes what can be learned about coethnic bias in preferences from observing individual-level play in the Dictator and Public-good games. Specifically, it highlights the interplay between general altruism and ethnic preferences in determining behavior. In particular, the model shows how a coethnic bias in preferences does not automatically translate into a coethnic bias in behavior: this depends on the level of altruism. We present the baseline model in the context of a Dictator game; afterward, we discuss how the model relates to the Public-good game.[5] We will return to how the theoretical

3. The full results for the pre-specified analysis are available in Online Appendix D.

4. Studying the effects of exposure to violence on ethnic bias is a fascinating line of inquiry that we are unfortunately not in a position to undertake, as we did not collect information from research subjects on this issue.

5. In Cappelen et al. (2007, 2013), individuals make a trade-off between selfish and other-regarding concerns when sharing a sum of money. We extend their original model by introducing ethnic preferences so that it matches the versions of our lab games where subjects have some information about the likely ethnicity of their partners.

model maps to the Choose-Your-Dictator game after we lay out that game's design and estimation procedure in Section 3.5.

Let m_i be the individual's fairness ideal. This could be an even split of the endowment, although the exact value does not matter for measuring the degree of observed coethnic bias in behavior. We introduce a coethnic bias in preferences, given by q_i, and an ethnicity indicator variable n_j, which takes the value one if the counterpart is a non-coethnic and zero if a coethnic. The utility of an individual i is

$$u_i = (1 - t_i) - \frac{(t_i - m_i)^2}{2b_i(1 + n_j q_i)}, \tag{1}$$

where the endowment of money is given by 1, t_i the transfer to the other player, and b_i is the degree of egoism ($b_i \geq 0$), that is, $b_i = 0$ denotes perfect altruism.

The first term in equation (1) captures the utility component based on what the decision-maker keeps for himself, and the second term is the decision-maker's disutility due to deviations from the fairness norm. The disutility from deviations from this norm depends on three factors: b_i, q_i, and n_j. In the limit as $b_i \to \infty$, the second term tends to zero as egoism increases, and a fully egoistic decision-maker chooses $t_i^* = 0$. In fact, for $b_i(1 + n_j q_i) \geq m_i$ there is a corner solution, where the decision-maker keeps all the money for himself, $t_i^* = 0$. Clearly, if the individual is sufficiently egoistic, a coethnic bias in preferences does not affect behavior: irrespective of whether the counterpart is coethnic or not, he keeps all money for himself. At the other end of the spectrum, for fully altruistic individuals ($b_i = 0$), the second term receives all the weight in the decision process, and $t_i^* = m_i$. Also in this case, ethnically biased preferences do not affect behavior. The degree of coethnic bias in preferences q_i has an analogous effect when the other party is a non-coethnic, that is, for $n_j = 1$. In that case, the larger is q_i, the lower is the weight given to the fairness term, and the smaller is the transfer.

For an interior solution, the first-order condition for sharing implies:

$$t_i^* = m_i - b_i(1 + n_j q_i). \tag{2}$$

Sharing with a coethnic (CE) is then given by $t_i^{CE} = m_i - b_i$, and with a non-coethnic (NCE) by $t_i^{NCE} = m_i - b_i(1 + q_i)$. We measure the observed coethnic bias in behavior in the Dictator game as $B^D \equiv t_i^{CE} - t_i^{NCE}$, that is, how much more the decision-maker contributes when facing a coethnic instead of a non-coethnic. This implies that B^D is only a function of the degree of altruism and the degree of coethnic bias in preferences:

$$B^D = b_i\, q_i. \tag{3}$$

A first implication of the model is that a strong observed coethnic bias in behavior ($b_i q_i$) is not necessarily the result of a high coethnic bias in preferences alone (high q_i), as it may be driven primarily by egoism (high b_i). Similarly, a lack of coethnic bias in behavior does not necessarily exclude a coethnic bias in preferences. Both for a very small level of altruism (in the model, high $b_i = 0$) and a high level of altruism ($b_i \geq m_i/(1 + n_j q_i)$) q_i does not affect b_i.

We now extend this analysis to the Public-good game, where we employ an analogous utility structure for respondent i when making a contribution t_i to the group fund. In the context of the Public-good game, an individual's fairness norm is allowed to be group-dependent: $m_i^g \equiv f(E_{g,-i}[t])$, where $E_{g,-i}[t]$ is individual i's expectation about other group-member contributions to the group fund of group g, and we assume $f' \geq 0$. The expression for an interior solution becomes

$$\hat{t}_i^* = m_i^g - b_i\left(1 + n_j q_i\right).$$

After applying an analogous derivation as before, the observed ethnic bias in the public good game, $B^{PG} \equiv \hat{t}_i^{CE} - \hat{t}_i^{NCE}$, becomes

$$B^{PG} = b_i\, q_i + m_i^{CE} - m_i^{NCE}.$$

The difference in contributions in a coethnic versus non-coethnic group reflects both a coethnic bias in preferences (the first term, as given in the previous equation) and in expectations about others' contributions. Critically, if other group members are expected to contribute less in a non-coethnic setting compared to a coethnic setting, this should amplify the observed coethnic bias in terms of Public-good game contributions.

3. Setting, Experimental Protocol, and Research Design

3.1. Setting, Sample, and Order of Lab Activities

The study sample draws from two ethnically diverse low-income neighborhoods in Nairobi, Kenya: Kibera, which was a focal point for the 2007–2008 post-election violence, and Viwandani. The experiments were carried out at the Busara Center for Behavioral Economics, which also oversaw the recruitment of subjects. Recruitment was limited to members of five of the six largest ethnic groups in Kenya (the Kikuyu, Luo, Luhya, Kamba, and Kisii, listed in order of size in both the Kenyan population and our study sample). Taken together, these five groups make up 82% of the Nairobi population and approximately 60% of the population countrywide.[6] They also contain the two groups with the deepest historical rivalry, Kikuyu and Luo. The only major Kenyan ethnic group not represented in the sample is the Kalenjin, whose members do not reside in large numbers in either of the two recruitment neighborhoods. All our respondents were first-time participants in a lab study at the Busara Center.

There were two data collection rounds in our study, each with a different set of participants recruited using an equivalent sampling protocol. We call the first round, which took place from July to August 2012 (7–8 months prior to the March 2013 presidential and parliamentary election), the *Non-election round* and the second round, which took place from January to February 2013 (1–2 months prior to the election),

6. Figures for Nairobi are from the 1999 Kenyan census; national figures are calculated from 2009 Kenyan census data from the Kenya National Bureau of Statistics.

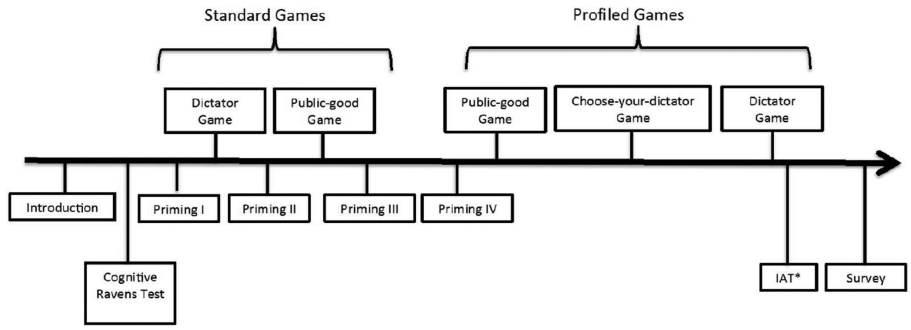

FIGURE 2. Experimental laboratory game structure. *Indicates that we only have this data for the Election Period.

the *Election round*. The purpose of dividing the lab sessions in this way was to test the expectation, documented by Eifert et al. (2010), that ethnic identities become more salient, and thus that coethnic bias might be more pronounced, during moments of heightened political competition. The macroeconomic environment, as captured in major commodity prices during 2011–2013 (see Online Appendix Figure A.1), was relatively stable during the study period, so we can rule out that any differences we observe across the lab rounds are due to changes in the country's economic conditions. The political climate, by contrast, was markedly different. Whereas just 25% of participants in the Non-election round reported having recently attended a campaign rally and 14% reporting having received cash from a candidate, this more than doubled to 61% and 31%, respectively, among participants in the Election round. The total sample size is 1362 distinct individuals, with 608 individuals in the Non-election round and 754 in the Election round (see Online Appendix Table A.4 for a comparison of the two subject pools).

