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ABSTRACT

Elections affect the division of resources in society and are occasions for political elites to make appeals rooted in voters' self-interest. Hence, elections may erode altruistic norms and cause people to behave more selfishly. We test this intuition using Dictator Games in a lab-in-the-field experiment involving a sample of more than 1000 individuals in Kenya and Tanzania. We adopt two approaches. First, we experimentally prime participants to think about the upcoming or most recent elections and find that this priming treatment reduces how much money participants are willing to give to other players. Second, we compare results obtained across lab rounds in Kenya taking place right before the country's 2013 national elections and eight months prior, and find that selfishness is greater in the lab round more proximate to the election. Our results suggest that elections may affect social behavior in important—and previously unrecognized—ways.

1. Introduction

Elections are moments of intense competition for control of the government and its resources. They tend to polarize electorates, deepen social divisions and generate a discourse revolving around material benefits and voters' self-interest. A natural question, then, is whether elections might erode social norms about altruism and generate a permissive environment for selfish behavior.

The plausibility of the connection between elections and selfishness is especially strong in East Africa, a region in which elections are viewed as occasions for choosing which group will control the country's resources — for determining whose turn it is to “eat” (Wrong, 2009; Branch et al., 2010)—and where politicians often engage in (or are assumed to engage in) blatantly clientelistic behavior (Mueller, 2008; Vicente and Wantchekon, 2009; Kramon, 2018). We draw on data from two countries from this region, Kenya and Tanzania, both places where politics is strongly associated with rent extraction and clientelism, and

where voters often perceive political candidates as self-serving and corrupt. By emphasizing individual opportunism, elections in these countries may erode the social norm on altruism and render selfish choices more acceptable. Perceptions of group favoritism may also cause citizens, without even having to be reminded so by politicians, to view elections as moments for deciding which community will benefit from state resources, thus reinforcing the tendency to view distributive decisions in selfish terms.

To examine the hypothesis that elections promote selfishness, we employ a lab-in-the-field Dictator Game, which provides a standard measure of participants' altruism towards others (with selfishness understood as being the inverse of altruism). We test the impact of elections by experimentally priming more than 1000 lab participants in Kenya and Tanzania to think about the upcoming or most recent electoral contest. The experimental prime provides cleanly identified evidence on how the situational salience of elections affects selfishness. While this election prime is subtle, it leads to a decline in Dictator Game

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transfers of 3 percent in our pooled sample ($p = 0.031$), controlling for participant characteristics.

While the priming experiment indicates a causal effect of the election prime on selfishness, one may be concerned about how closely the priming treatment relates to exposure to actual elections. To address this concern, we evaluate the external validity of our findings beyond our priming experiment by comparing results obtained across lab rounds in Kenya that took place close to and more distant from the country's 2013 national election. We find that Dictator Game transfers dropped from 42.6 percent of the endowment in the sessions held 7–8 months before the election to 36.6 percent in the sessions held 1–2 months before the election ($p = 0.0003$).

Taken together, these results suggest that elections affect selfishness. Whereas prior work has examined the effects of altruism or selfishness on turnout and electoral behavior (Riker and Ordeshook, 1968; Fowler, 2006; Jankowski, 2007), researchers have yet to document the impacts of elections on altruism/selfishness. Outside the context of electoral politics, our paper relates to an emerging literature that examines how preferences – which are commonly assumed to be exogenously determined – are endogenously affected by prominent events. For instance, Voors et al. (2012) examine how exposure to conflict affects social, time and risk preferences in Burundi, while Fisman et al. (2015) show that subjects exposed to an economic recession exhibit greater selfishness. Our findings therefore highlight a novel source of changes in social preferences, with potentially important implications for our understanding of how exposure to electoral competition may affect social, economic, and political outcomes that extend far beyond the election itself.