Two lab sessions were held each day, each lasting 2–3 hours and including up to 20 participants. Participants were reimbursed for transport and given a "show-up fee", in addition to their payouts from the games played. The results of each game were not revealed until after the session, at which point payments were made by mobile money transfer. Further details of the experimental lab protocol are contained in Online Appendix B.

Figure 2 illustrates the structure of the lab activities. Each lab session began with the administration of a 16-piece Raven Progressive Matrices test to measure individual cognitive ability (Raven 2008)—a potentially important factor in laboratory games. We use the normalized scores from this test both to confirm balance across treatment groups and as a control in our analyses.

Each lab session was then divided into two parts, both chronologically and conceptually. In the first, "standard", part participants were randomly paired with individuals about whom they were provided no information. These partners were individuals who had played in pilot lab rounds and were randomly chosen to receive payouts based on the decisions they made in those games. Although participants had

no way of knowing the specific ethnicities of the other players they were paired with, they knew that the other players were recruited from Nairobi's multiethnic population and, hence, that they were unlikely to be coethnics (even members of the largest ethnic group in our sample—the Kikuyu, who comprise 30.9% of Nairobi residents according to the most recent available Kenyan census data—would have a lower than one-third chance of being paired with a coethnic). Patterns of play in these "standard" games can thus be interpreted as reflecting how participants behave when interacting with non-coethnics.[7]

In the second, "profiled", part of the lab, participants were paired with individuals about whom they received three pieces of background information: their education, their age, and their home town.[8] Home towns are strong indicators of ethnic affiliation in Kenya because of the high correspondence between administrative and ethnic boundaries (see Online Appendix A, Table A.5). Piloting prior to the study confirmed that the vast majority of participants could correctly identify the intended ethnic backgrounds of their partners based on the home towns ascribed to them (see Online Appendix B, pp. 46–47). The inclusion of information about the partner's age and education was meant to obscure the ethnic focus of the study and to minimize experimenter demand effects. The order of the three pieces of information (home town, school completion, and birth year) was varied across subjects so as to avoid bias stemming from order effects.

The profiles of the players with whom participants were paired were randomly assigned. As in the standard games, the profiles were drawn from among the individuals who participated in the pilot sessions, only this time we limited the sample to those whose home towns provided unambiguous clues about their ethnicity. To ensure a sufficiently large number of coethnic matches, we further limited the sample to individuals from the two largest ethnic groups, Kikuyus and Luos, groups whose high political relevance insures that many of the non-coethnic pairings in our sample were between members of groups whose relations have been historically fraught.

As pre-specified in our pre-analysis plan (see Online Appendix C), and in accordance with the ethnic political coalitions that emerged in advance of the 2013 national election, we categorize Luos together with Luhyas and Kisiis (two other western Kenyan groups long in coalition with Luos in national politics) as coethnics in the analysis, as distinct from the larger Kikuyu ethnic group. To be conservative, we pre-specified that the Kamba would be excluded from the main analysis due to the shifting political alliances of their leaders during 2012 and 2013 (but the results do not differ if they are included and grouped with the Kikuyu (see Online Appendix D, part 2), the group that is culturally and geographically closest to them). The main

7. For a similar design, see Ferraro and Cummings (2007).

8. There were 12 profiles in total, with 6 Kikuyu profiles and 6 Luo profiles. The home towns for Kikuyu participant profiles are: Gatanga, Nyeri (2 profiles), Murang'a, Kiambu, Mwea. The home towns for Luo participant profiles are: Kisumu (2 profiles), Siaya (2 profiles), Nyakach, and Homa Bay.

empirical results are also unchanged if we ignore allied ethnic groups and restrict the sample to just Kikuyu and Luo subjects alone.[9]

Each lab session consisted of three main games: the Dictator Game (DG), the Public-good game (PG), and the Choose-Your-Dictator (CYD) game. Within the profiled parts of the DG and PG, the order of the coethnic and non-coethnic games was randomized. The CYD, by contrast, was always played with at least some partner information, although there are two information variants to this game, described in what follows. At the end of the Election round sessions, subjects were also administered an IAT and then asked survey questions to capture self-reported political preferences and attitudes.

3.2. Priming Treatments

Participants were exposed during the lab sessions to a randomly assigned priming treatment designed to increase the situational salience of (1) political competition, (2) ethnic–cultural differences, (3) ethnic–political differences, or (4) national identity. There was also a control group in which participants were exposed to a neutral prime.

The political competition prime was designed to serve as an experimental counterpart to the real world variation in proximity to elections afforded by our two lab rounds. As with the comparison of the lab rounds occurring 7–8 months and 1–2 months before the 2013 elections, the expectation was that priming participants to political competition would increase coethnic bias in the behavioral games. The rationale for the ethnic primes, which were meant to mimic the daily exposure that individuals have to appeals to shared ethnic culture (the ethnic–cultural prime) or to blatant tribal politics (the ethnic–political prime), was even more straightforward. By priming subjects to think about ethnicity, the expectation was that ethnic differences between players in the games would become more salient, resulting in higher levels of in-group bias (Bowles and Gintis 2004; Shayo 2009). Finally, the purpose of the national identity prime was to explore whether ethnically motivated behavior would be reduced by invoking a common superordinate identity (Kramer and Brewer 1984; Gaertner and Dovidio 2000; Charnysh, Lucas, and Singh 2015).

To reduce the likelihood that priming effects would dissipate, priming was implemented at four distinct points during the lab session (see Figure 2). The primes were administered between each set of games as short, three question on-screen quizzes. For example, one of the political competition priming questions asked: "*How many political candidates are running for the Presidency?*" One of the ethnic–cultural priming questions highlighted the country's ethnolinguistic diversity: "*This greeting comes from which region: 'Orie'*?" Questions specific to the priming conditions were intermixed with neutral questions, mainly focused on Kenyan popular culture

9. The fact that the results are unchanged when we restrict the sample to Kikuyu and Luo subjects also speaks to the potential concern about the weakness of the home town primes as cues to the coethnicity of the other player(s). Since the home towns we employ are all from Kikuyu and Luo areas, they are likely to provide the strongest cues to Kikuyu and Luo subjects (as confirmed in Online Appendix A, Table A.5).

or everyday life in Nairobi, for example: "*How often do you ride a matatu (mini-bus) every week?*" Each quiz included two priming questions and one neutral question. Those in the control group were asked only neutral questions. Online Appendix B provides the full set of priming questions. Exit interviews and focus group discussions confirmed that the priming questions were effective in eliciting concepts of political competition, ethnic–cultural differences, political–cultural differences, and national identity.

3.3. Dictator Game

The Dictator game aims to capture an individual's altruism toward others. Participants were given an endowment of 50 Ksh ($0.60)—equivalent to roughly an hour's wage for many Nairobi workers—and were asked to decide how much to give away to another player (the "receiver") with whom they were randomly paired. Subjects played both the standard and profiled version of the game, with the latter played twice: once with a coethnic receiver and once with a non-coethnic receiver (with the order randomized). This allows us to compare the level of altruism toward coethnics versus non-coethnics.