2. Setting and sample

We recruited 1018 participants to play Dictator Games at experimental laboratories in Nairobi, Kenya and Dar es Salaam, Tanzania.² In Nairobi, participants attended one of two lab rounds held at the Busara Center for Behavioral Economics: one held in July–August 2012, seven to eight months prior to Kenya's 2013 elections ($N = 302$) and one held in January–February 2013, one to two months before these elections ($N = 300$). We refer to the former lab round as the *non-election round* and to the latter as the *election round*. None of our respondents had participated in previous lab sessions at the Busara Center, and all of them participated only once in our study. In Dar es Salaam, we set up our own lab to conduct a single lab round in November–December 2012, two years after Tanzania's 2010 national election and three years prior to its 2015 election ($N = 416$). In both cities, the samples were recruited from low-income neighborhoods and are broadly representative of the cities' largest ethnic groups.

In each round of the Dictator Game, we gave participants an endowment of KES 50 or TZS 1000 (circa USD 0.60), roughly equal to an hours' wage for workers in these locations, of which they decided how much to give away to a randomly selected participant in the lab about whom they were given no information. Online Appendix B provides the detailed lab protocols.

3. Experimental priming

Measuring the causal impact of elections on altruism requires random variation in lab participants' exposure to elections. We provide this via experimental priming. We pool the 1018 participants from both the Kenya and Tanzania labs and divide them into two groups: a control group ($N = 559$) and an election prime treatment group ($N = 459$). Both

groups were given a short, five question on-screen quiz immediately before playing the Dictator Game. In the control group, we asked neutral questions such as “How often do you ride a matatu/daladala every week?”; “What cell phone provider do you use the most?”; “In your opinion, what is the most popular soda drink?” In the treatment group, we asked a mix of neutral and election-oriented questions. The election prime questions were: “In your opinion, what share of the population voted in the last national election?”; “How many political candidates are (were) running for the Presidency (in 2010)?”; “How many political candidates are (were) running for the office of MP in your constituency (in 2010)?” Our treatment and control groups are balanced on observables (see Online Appendix Table A.1).

To transparently estimate the average treatment effect of our priming treatment, we employ standard OLS regressions. More precisely, our baseline specification is

$$y_i = \alpha + \beta_1 \text{ElectionPrime}_i + \beta_2 X_i + \varepsilon_i$$

where y_i is respondent i 's Dictator Game transfer, and ElectionPrime_i is an indicator variable for whether this respondent was in the priming treatment group. Hence, β_1 estimates the average treatment effect of the prime, namely the difference in average generosity in the priming versus the control group. We estimate this effect both unconditionally as well as conditional on a vector of control variables X_i .

In additional specifications, we augment our estimation with indicator variables for the Kenya election lab round and the Tanzania lab round. Each time, the coefficients on these indicator variables estimate the difference in average generosity for the respective lab round relative to the Kenya non-election round, conditional on respondents' treatment status and the control variables. Finally, we also estimate heterogeneous effects of the priming treatment by interacting the lab round indicators with the ElectionPrime_i indicator. Throughout this entire analysis, OLS regressions provide a transparent, straightforward and flexible estimation approach. We show our main findings in Table 1 and provide the full estimation output in Online Appendix Table A.2.

Pooling the two lab rounds in Kenya and the lab round in Tanzania, we find that the election prime leads to a reduction in the share of the endowment transferred to the other player, i.e. an increase in selfishness. Not controlling for any covariates, Dictator Game transfers fall by 2.7 percentage points, which is an economically meaningful decline of 6.6 percent (column 1 in Table 1, $p = 0.052$). This effect is robust to adding controls for gender, age, education and a score on a Raven (2008) test for cognitive ability administered during the lab (column 2, $p = 0.031$); to allowing for different base levels of generosity in the Tanzanian and the two Kenyan lab rounds (column 3); and to permitting the control variables to have different effects across lab rounds (column 4). In this most exhaustive specification for estimating the average treatment effect, the election prime reduces generosity by 3.12 percentage points ($p = 0.024$). Given the subtlety of our priming treatment—consisting of variation in just three purely informative questions asked to participants prior to playing the Dictator Game—we view these results as likely representing a lower bound on the effects of elections on selfishness.

The pattern of heterogeneity in the estimated treatment effect across lab rounds is interesting (columns 5–6).³ First, we find that the estimated effect of the election prime is close to zero in the Kenya *election round*. This may be because elections are already salient to participants attending the *election round* sessions, regardless of the experimental treatment: when the election campaign has primed subjects already, the marginal impact of the experimental priming may be weak to non-

² Berge et al. (2020) also examine Dictator Game transfers under varying proximity to elections, but with an emphasis on ethnic divisions, and an exclusive focus on Kenya. The current paper employs data from both Kenya and Tanzania to address a different question.

³ Columns 5 and 6 both estimate heterogeneous treatment effects of the election prime across lab rounds, each controlling for participant characteristics. Column 6 additionally allows the control variables to have different effects across lab rounds. Online Appendix Table A.2 presents the full estimation output.

Table 1
Impact of the election prime on dictator game transfers.

	Dictator Transfer (Percent of Endowment)					
	(1)	(2)	(3)	(4)	(5)	(6)
Election Prime	−2.68*	−2.98**	−3.04**	−3.12**	−2.18	−2.18
	(1.38)	(1.38)	(1.38)	(1.38)	(2.49)	(2.49)
Tanzania * Election Prime					−4.06	−4.06
					(3.33)	(3.33)
Kenya Election Round * Election Prime					2.37	2.16
					(3.53)	(3.55)
Kenya Election Round			−7.30***	−7.51**	−8.44***	−8.34**
			(1.82)	(3.10)	(2.53)	(3.52)
Tanzania			−3.78**	−3.76	−2.11	−2.00
			(1.71)	(2.63)	(2.27)	(3.06)
Constant	40.78***	38.61***	42.33***	42.07***	41.98***	41.59***
	(0.93)	(1.23)	(1.72)	(2.26)	(2.01)	(2.50)
Election Prime in Tanzania					−6.23***	−6.23***
					(2.21)	(2.21)
Election Prime in Kenya Election Round					0.19	−0.01
					(2.50)	(2.53)
Covariates	No	Yes	Yes	Yes	Yes	Yes
Interacted Covariates	No	No	No	Yes	No	Yes
Observations	1018	1010	1010	1010	1010	1010

Notes: The table presents OLS regressions with the Dictator Game transfer, as a percentage of the endowment, as the dependent variable. Data are pooled from the Tanzania round and the Kenya *non-election* and *election rounds*. Covariates include age, education level, a female indicator, and the participant's score on a Raven's test for cognitive ability. "Interacted covariates" indicates the inclusion of interaction terms of the covariates with indicator variables for the Kenya *election round* and the Tanzania round. Except for the female indicator, the interacted covariates are demeaned. The coefficient for "Election Prime in Tanzania" sums the first two coefficients in the column, while the coefficient for "Election Prime in Kenya Election Round" sums the first and third coefficient. [Online Appendix Table A.2](#) presents the exhaustive regression output. Standard errors in parentheses. P-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

existent. This is in line with the intuition in [Druckman and Leeper \(2012\)](#), who argue for a decreasing marginal impact of priming for subjects who are "pre-treated." Second, comparing across countries, we find no statistically significant difference between the election prime in Kenya and Tanzania. If anything, the effect of elections is stronger in Tanzania, as suggested by the 6.2 percentage point decline in average generosity in that country.

4. Cross-round comparison

While experimentally priming lab participants to think about elections is advantageous from a causal inference standpoint, it is not identical to exposing them to the mobilization, political appeals, and social pressures of an actual election campaign. To corroborate the external validity of our experimental results, we leverage the fact that we conducted our lab rounds in Kenya during two periods: one 1–2 months before the 2013 national elections (the *election round*), and one 7–8 months prior (the *non-election round*). This allows us to compare how participants play the Dictator Game when elections are imminent and when they are in the more distant future.⁴

To establish that elections are indeed more salient to participants during the *election round*, we start by documenting that participants who attended these lab sessions were more exposed to the coming electoral contest. As shown in Panel A of [Table 2](#), participants in the *election round* were significantly more likely than participants in the *non-election round* to say that they had attended a campaign rally (62 percent vs. 24 percent), received cash from a politician (27 percent vs. 12 percent), or received some other non-cash gift such as a T-shirt, food, or alcohol (13 percent vs. 8 percent). Election-related themes were also much more prevalent in the media during the *election round*. An analysis of major Kenyan media outlets aggregated by [KenyaMOJA.com](#) reveals that the terms "election," "political parties," and "vote" were mentioned much more frequently during the January–February 2013 period (455 times) than during the July–August 2012 period (206 times).