The standard Dictator game was played in both the Election and Non-election rounds, but due to a programming error in the lab software, only the coethnic version of the profiled game was played in the Non-election round. This means that we can estimate the extent of coethnic bias in the Dictator game only in the Election round. (There was no such coding error for the Public-good game or the Choose-Your-Dictator game, so for those games we are able to measure coethnic bias in both the Non-election and Election rounds, as described in what follows.)

The outcome of interest is the percentage of the endowment transferred to the receiver. We first focus on how receiver coethnicity affects transfers, and how coethnicity interacts with election proximity, and then estimate the effect of the priming treatments.

The first specification includes data from both the standard and profiled games:

$$Y_{ij} = \alpha + \beta_1 Coethnic_{ij} + \beta_2 Election_i + \beta_3 Coethnic_{ij} \times Election_i + \beta_4 Noncoethnic_{ij} \times Election_i + \varepsilon_{ij}, \quad (4)$$

Y_{ij} is the transfer (in percent of the endowment) by subject i to their partner in game j, $Coethnic_{ij}$ ($Noncoethnic_{ij}$) is an indicator variable for being paired with a coethnic (non-coethnic) partner in game j, $Election_i$ indicates whether respondent i is observed in the Election round, and ε_{ij} is the error term, clustered by individual. The lack of a non-coethnic profiled Dictator game in the Non-election round explains the missing non-interacted $Noncoethnic_{ij}$ term. The estimate of coethnic bias in behavior (B^D) in the Election round is given by the difference in coefficient estimates on the coethnic partner and non-coethnic terms, and the formal hypothesis test is that $(\beta_1 + \beta_3) = \beta_4$. The Election round effect captured in β_2 is potentially of independent interest. As a robustness check, we also estimate a specification with covariates including ethnicity and gender, years of education, and the normalized Raven's test score.

We estimate effects of the priming treatments in two slightly different ways. In the main analysis that we report in the text, we focus on the standard Dictator game in order to provide the cleanest possible test of priming effects. Since participants could reasonably infer that anyone they were paired with was likely to be a non-coethnic, the results of the standard Dictator game provide insight into the effects of priming on levels of altruism vis-a-vis non-coethnics. We estimate the following equation:

$$Y_{ij} = \alpha + \beta_1 Election_i + \sum_{k=1}^{3} \beta_{1+k} T_k + \sum_{k=1}^{4} \beta_{4+k} (T_k \times Election_i) + \varepsilon_{ij}. \quad (5)$$

The terms T_1, T_2, T_3, and T_4 are indicator variables for the four priming conditions, namely, the political–competition prime, the ethnic–cultural prime, the national prime, and the ethnic–political prime, respectively. β_1 estimates the difference in the average level of transfers across the Election and Non-election rounds, β_2, β_3, and β_4, the average effects of the first three priming treatments in the Non-election round, and β_5, β_6, and β_7 the differential effect of these primes in the Election round. β_8 is the effect of the ethnic–political prime, which was only administered in the Election round, and thus is only included in interaction with the Election round indicator.

We also analyze the effects of priming using the profiled Dictator game (see Online Appendix A, Table A.1). This analysis provides an arguably more direct test of the impact of priming on coethnic bias. However, it is theoretically ambiguous if the impact of priming will be stronger or weaker in the standard or the profiled version of the game (see Benjamin et al. 2010). After all, the goal of the primes is to increase the situational salience of ethnicity, and this situational salience is already amplified in the profiled games. Hence we put more weight on the priming analyses that employ the non-profiled games. In any case, results are similar using both approaches.

3.4. Public-Good Game

Participants in the Public-good game were given an endowment of 60 Ksh ($0.70) and could choose how much to keep for themselves versus how much to contribute to a group fund in which contributions were multiplied by two before being equally shared among the three players in the game. The contribution level thus captures an individual's willingness to share resources to make others better off, and resembles the classical prisoner's dilemma. Compared to the Dictator game, the Public-good game's framing in terms of a "group fund" is distinct, and the recognition that other actors are also making decisions could trigger reciprocity that might affect behavior.

Before deciding how much to contribute themselves, subjects stated how much they believed each of the other players would contribute. These were non-incentivized elicitations. Subjects received no information about the two other players in the standard part of the session, where once again the other players were randomly drawn from subjects in the pilot rounds. In the second, profiled part of the lab, they were informed about each of the two other players' years of education, age and home town,

just as in the profiled Dictator game. There were two types of profiled Public-good games. The first was a "mixed" group, with one coethnic player and one non-coethnic player, and the second, the "coethnic" group, in which both players were coethnics with the subject. The order in which these were played was randomized. The difference in individual contributions to the group fund across the coethnic group and the mixed group is our measure of coethnic bias. In the pre-analysis plan, we pre-specified both a focus on contributions and an analysis of "contributions minus beliefs", a measure of conscious free-riding on the part of the subject proposed in Fischbacher and Gächter (2010). For simplicity here, we focus on contributions alone but results are very similar for the contributions minus beliefs outcomes (see Online Appendix D).

The main econometric specification follows the Dictator game, where $Mixed_{ij}$ is an indicator for an ethnically mixed group, $Coethnic_{ij}$ is a homogeneous coethnic group, and Y_{ij} is the contribution of individual i in game j, and all games (standard and profiled) are pooled in the analysis:

$$Y_{ij} = \alpha + \beta_1 Coethnic_{ij} + \beta_2 Mixed_{ij} + \beta_3 Election_i + \beta_4 Coethnic_{ij} \times Election_i + \beta_5 Mixed_{ij} \times Election_i + \varepsilon_{ij}. \quad (6)$$

We estimate coethnic bias in the Public-good game (B^{PG}) in the Non-election round in an F-test of the hypothesis that $\beta_1 = \beta_2$. We also assess if there is a differential degree of coethnic bias in the Election round by testing if $\beta_4 = \beta_5$. As in the Dictator game, we also estimate the effects of the priming interventions in the standard Public-good game (see Online Appendix D).

3.5. *Choose-Your-Dictator Game*

The Choose-Your-Dictator game was designed to capture expectations of differential altruism in coethnic and non-coethnic interactions by measuring whether participants discriminate along ethnic lines in their choice of a "leader" responsible for allocating resources, a conception thus broadly linked with the process of electing public officials.[10]

After having already played the Dictator game and become familiar with its rules, participants ("choosers") were presented with two randomly drawn profiles of individuals from our pilot rounds and were asked to choose one to play the role of dictator in a Dictator game. One of the two drawn profiles was a coethnic, the other a non-coethnic. As in the profiled Dictator and Public-good games, choosers were provided with basic information about both of the potential dictators' years of education, age, and home town. Prior to choosing "their" dictator, we also elicited the

10. In a novel study on homophily in social networks, Adida et al. (2015) develop a similar game. Their experimental protocol differs, however, as the participants go through a face-to face speed-chatting process prior to voting for a group "leader". Although this setup may benefit from the realistic features of the experimental protocol, it also implies that the choice of "leader" could be affected by personal characteristics out of the researchers' control. Hence, the impact of ethnicity may be more difficult to discern.

chooser's beliefs about how much each of the potential dictators they were choosing between would allocate to them in the game (this is the source of the Dictator game beliefs data presented in Figure 1, panel (b)). The participant's payoff in the game was the amount the selected dictator actually transferred when he or she played the game in the pilot rounds.