The increased salience of elections during the *election round* is

Table 2
Cross-lab round comparison in Kenya.

	Non-election Round (July–Aug 2012)	Election Round (Jan–Feb 2013)
A. Exposure to the political campaign		
Days from the 2013 election	208–231	24–50
% saying they attended a rally ^a	24	62
% saying they received cash ^a	12	27
% saying they received gift ^a	8	13
Media mentions of election-related issues ^b	206	455
B. Environmental factors^c		
GDP growth	4.7%	5.2%
Inflation	6.1%	4.5%

Notes:

^a Share of participants in our Kenya *non-election* or *election round* samples.

^b Mentions of the terms "election," "political parties," or "vote" in the Daily Nation, The Standard, Capital News, Nairobi Star, KTN, NTV, Citizen TV, and K24, as aggregated by [KenyaMOJA.com](#).

^c Macroeconomic data were retrieved on March 2, 2020 from the website of the Kenya National Bureau of Statistics at <https://www.knbs.or.ke/?cat=64>. GDP growth comparisons are based on data from the third quarter of 2012 and the first quarter of 2013. Inflation comparisons are based on data from August 2012 and February 2013.

associated with a significant decline in generosity. As shown in [Table 3](#), Dictator Game transfers are 6.02 percentage points lower during the election round (column 1, $p = 0.0003$). This difference is strongly robust to controlling for background characteristics such as gender, education, age, cognitive ability, number of children, and number of years living in Nairobi (columns 2–7).⁵ When we control for all these characteristics,

⁵ This robustness is reassuring, as there are some imbalances on these characteristics across the samples in the two Kenya labs rounds—notwithstanding our use of identical recruitment procedures in both rounds (see [Online Appendix Table A.6](#)).

⁴ For an analogous design, see [Michelitch \(2015\)](#).

Table 3

Dictator game transfers and proximity to elections in Kenya.

	Dictator Transfer (Percent of Endowment)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Election Round	−6.02*** (1.64)	−6.60*** (1.65)	−6.44*** (1.73)	−6.09*** (1.65)	−6.15*** (1.67)	−6.14*** (1.65)	−6.05*** (1.65)	−6.56*** (1.74)
1(Female)		4.31** (1.68)						4.00** (1.85)
Years of Education			−0.20 (0.25)					−0.07 (0.30)
Age				0.06 (0.07)				0.17 (0.13)
Raven's Test Score					−0.39 (0.82)			0.96 (1.01)
Number of Children						0.34 (0.39)		−0.24 (0.64)
Years in Nairobi							−0.16** (0.08)	−0.19** (0.09)
Constant	42.62*** (1.16)	40.36*** (1.45)	43.05*** (1.29)	42.67*** (1.16)	42.70*** (1.17)	42.67*** (1.16)	42.76*** (1.16)	40.76*** (1.71)
Observations	602	602	602	602	602	602	597	597

Notes: The table presents OLS regressions with the Dictator Game transfer, as a percentage of the endowment, as the dependent variable. Data are pooled from the Kenya *non-election* and *election* rounds. Except for the female indicator, the control variables are demeaned. The Raven's score for cognitive ability, administered during the lab, is demeaned and normalized to standard deviation units. Standard errors in parentheses. P-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

generosity declines by 6.6 percentage points in the *election round* (column 7, $p = 0.0002$)—an effect size similar to the 8 percentage point decline in generosity before and during the Great Recession, as reported in [Fisman et al. \(2015\)](#).⁶

This decline in generosity is found in all observable subgroups of the sample. As shown in [Table 4](#), we find a substantial and significant increase in selfishness in the *election round*, regardless of whether participants are female or male, young or old, high- or low-educated, have high or low cognitive ability, have few or many children, and are long-time residents of Nairobi or not. The results are also robust to limiting the sample to participants in the no-prime group rather than pooling across all participants, as we do in our main analyses. In fact, in the no-prime subsample we tend to find effect sizes that are – if anything – slightly larger (see [Online Appendix Tables A.8 and A.9](#)).