The Choose-Your-Dictator game was played twice. In the first, "standard" version, choosers select a dictator knowing that the potential dictators do not have any information about them, that is, the payoff will be from the standard Dictator game played by these dictators. This choice depends only on expected differences in average generosity between the two potential dictators, but not on their differential altruism toward a person of the chooser's type. In the second, "profiled" version, the choosers select a dictator knowing that the dictator will be provided with the choosers' own basic information profile (i.e., their years of education, age, and home town). The payoff now is from the chosen dictator's play in the profiled Dictator game played in the pilot rounds when the dictator was paired with a receiver of the chooser's ethnic group. In this second version of the game, where the dictator has information about the ethnicity of the recipient, the chooser's selection depends on both expected differences in altruism by the two dictators generally and toward a coethnic. The difference in play across the "standard" and "profiled" versions of the Choose-Your-Dictator game thus allows us to isolate the chooser's expectations of coethnic bias.

To understand the extent of participant preferences for a coethnic dictator, we implement an ordered logit specification. The dependent variable is Y_{ip}, which takes on the following values for dictator profiles p in the choice set of chooser i: $Y_{ip} = 0$ if profile p is not chosen by the participant, $= 1$ if the participant is indifferent between profile p and the other profile in her choice set, and $= 2$ if the participant chooses profile p. The "indifferent" option was included after we piloted the game and observed that large shares of participants claimed that they were unable to choose between potential dictators based on the basic profile information alone—an early hint about the low levels of differential altruism across ethnic lines in this population.

We specify the probabilities for observing each outcome value of Y_{ip} as a function of V_{ip}, which can be interpreted as participant i's latent utility for profile p (Woolridge 2001). Using this framework, we carry out maximum likelihood estimation. The main specification estimates the difference between the valuation placed on a coethnic versus a non-coethnic profile across the "standard" and the "profiled" versions of the game:

$$V_{ip} = \alpha_p + \beta_1 Coethnic_{ip} + \beta_2 Coethnic_{ip} \times Profiled_{ip} + \beta_3 Coethnic_{ip} \times Election_i + \beta_4 Coethnic_{ip} \times Profiled_{ip} \times Election_i + \varepsilon_{ip}. \quad (7)$$

Here α_p is the set of profile fixed effects, $Coethnic_{ip}$ is an indicator variable for profile p being coethnic of the chooser, and $Profiled_{ip}$ indicates whether the dictator was playing the profiled version of the Dictator game, and thus had information about the receiver's home town. In the Non-election round, β_1 estimates the degree of coethnic bias in the standard Choose-Your-Dictator game (equivalent to the generosity term b_i

TABLE 1. Descriptive statistics on the sample and average game play.

	Dictator game transfer (percent of endowment)		Public-good game contribution (percent of endowment)		Choose-Your-Dictator game (dictator choice)	
	Coethnic	Non-coethnic	Coethnic group	Mixed group	Coethnic	Non-coethnic
Demographics						
Full sample	35.6	35.4	46.2	46.4	31.7	25.2
Female (60% of sample)	36.8	35.8	44.9	46.2	27.3	24.3
Male (40% of sample)	33.4	34.6	48.0	46.8	38.0	26.4
Age (mean = 33.0 years)						
Below median	33.3	32.1	41.2	43.4	32.9	27.2
Median or above	37.8	38.5	48.0	47.5	30.5	23.2
Education (mean = 9.6 years)						
Below median	36.5	35.4	46.9	46.2	27.6	26.4
Median or above	34.4	35.4	45.6	46.6	35.0	24.3
Ravens score (mean = 0.0)						
Below median	37.5	37.5	47.3	48.7	28.5	25.2
Median or above	33.2	32.8	45.2	44.3	34.7	25.2
Ethnic affiliation						
Kikuyu (32% of sample)	36.3	36.1	45.5	47.4	26.7	25.7
Luo (21% of sample)	33.4	34.5	42.7	43.6	37.2	23.8
Observations	593	593	1080	1095	1095	

Notes: Pooled data from the Non-election round (July–August 2012) and Election round (January–February 2013). Values are averages, presented in percent terms. For the Dictator game, only the Election round averages are displayed, as there is incomplete information for the profiled version of the Dictator game in the Non-election round. For the Choose-Your-Dictator game, the table presents results for the profiled version of the game. Respondents with Kamba ethnicity are excluded from analysis, as pre-specified in the pre-analysis plan.

in our model), and β_2 is the additional degree of coethnic bias in the profiled Choose-Your-Dictator game, which captures the degree of coethnic bias that the chooser expects from the dictator given that the dictator knows that the receiver is a coethnic (equivalent to their expectation of the q_i coethnic preference term in the model). Thus, the estimate of β_2 is of central interest. The β_3 and β_4 terms capture any additional coethnic bias in the Election round. The error-term ε_{ip} has a Type I Extreme-value distribution. As a robustness check, we also condition on subject covariates, in this case interacted with the $Coethnic_{ip}$ term (since explanatory variables that do not vary across choice options cannot be estimated in a logit model).

4. Results

4.1. Descriptive Statistics

Descriptive statistics for the lab participants are presented in Table 1 (left column). Females are slightly more than half the sample (at 60%). Respondents are 33 years old

on average, and they have completed an average of 9.6 years of education (a schooling level that is typical for young Kenyan adults in urban areas but higher than national averages). Twenty-nine percent of the sample report having a continuous source of wage income, 13% report self-employment, and 30% are unemployed (not shown). More than half of participants belong to the two largest ethnic groups in our sample, with 32% Kikuyu and 21% Luo. We present evidence in Online Appendix D that the randomization across the various priming interventions created treatment groups with similar observable characteristics along these dimensions.

4.2. Estimating Coethnic Bias

As noted in the introduction, there is no evidence for coethnic bias in either the Dictator game or the Public-good game. Reproducing the result from Figure 1, the top row of Table 1 presents average transfers in both games in the full sample. For both games, the differences in average transfers are small (35.6% vs. 35.4% in the Dictator game and 46.2% vs. 46.4% in the Public-good game for Coethnic versus Non-coethnic transfers, respectively) and not statistically significant.

Given the large number of hypotheses we investigate, an important concern is the over-rejection of null hypotheses (i.e., "false positives") due to the problem of multiple inference (Anderson 2008). To mitigate this risk, we present two types of p-values in our analyses: first the regular or "per-comparison" p-value, which is appropriate when testing a single hypothesis, and second, the family-wise error rate (FWER)-adjusted p-value, which captures the likelihood that at least one true null hypothesis is falsely rejected within a given set of hypotheses, using the free step-down resampling method described in Westfall and Young (1993).

As explained previously, the lack of a Non-coethnic arm in the Non-election round lab implies that the relevant test for differential altruism in the Dictator game is the sum of the coefficient estimates on the Coethnic terms in both rounds minus the coefficient estimate on the Non-coethnic term in the Election round. The average coefficient on bias is just 0.6 percentage points and not statistically significant (Table 2, columns (1) and (2), p-value $= 0.54$).[11] Our large sample size implies that these are quite precisely estimated zeros: the 95% confidence interval on the overall Coethnic effect in the Election round ranges from –1.3 to +2.5 percentage points. For those in the priming control group (i.e., who received no priming to ethnic identity, political competition, or national identity), the effect is slightly larger in magnitude but not significantly different from zero (column (3), p-value $= 0.31$). In line with these findings, beliefs about the generosity of a coethnic Dictator are nearly identical to beliefs about non-coethnic Dictators and not significantly different from zero (Figure 1, panel (b), p-value $= 0.51$).

11. This number differs from the difference for the full sample in Table 1, since it is estimated after excluding the ethnic–political priming group from the sample, which was not administered in the Non-election round.

TABLE 2. Dictator game transfers, in standard and profiled games.