A concern with the cross-round comparison is that environmental factors that vary across the two lab rounds might offer alternative explanations for the differences we find in selfishness. As shown in Panel B of [Table 2](#), the macroeconomic situation in Nairobi improved modestly between the non-election and election rounds. GDP growth ticked up from 4 percent to 5.2 percent, and inflation decreased from 6.1 percent to 4.5 percent. Commodity prices, meanwhile, were largely unchanged (see [Online Appendix Figure A.1](#)). Insofar as improving economic conditions are likely to be associated with a decrease in selfish behavior ([Fisman et al., 2015](#)), these macroeconomic trends would bias *against* our finding of increased selfishness in the *election round*.⁷

5. Discussion

To the best of our knowledge, we are the first to document the impact of elections on selfishness. Our findings suggest that increasing the situational salience of elections makes people less generous to others. Our evidence is based on random experimental variation in priming conditions in Kenya and Tanzania, and is corroborated by the comparison across the two Kenyan lab rounds that differ in their temporal distance to elections.

⁶ Part of this reduction in generosity is due to a steep increase, by 11 percentage points, in the share of participants giving a zero transfer in the *election round* (see [Online Appendix Table A.7](#)).

⁷ Ideally, we would control for participants' individual economic conditions, but the data on this dimension has too many missing values.

Table 4

Dictator Game Transfers in Kenya: Non-election vs. Election Round.

	Non-election Round	Election Round	Difference
Full sample	42.6	36.6	−6.02*** (1.64)
Female	44.2	38.5	−5.71** (2.22)
Male	40.9	33	−7.89*** (2.46)
Below median age	41.1	35.3	−5.79** (2.56)
Above median age	44	37.6	−6.42*** (2.14)
Below median education	41.8	37.3	−4.5* (2.53)
Median education or above	43.3	35.7	−7.55*** (2.15)
Below median Raven's test score	44.2	37.2	−7.03*** (2.69)
Above median Raven's test score	41.7	36	−5.66*** (2.09)
Less than two children	41.6	36.5	−5.05** (2.41)
Two children or more	43.6	36.6	−6.99*** (2.27)
Residing less than 15 years in Nairobi	44.4	38.3	−6.06** (2.61)
Residing at least 15 years in Nairobi	41.3	34.9	−6.43*** (2.08)
Observations	302	300	

Notes: The first and second column show average Dictator Game transfers (in percentage terms) in the *non-election* and *election round*, respectively. The third column shows the estimated difference between the two, with standard errors in parentheses. The Raven's test score measures cognitive ability ([Raven, 2008](#)). P-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

While our findings are suggestive about the selfishness-inducing effects of elections per se, they are also no doubt rooted in factors specific to the context we study – specifically, the strong association of politics and elections in Kenya and Tanzania with clientelism, rent extraction, and corruption ([D'Arcy and Cornell, 2016](#); [Gray, 2015](#)). Indeed, the majority of Afrobarometer respondents in Kenya believe that most government officials are corrupt, and in Tanzania, more than 90 percent believe that at least some of them are corrupt ([Afrobarometer Data](#),

2011/2013). To us, it seems plausible that this strong association of politics and elections with personal rent extraction contributes to the erosion of the social norm on altruism as elections approach. Krupka and Weber (2009) show that the salience of this social norm is context dependent and malleable to priming. From this perspective, elections may then bring about a “scarcity mindset” (Shah et al., 2015), which induces more selfish behavior.