	Full sample		No prime	FWER p-value
	(1)	(2)	(3)	(4)
Coethnic recipient	−1.51	−1.44	−1.85	0.518
	(1.00)	(1.00)	(1.80)	
Election round	−5.21***	−5.98***	−6.52***	0.008
	(1.30)	(1.38)	(2.50)	
Election round × Coethnic recipient	0.70	0.63	−0.83	0.786
	(1.48)	(1.48)	(3.02)	
Election round × Non-coethnic recipient	−1.41	−1.42	−4.54*	0.153
	(1.09)	(1.09)	(2.35)	
Covariates	No	Yes	No	
Observations	2881	2881	748	
Recipient: Coethnic + Election round × (Coethnic − Non-coethnic)	0.60	0.60	1.86	
	(0.98)	(0.98)	(1.85)	

Notes: The dependent variable is the transfer in the Dictator game (in percent of the endowment). Data are pooled from the Non-election and Election rounds. Standard errors in parentheses are clustered at the individual level. Covariates include ethnicity indicators, a gender indicator, education controls, and the Raven's test score. FWER p-values are simulated as described in the pre-analysis plan for column (3). There was no Non-coethnic profile in the Dictator game during the Non-election round, hence the absence of a direct "Non-Coethnic Recipient" term. The F-test in the bottom row tests the hypothesis that the average level of coethnic bias in the Election round was zero. All specifications exclude ethnic Kamba subjects, as specified in the pre-analysis plan. The specifications for the full sample exclude the ethnic–political priming group, which was only administered in the Election Round. $^{*}p < 0.10$; $^{***}p < 0.01$.

In the Public-good game, average coethnic bias in the Non-election round is approximately 1.57 percentage points, but this difference is not statistically significant (Table 3, columns (1) and (2), p-value = 0.16). This null finding is somewhat less precisely estimated than in the Dictator game, with the 95% confidence interval ranging from –0.63 to +3.76 percentage points. In the Election round, there is actually a significant reduction in coethnic bias for the Public-good game, with an average effect of −3.67 percentage points (p-value = 0.027), which runs counter to the existing evidence that ethnic identity and preferences might become more salient closer to elections (Eifert et al. 2010). Taken together, the overall degree of coethnic bias in the Election round is the sum of these two effects, and thus is small and negative (roughly −2 percentage points) and not statistically significant. In the priming control group, there are no significant coethnic bias effects in either lab round (column (3)). Subjects also believe groups members will be equally generous in coethnic and ethnically mixed groups (Figure 1, panel (c)). These results are unchanged when the dependent variable is "contributions minus beliefs" (see Online Appendix D).

Even if overall coethnic bias is close to zero, a natural question is whether there might be heterogeneity in the degree of this bias across subgroups of our sample. This is illustrated in Table 1. The striking pattern that emerges is how little variation there is along the various dimensions that we pre-specified in the pre-analysis plan. For instance, across both gender groups, the difference in average transfers in the

TABLE 3. Public-good game contributions, in standard and profiled games.

	Full sample		No prime	FWER p-value
	(1)	(2)	(3)	(4)
Coethnic group	2.22*	2.22*	−1.73	0.984
	(1.33)	(1.33)	(2.54)	
Mixed group	0.65	0.65	0.32	0.988
	(1.18)	(1.18)	(2.29)	
Election round	−2.97*	−3.95**	−6.03*	0.321
	(1.76)	(1.81)	(3.37)	
Election round × Coethnic group	−1.67	−1.68	2.00	0.984
	(1.89)	(1.89)	(3.39)	
Election round × Mixed group	2.00	2.00	2.30	0.984
	(1.85)	(1.85)	(3.59)	
Covariates	No	Yes	No	
Observations	2939	2939	763	
Coethnic group – Mixed group	1.57	1.57	−2.05	
	(1.12)	(1.12)	(2.21)	
Election round × (Coethnic	−3.67	−3.67	−0.30	
group – Mixed group)	(1.65)	(1.66)	(3.32)	

Notes: The dependent variable is the contribution in the Public-good game (in percent of the endowment). Data are pooled from the Non-election and Election rounds. Standard errors in parentheses are clustered at the individual level. Covariates include ethnicity indicators, a gender indicator, education controls, and the Raven's test score. FWER p-values are simulated as described in the preanalysis plan for column (3).The first F-test tests the hypothesis that the average level of coethnic bias across both the Non-election round and the Election round is zero; the second tests the hypothesis that the difference in coethnic bias across the Non-election round and the Election round is zero. All specifications exclude ethnic Kamba subjects, as specified in the pre-analysis plan. The specifications for the full sample exclude the Ethnic–Political priming group, which was only administered in the Election Round. $^{*}p < 0.10$; $^{**}p < 0.05$.

Dictator game to coethnics and non-coethnics is less than one percentage point, and among subgroups defined by age, education, and major ethnic groups, the differences are at most 1.2 percentage points. None of these differences are statistically significant at traditional confidence levels. A similar pattern holds for Public-good game contributions between coethnic and mixed groups, with coethnic bias estimates of at most 1.9 percentage points for any subgroup. Again, none of these differences are significant. This is also true when we split the sample according to the Raven's score, implying that the null results persist among participants with high or low cognitive ability.

The evidence from the Dictator game and Public-good game is thus both consistent with little to no coethnic bias for this population on average, as well as for the major demographic subgroups. Online Appendix D contains further descriptive statistics and histograms, as well as tests indicating that the null hypothesis of equality of the distributions of Coethnic and Non-coethnic transfers cannot be rejected in either game.[12]

12. A careful reader of Tables 2 and 3 will note that average transfers in the Dictator game and contributions in the Public-good game are significantly lower in the Election round. Although this finding

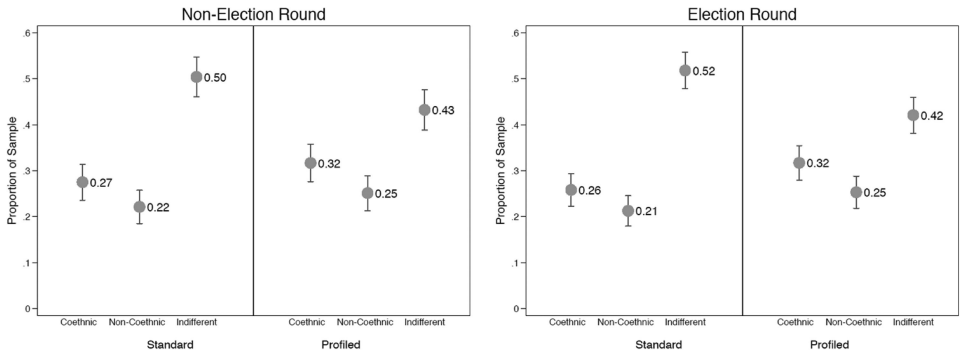

FIGURE 3. Coethnic bias in the Choose-Your-Dictator game. Sample averages and 95% confidence intervals for standard and profiled Choose-Your-Dictator (CYD) games during the Non-election round (left panel) and Election round (right panel).

The Choose-Your-Dictator game also sheds light on subjects' expectations about whether others will be differentially altruistic toward them. The overall preference for a coethnic dictator (captured in the standard Choose-Your-Dictator game) in the Non-election round is minimal, with 27% choosing a coethnic versus 22% choosing a non-coethnic, and half simply opting for "indifferent" (Figure 3, left panel). The patterns in the Election round (right panel) are nearly identical. Although seemingly small in magnitude, the effect is significant (Table 4, row 1) though not robust to the inclusion of covariates or focusing on the priming control group (columns (2) and (3)).

The more important test from the point of view of understanding coethnic bias is the difference between these patterns and those that emerge in the profiled Choose-Your-Dictator game, where the dictator is given information about the participant and is therefore perceived by the participant as being in a position to condition his/her generosity on the participant's ethnicity. In the profiled game in the Non-election round, the proportion of participants choosing a coethnic dictator rises slightly, to 32%, as does the proportion who choose a non-coethnic (25%) (Table 1, top row and Figure 3, left panel). Hence, the difference between the shares of respondents choosing a coethnic versus a non-coethnic increases only slightly from 5% in the standard game to 7% in the profiled game. In the ordered logit analysis, this difference in behavior between the standard and profiled games is captured in the coefficient estimate on the Profiled Game × Coethnic term, and this effect is small and not statistically significant (Table 4, row 2); taking into account the issue of multiple hypothesis testing, the FWER adjusted p-value is close to one (at 0.993). Results for the Election round are similar (row 4). There is thus no evidence that participants are more likely to choose a coethnic dictator when the dictator has information about their home town (and thus a way to

is intriguing, interpreting it is complicated by the fact that the comparison across the two lab rounds bundles together variation in proximity to the election with a "time effect" that captures all of the other changes that took place in the broader Nairobi (or Kenyan) environment, the study samples, and the Busara Center lab space. We discuss this issue at greater length in Bjorvatn et al (2019).

TABLE 4. Choose-Your-Dictator game choices, in standard and profiled games.

	Full sample		No prime	FWER p-value
	(1)	(2)	(3)	(4)
Coethnic profile	0.25**	0.22	0.071	0.979
	(0.11)	(0.14)	(0.23)	
Profiled game × Coethnic	0.048	0.048	0.021	0.993
	(0.11)	(0.11)	(0.21)	
election round × Coethnic	−0.059	−0.0013	0.15	0.979
	(0.11)	(0.12)	(0.23)	
Election round × Profiled game × Coethnic	0.074	0.074	0.095	0.979
	(0.15)	(0.15)	(0.27)	
Covariates	No	Yes	No	
Observations	3924	3924	1020	

Notes: Ordered Logit specification, with dependent variable 0 = not chosen, 1 = indifferent, 2 = chosen. Data are pooled from the Non-election and Election rounds. Standard errors in parentheses are clustered at the individual level. All specifications include fixed effects for each Dictator-profile (12 profiles in total). The variable "Coethnic" indicates if the dictator profile is a coethnic or not. Covariates include interaction terms of the "Coethnic" indicator with a gender indicator, education controls, and the Raven's test score. FWER p-values are simulated as described in the pre-analysis plan for column (3). All specifications exclude ethnic Kamba subjects, as specified in the pre-analysis plan. The specifications for the full sample exclude the Ethnic–Political priming group, which was only administered in the Election Round. $^{**}p < 0.05$.

make an inference about their ethnic background), relative to when the dictator does not have this information. This is consistent with the previous findings since there is no evidence for coethnic bias in behavior in the Dictator game (Table 2). Expectations and actions regarding differential altruism across ethnic lines are thus aligned among our participants.

4.3. The Impact of Priming on Coethnic Bias

We also investigated whether priming participants to the salience of ethnicity, political competition or national identity affected the way they played the behavioral games. As we show in Table 5, none of our priming treatments had a statistically significant effect on behavior in either the standard Dictator game (column (1)) or the standard Public-good game (column (4)), and these effects did not differ significantly in the Election round (columns (2) and (5), respectively).

As noted, the ethnic–political prime was only included in the Election round. This is because, after finding no effect of the ethnic–cultural prime in the Non-election round, we hypothesized that this might be because our priming approach was too subtle. Hence, we decided to introduce a more "blatant" ethnic priming treatment in the Election round. In contrast to the ethnic–cultural prime, which sought to enhance the salience of ethnic identity by emphasizing cultural aspects of ethnic identity, this new treatment directly and overtly primed subjects to the link between ethnic identity and political outcomes. For example, one of the questions asked: "*Which of the following ethnic groups controls the largest share of cabinet positions?*" Yet this

TABLE 5. Priming effects in the standard Dictator game and Public-good game.

	Dictator game transfer (percent of endowment)		FWER *p*-value	Public-good game contribution (percent of endowment)		FWER *p*-value
	(1)	(2)	(3)	(4)	(5)	(6)
Election round	−6.08***	−7.60***	0.008	−2.61	−6.36*	0.321
	(1.18)	(2.38)		(1.61)	(3.24)	
Political competition prime	−0.63	−2.19	0.591	0.26	−1.72	0.826
	(1.67)	(2.37)		(2.28)	(3.23)	
Ethnic–cultural prime	−1.45	−0.52	0.8	1.48	0.32	0.919
	(1.67)	(2.37)		(2.28)	(3.23)	
National prime	−2.02	−4.43*	0.166	−0.37	−4.70	0.462
	(1.66)	(2.37)		(2.28)	(3.23)	
Election × Political competition prime		3.14	0.675		3.96	0.664
		(3.36)			(4.58)	
Election × Ethnic–cultural prime		−1.88	0.815		2.29	0.84
		(3.35)			(4.57)	
Election × National prime		4.82	0.408		8.68*	0.321
		(3.35)			(4.56)	
Election × Ethnic–political prime		0.37	0.994		−1.77	0.964
		(2.38)			(3.24)	
Observations	1211	1362		1211	1362	

Notes: The dependent variables are the transfer in the Dictator game (in percent of the endowment) in columns (1) and (2) and the contribution in the Public-good game (in percent of the endowment) in columns (4) and (5). Data are pooled from the Non-election and Election rounds. Standard errors in parentheses are clustered at the individual level. FWER *p*-values are simulated as described in the pre-analysis plan for columns (2) and (5). The Ethnic–political priming, which was only implemented in the Election round, is not included in columns (1) and (4). $^{*}p < 0.10$; $^{***}p < 0.01$.

prime also had no significant effect on transfers in either game (Table 5, columns (2) and (5)). In Online Appendix A, we show that there is also little evidence of priming effects in the profiled Dictator game, profiled Public-good game or Choose-Your-Dictator game. Few of the relevant differential priming effect estimates are statistically significant in the per-comparison sense, and almost none survive the multiple testing adjustment.

Although we cannot rule out the possibility that stronger primes might have generated different results, and while we concede that these tests are less well powered than those that employ the pooled sample, we view these results as highly suggestive, and consistent with our main findings regarding both the lack of evidence for coethnic bias in our sample and the absence of any differences in levels of bias across the Election and Non-Election lab rounds. The results suggest that coethnic bias does not appear to be "just below the surface" in a way that might be triggered by priming subjects to ethnicity or political competition. They also suggest that priming subjects to their membership in a superordinate national identity does not reduce their (already low) levels of coethnic bias.

4.4. Implicit Association Tests (IATs)

As an additional strategy for measuring ethnic bias, we included an Implicit Association Test (IAT) as part of our main lab protocols in the Election round. The IAT measures a type of ethnic bias that is conceptually different from the bias measured by the DG, PG, or CYD games, as it picks up potentially unconscious positive or negative associations with in- and out-group members. Hence, it is useful to view the IAT as a complementary approach to measuring ethnic bias.

The IAT is premised on the idea that individuals find it easier to respond to concepts that are "strongly associated" (i.e., about which they agree) than for concepts that are weakly associated (Nosek et al. 2007). In our lab, we focused on the degree to which respondents have a bias in favor of their own ethnic group.[13] Others have used IATs to assess coethnic bias in Africa (Lowes et al. 2015), gender bias in India (Beaman et al. 2009), and racial bias in the United States and elsewhere (Greenwald et al. 2003; Bertrand et al. 2005; Ogunnaike et al. 2010; Rooth 2010).[14]

The specific IAT that we developed provides a measure of a respondent's automatic associations with certain Kenyan ethnic groups. In the "congruence" round of the IAT, individuals were timed to assess how quickly they were able to associate coethnics with "good" traits and non-coethnics with "bad" traits. In the "dissonance" round, coethnics were associated with bad traits and non-coethnics with good ones. A faster response time for congruence tasks compared to dissonance tasks implies that the

13. We also carried out a parallel "national" IAT to capture the strength of feelings toward Kenya, and plan to discuss those results in a future study (see Online Appendix D).