An alternative explanation is that the lower transfers in the Dictator Game at election time are not driven by a general increase in selfishness, but instead by increased ethnic polarization and an associated decline in generosity toward non-coethnics (Bates, 1983; Eifert et al., 2010; Hjort, 2014). While we do not have data on coethnic giving in Tanzania, we can test this hypothesis with additional data available from Kenya. After the “anonymous” Dictator Game that we employ in the analyses presented in this paper, we also implemented a “coethnic” Dictator Game in which respondents were given cues about the “home town” of the recipient (see Online Appendix B.2).⁸ Specifically, participants were given three pieces of background information about the other player with which they were paired: their education, age, and home town. 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. We included information about education and age to obscure the ethnic focus of this lab game.

Our results indicate that exposure to elections also reduces generosity to coethnics. First, in the cross-round analysis, generosity toward coethnics declines strongly and significantly in the *election round* (see Online Appendix Tables A.10 and A.11). Second, the election prime is also associated with lower generosity to coethnics (Appendix Table A.3), particularly in the *non-election round* (columns 5–6). However, this result is not statistically significant ($p = 0.12, 0.15$), in part because the absence of the Tanzania round leads to a loss of statistical power. In sum, we conclude that an amplified coethnic bias in altruism is not the main driver of our findings.⁹ Instead, the results strongly indicate an increase in general, indiscriminate selfishness.

While we do not find that people exhibit coethnic bias in their generosity, ethnic divisions do matter in a different way. Specifically, in Kenya the decline in generosity associated with elections is concentrated among those ethnic groups who lost the previous national election in 2007 (the Luo, Luhya and Kisii), while the winners of that election (the Kikuyu) exhibit no decline in generosity associated with elections. This pattern holds for both the cross-round comparison and for the priming treatment (see Online Appendix Tables A.4 and A.12), although we are somewhat underpowered to properly examine heterogeneous responses to the election prime. Importantly, the losing ethnic groups also reduce their generosity toward coethnics when exposed to elections, which again shows that our findings are not driven by a coethnic bias in altruism (see Online Appendix Tables A.5 and A.13). Instead, the overall decline in generosity for the losers of the election may be driven by resentment about missing out on material benefits associated with group favoritism by the election winners.

A final alternative argument is that it is not elections per se but the competitive atmosphere induced by elections that erodes altruism, as in Fehr and Schmidt (1999). The strong differences in the degree of electoral competition in Kenya and Tanzania allow us to shed light on this hypothesis. Since the creation of the multi-party system in Tanzania, a single party (Chama Cha Mapinduzi) has won all five elections and has

ruled uninterruptedly. In Kenya by contrast, government power has shifted hands several times. If competitiveness is what drives the decline in altruism, we would expect a stronger impact of elections on selfishness in Kenya than in Tanzania. Yet we find the opposite: if anything, the effect of elections on selfishness is stronger in Tanzania than in Kenya (see Table 1). Hence, the competitive aspect of elections does not appear critical for explaining our findings.

Since alternative explanations do not appear convincing, we view the link between elections and clientelism as the most plausible explanation for our findings. Admittedly though, our research design does not allow us to directly test this mechanism, so further research on this topic is clearly warranted. If confirmed by follow-up research, our findings may have more general implications for understanding how elections shape societal outcomes — including beyond East Africa. For instance, the increase in self-interest may encourage “pocketbook voting” based on personal economic considerations rather than “sociotropic voting” (Fiorina, 1981; Kinder and Kiewiet, 1981; Healy et al., 2017; Morton and Ou, 2019). More particularly in environments with widespread clientelism, there may be a negative feedback loop between clientelism, an augmented focus on self-interest during elections, and self-serving choices by voters. From this perspective, our findings may help explain why rent extraction remains rampant in young democracies (Keefer, 2005) and why democratization does not necessarily help in battling entrenched corruption (Sun and Johnston, 2009).

Data availability

Data will be made available on request.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.electstud.2020.102267>.

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⁸ In Kenya, “home town” is a strong indicator of a person's ethnicity.

⁹ Interestingly, Berge et al. (2020) document an absence of coethnic bias in altruism for lab participants in Nairobi, even close to the 2013 general election. Their finding confirms evidence on lack of ethnic bias in dictator games in East Africa from both Habyarimana et al. (2007) and Blum et al., (Forthcoming). This combined evidence therefore discredits an explanation for our current findings based on ethnic polarization.

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