14. Blum, Hazlett, and Posner (2018) use a similar test, the Affect Misattribution Procedure (AMP), to measure implicit bias across ethnic lines in Kenya.

TABLE 6. Coethnic bias in the implicit association test (IAT).

	Ethnic IAT: *D*-score				FWER *p*-values
	(1)	(2)	(3)	(4)	(5)
Constant	0.079***	0.013			
	(0.027)	(0.058)			
Political competition prime		0.046	0.15**	0.15**	0.133
		(0.081)	(0.067)	(0.066)	
Ethnic–cultural prime		0.077	0.11	0.082	0.459
		(0.085)	(0.067)	(0.066)	
Political–ethnic prime		0.100	0.10	0.088	0.459
		(0.086)	(0.067)	(0.066)	
National prime		0.12	0.11	0.091	0.459
		(0.088)	(0.067)	(0.066)	
Order controls	No	No	Yes	Yes	
Controls	No	No	No	Yes	
Observations	547	547	547	547	

Notes: The dependent variable is the *d*-score: the difference (in standard deviation units normalized by respondent) between the average response times in the dissonance and congruence IAT rounds. Data are from the Election round only. FWER p-values are simulated as described in the pre-analysis plan for column (3). Order Controls are indicators for the randomized order of (i) the ethnic and national IAT, and (ii) the Dissonance and Congruence rounds within each IAT. Columns (1) and (2) also adjust for the different sampling weights of these randomized IAT order-groups. Covariates include ethnicity indicators, a gender indicator, education controls, and the Raven's test score. All specifications exclude ethnic Kamba subjects, as specified in the pre-analysis plan. Slow-response observations are excluded, as specified in the pre-analysis plan. $^{**}p < 0.05$; $^{***}p < 0.01$.

subject has a more positive attitude toward coethnics (or a more negative attitude toward non-coethnics).[15]

The outcome measure for the IAT is the within-respondent normalized difference in average reaction times (ART) between the dissonance (D) and congruence (C) tasks, called the d-score for subject i: $d_i = ART_i^D - ART_i^C / \sigma_i$, where σ_i is the standard deviation of a respondent's reaction times across all items. In keeping with the literature, we interpret a higher d-score as indicating stronger bias. We estimate the average level of coethnic bias in the IAT both with and without the priming treatments.

We find a statistically significant and positive degree of bias in this test (with a magnitude of 0.079 standard deviation units; p-value < 0.01; Table 6, column (1)), which contrasts with the null results in the behavioral games. However, this level of bias is considered "small" in the related IAT research literature in psychology (Cohen 1988). For instance, studies of the bias whites hold against blacks in the United States find estimates that are roughly six times as large, in the range from 0.45 to 0.52 (Nosek et al. 2007). In India, the average bias toward female political leaders is notably higher, at 0.11–0.15 (Beaman et al. 2009). In Congo, Lowes et al. (2015) report d-score values in a study of coethnic bias of 0.14, nearly twice as large as our estimates. With respect

15. Examples of screen shots from the IAT are in Online Appendix B, and the pre-analysis plan for the Election round in Online Appendix C contains further details on the IAT.

to priming, there is some evidence that the political competition treatment increased coethnic bias in the IAT (Table 6, columns (3) and (4)). We prefer not to overemphasize this effect since it is not significant at conventional levels after adjusting for multiple inferences (column (5)). Still, a weak implicit bias that can be amplified in a context of political competition, is consistent with the prominence of ethnicity in Kenya's electoral campaigns, as well as with the increased salience of ethnicity around election time in Africa at large (Eifert et al. 2010).[16]

4.5. Addressing Experimenter Demand and Social Desirability Biases

A potential concern with the attempt to measure coethnic bias through experimental games is that game behavior may be affected by the desire not to be seen to be discriminating along ethnic lines.[17] Anticipating this issue, we designed the laboratory protocols to reduce the likelihood that participants would be cued to our interest in ethnicity.[18] In addition, we assured lab participants that their answers would be kept confidential and seated them in private cubicles with headphones.

To ascertain how successful we were in masking our interest in ethnic discrimination, we conducted exit interviews during our piloting in which we asked subjects detailed questions regarding their understanding of the main focus of the experiment. We asked these questions both at the midpoint of the lab sessions and at their conclusion. The most common response was that the experiment was mainly about economic or business issues. Roughly equal numbers of subjects thought the activities were about education or about "tribe" (i.e., ethnic issues). The proportion that believed that ethnic issues were a focus of the experiment rose slightly by the end of the lab, perhaps due to the nature of the Choose-Your-Dictator game, which was played last, which asks participants explicitly to choose between two other players after being given information about those players' home towns (among other characteristics), but this still remained less than a quarter of all subjects. We are therefore confident that the vast majority of participants were not aware of the study's core research aims (for further details, see Online Appendix B).[19]

Participants' behavior in the IAT also provides a social desirability bias-free check on our main results. We interpret our significant but substantively small estimates of ethnic bias in the IAT, taken alongside the robust null findings in the behavioral games,

16. Note that since the IATs were administered only in the Election round we are unable to assess whether implicit coethnic bias is different in actual proximity to an election, but the political competition priming was set up to mimic variation in proximity to elections within the lab.

17. Referring to the theoretical model presented in Section 2, the degree of altruism can serve as a self-regulating filter between an unconscious implicit bias and actual bias in behavior.

18. Even the ethnic priming was subtle, presented in the form of a quiz that also included other neutral questions.

19. Since these questions were only asked during our piloting, it is unfortunately not possible to test whether participants who thought the experiments were about "tribe" exhibited different patterns of behavior in the games.

as providing only weak support for the presence of social desirability bias in our main results.

4.6. Pre-Analysis Plan and a "Selective Presentation Test"

A strength of our analysis is that we pre-specified our analytical approach in a pre-analysis plan (AEA Social Science Registry ID# AEARCTR-0000016; see Online Appendix C). A registered pre-analysis plan helps address concerns of publication bias and data mining. This latter concern is especially salient in a study such as ours given its complex structure, with multiple games, primes, subsamples, lab rounds, and potential tests.

Although the past few years have seen a rise in the use of pre-analysis plans for field experiments (Casey et al. 2012; Miguel et al. 2014), they are far less common in laboratory studies—in part because, as Coffman and Niederle (2015) argue, they may be superfluous in settings where experiments can be easily replicated. Although we wholeheartedly endorse Coffman and Niederle's emphasis on replication, we view replication studies and pre-analysis plans as complements rather than substitutes. Moreover, in certain settings where lab experimental researchers only have "one shot" at a particular type of data—due to access, cost, or timing—replication is infeasible. Hence, since we examine ethnicity in the unique context of Kenya's 2013 national election, we view the pre-analysis plan as a crucial component of our study.

Given the space constraints of a journal article, it was not possible to present every result that was pre-specified. This is a common problem for authors of complex, multifaceted research projects. It raises the possibility that we might have selectively emphasized a non-representative subset of results that were more (or less) statistically significant, or more consistent with our theoretical priors. One means—to our knowledge, novel—of assessing whether we might have done this is to undertake a "selective presentation test" in which we plot the distribution of p-values presented in the paper's main tables against the distribution of p-values for all hypothesis tests specified in the pre-analysis plans.

We do this in Figure 4. The distributions are broadly similar, but the main study tables do appear to slightly over-report statistically significant estimates (those with p-values less than 0.05) for both the unadjusted p-values (panel (a)) and the FWER adjusted p-values (panel (b)) and to somewhat under-report those with p-values close to one. Consistent with this visual inspection, the Kolmogorov–Smirnov test on the equality of these two distributions is rejected at 95% confidence for both the unadjusted and the FWER adjusted p-values. Of course, since our central finding is a null result, the slightly disproportionate emphasis on statistically significant effects cannot be driving our main conclusions. Rather we think it likely reflects our decision to relegate a large number of null priming treatment estimates and interaction effects to the Online Appendix.[20]

20. Out of the twelve p-values below 0.05 in the paper, six are from the non-causal Election-round effect, and three are related to the IAT.

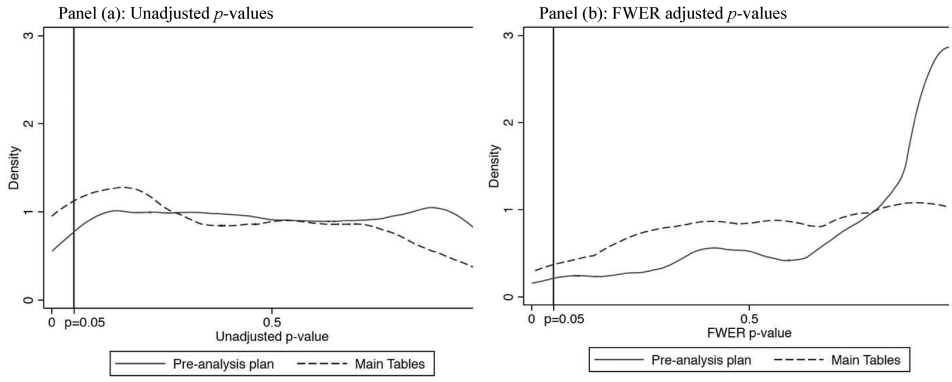

FIGURE 4. Distribution of p-values in pre-analysis plans versus main tables. The distribution of p-values from the pre-analysis plan includes p-values for all hypothesis tests discussed in both pre-analysis plans (Non-election round, Election round). The distribution of p-values from the main tables includes all tables (Tables 1 through 6) in this article. The dark vertical line denotes statistical significance at the standard 95% confidence level. Panel (a) presents p-values unadjusted for multiple testing. Panel (b) presents FWER adjusted p-values. Kolmogorov–Smirnov tests on the equality of the two distributions yields a p-value of 0.014 in panel (a), and 0.005 in panel (b).

5. Conclusion and Broader Implications for Africa

Using a much larger sample than prior studies and a richer set of experimental methods and measures, we find little evidence for an active preference to discriminate against non-coethnics in favor of coethnics in an urban Kenyan sample. These findings present a challenge to both theory and conventional wisdom about why ethnicity matters. With respect to theory, our results raise questions about why our findings diverge so sharply from the consistent results in the social psychology literature with respect to in-group favoritism (Tajfel and Turner 1986; Chen and Li 2009). With respect to conventional wisdom, the findings suggest that mechanisms other than coethnic bias in preferences, such as the role of ethnicity in social institutions (Miguel and Gugerty 2005; Habyarimana et al. 2007, 2009; Burgess et al. 2015), should be considered more seriously as causes of the political, social and economic outcomes that ethnicity is commonly thought to produce in Africa. We briefly discussed one such mechanism, namely the interplay of a weak but lingering unconscious bias with certain societal contexts such as electoral campaigns, but further research is needed in this area.

Notwithstanding the importance of our overall null findings for coethnic bias in the behavioral games, it is reasonable to wonder, given the urban nature of our study location, whether our results should be interpreted as speaking only to the way ethnicity operates in an urban setting. Indeed, our findings are in apparent tension with the results of Hjort's (2014) important study of coethnic bias in rural Kenya. Taking advantage of the random assignment of workers to teams on a flower farm to study whether within-team productivity is lower when those teams are ethnically diverse, Hjort finds strong evidence of discrimination by team members on behalf of coethnics.

There are several possible ways to account for the differences between these findings and our own that have nothing to do with the contrasting urban and rural locations of the two studies. The most immediate is that, although coethnic bias may play a role, the negative diversity effects in Hjort may be caused by other mechanisms, either alone or in interaction with coethnic bias. For example, the fact that Hjort shows that shifting to group-based pay on work teams mitigates much of the negative effect suggests that institutional factors are critical. Hjort's design also makes it hard to rule out the possibility that ethnicity matters by providing a technology that facilitates team production. To the extent that these alternative mechanisms are driving Hjort's results, his findings are not contradictory to ours—indeed they reinforce our point regarding the salience of other channels in explaining ethnicity's effects. However, it is difficult to completely rule out the possibility that the divergent findings are products of different degrees of coethnic bias in urban and rural domains. Given the strictly urban nature of our subject pool, our study is not well suited to fully assess the possibility of different degrees of coethnic bias in urban and rural domains. However, we can make some progress by comparing the behavior in our sample of subjects who were long-time Nairobi residents with the behavior of those who had moved to the city relatively recently. Although only suggestive, this is a meaningful test to the extent that individuals gradually assimilate to local norms over time, such that those who have moved to Nairobi relatively recently have orientations to non-coethnics that are more similar to those who remain in rural areas. Such an interpretation is consistent with research that finds that migrants gradually adapt their social preferences and behaviors over time to the norms in their new locations (Laitin 1998; Henrich et al. 2006; Jang and Lynham 2015).

This analysis was not specified in our pre-analysis plan, and is thus more speculative than our other results. Although we would ideally focus on those individuals who had just moved to the city, there are relatively few such individuals in our sample: just 2% had lived in Nairobi for less than one year. However, a sizeable proportion (roughly 20%) had moved to Nairobi within 5 years and even more (40%) in the last 10 years. We examine the behavior of this latter group in comparison with that of longer-term urban residents. Consistent with the hypothesis that there might be an important urban–rural divide in coethnic bias, we find statistically significant differences in behavior of these two subsamples. Although individuals who have resided in Nairobi for more than a decade exhibit no evidence of coethnic bias in their behavior, more recent migrants are significantly more generous toward their coethnics in the Election round for the profiled Dictator game, giving an average of 4.2 percentage points (standard error 1.8) more to coethnics. Coethnic bias in the Dictator game is even larger among those who had resided in Nairobi for at most 5 years, with an average increase of 6.2 percentage points (standard error 2.4).

Migrants from rural settings thus appear to gradually assimilate to a new set of norms regarding interethnic cooperation and ethnic preferences over time, perhaps due to the far more extensive interactions they have with non-coethnics in Kenya's cosmopolitan capital. Further evidence comes from our political attitudes survey, which shows rising ethnic identification (relative to other dimensions of individual identity)

in the 2013 Election round among those who have lived in Nairobi for less than 10 years (coefficient estimate 8 percentage points, significant at 90% confidence) but not among longer-term Nairobi residents.

Obviously, these findings cannot be taken as causal for many reasons. Most importantly, we cannot rule out that the differences we find are driven by selective urban migration patterns over time rather than the true effect of the length of urban residency. Furthermore, as noted, these analyses were not pre-specified. Nevertheless, the suggestion that altruism vis-à-vis one's coethnics may be weaker in urban than in rural settings is provocative—in part because it would call for a reevaluation of the well-known variant of modernization theory that posits a positive relationship between urbanization and ethnic divisions (e.g., Young 1976; Bates 1983).

Our suggestive findings may also provide new insight into the future trajectory of ethnic divisions in a region whose urban population has swelled from 27% in 1990 to 40% today and is expected to reach 55% by 2050 (United Nations 2014). Others have pointed to the role that expanding democratization may play in dampening ethnic favoritism (Burgess et al. 2015). Gaining a better understanding of the role played by rapid urbanization will also be an important goal for future scholarship on ethnicity in Africa.

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Supplementary Data

Supplementary data are available at *JEEA* online.