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Elections and Selfishness[*]

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Abstract

Election outcomes affect the allocation of resources in society, and election periods may therefore trigger voters' self-interest. By employing dictator games in a lab-in-the-field experiment involving a sample of more than a thousand individuals in Kenya and Tanzania, we document that election periods increase selfishness, using two approaches. First, comparing lab rounds at different time periods in Kenya, selfishness increases in the lab round closer to election time. Second, to improve the scope for causal inference, we randomly vary the situational salience of election periods within the lab and find that this priming treatment similarly amplifies selfishness. These results may hold important implications for our understanding of the institutional role of elections, and how they shape societal outcomes, for instance by increasing social divisions.

Elections are moments of intense competition for control of the government and its resources. They tend to polarize the electorate and deepen social divisions between "us" and "them." They generate a discourse revolving around material benefits and appeals to voters' self-interest (see e.g. Healy et al. (2017)). And they frequently involve blatantly opportunistic or clientelistic behavior by political elites, who demonstrate by their actions that taking care of oneself, or one's group - that it is "our turn to eat" (Wrong, 2009) - is more important than taking care of the community as a whole. All of these aspects of elections raise a question: does exposure to elections lead to an erosion of social norms and a rise in selfishness?

We investigate this possibility using evidence from Dictator Games played by more than a thousand lab participants in Kenya and Tanzania, testing for the impact of elections by comparing results obtained in lab sessions in Kenya that were scheduled close to and more distant from the 2013 national election and by experimentally priming participants in both Kenya and Tanzania to think about the upcoming or most recent electoral contest. In both of these tests, we find evidence that elections promote selfishness. In the cross-lab session comparison in Kenya, we find that Dictator Game transfers dropped from 42.6 percent of the endowment in the session held 7-8 months before the election to 36.6 percent in the session held 1-2 months before the election (p=0.0003). In the priming analysis in both countries, we find that exposure to the election prime is associated with a decline in Dictator Game transfers of 2.7 percentage points (p=0.052) in the pooled sample. As we demonstrate below, both of these findings are robust to the inclusion of controls for participant characteristics. Our results highlight a potentially important - and heretofore undocumented - effect of elections on social cohesion.

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Setting and Sample

We recruited a total of 1,018 participants to play Dictator Games at experimental laboratories in Nairobi, Kenya and Dar es Salaam, Tanzania.[1] In Nairobi, participants attended one of two lab sessions 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 session as the *non-election round* and to the latter as the *election round.* In Dar es Salaam, we set up our own lab and conducted only one 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 its largest ethnic groups.

Proximity to Elections

The fact that we collected data at two different intervals from the 2013 Kenyan election allows us to test whether proximity to the election - and, presumptively, greater exposure to the mobilization and campaign appeals that surrounded it - alter how participants play the Dictator Game.[2] We do in fact find a significant difference: participants in the *election round* played the Dictator Game more selfishly on average than those in the *non-election round.* However, attributing this cross-round difference to proximity to the election requires a) that participants in the *election round* were in fact more strongly exposed to the election and its mobilizing currents than those in the *non-election round*;

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[1] As an aspect of their study, Berge et al. (2018) also examine dictator transfers in Kenya under varying proximity to the Kenyan elections, but with a focus on ethnic divisions. The current paper employs data from both Kenya and Tanzania to address a different question.

[2] Michelitch (2015) has an analogous design, but studies interethnic and interpartisan discrimination instead of general generosity.

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b) that there are no other differences in the context in which participants found themselves that might provide alternative explanations for the differences we observe; and c) that the characteristics of the participants who attended the two lab sessions are balanced (or, if not, that the results are robust to controlling for any differences).

Evidence that the participants who attended the *election round* lab sessions were plausibly more exposed to the coming electoral contest comes from several sources. As shown in Panel A of Table 1, election-related themes were 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 455 times during January-February 2013, compared to just 206 times during July-August 2012. Moreover, our lab 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). These benefits received at campaign rallies also illustrate the focus on materialistic self-interest during Kenyan election campaigns.

What about other contextual factors, unrelated to elections, that might plausibly affect levels of selfishness across the two lab rounds? As shown in Panel B of Table 1, the macroeconomic situation in Nairobi improved modestly between the *non-election* and *election round*s. 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 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*.

The third element required to lend plausibility to our cross-round comparison is balance across the two samples of participants on a range of potentially salient covariates.

	Non-election Round (July-Aug 2012)	Election Round (Jan-Feb 2013)	Difference
A. Exposure to the political campaign			
Days from the 2013 election	208-231	24-50	
% saying they attended a rally	24	62	
% saying they received cash	12	27	
% saying they received gift	8	13	
Media mentions of election-related issues[†]	206	455	
B. Environmental factors[††]			
GDP growth	4%	5.2%	
Inflation	6.1%	4.5%	
C. Participant characteristics			
Gender	52	66	-13***
Age	33	32	-1.1
Education	9.7	9.7	0
Raven's score (demeaned)	0	0.16	-0.16***
Observations	302	300	

Notes: Participant characteristics are sample averages. P-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[†] 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; [††] Macroeconomic data were retrieved on December 10, 2018 from the website of the Kenya National Bureau of Statistics at https://www.knbs.or.ke/category/key-figures/. 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.

Although our recruitment procedures were identical in both rounds, we do find some differences in the *election* and *non-election round* samples. As shown in Panel C of Table 1, while our samples are balanced with respect to age and education, our *election round* sample was significantly more likely to be female (66 percent vs. 52 percent) and per-

formed more strongly on the Raven's progressive matrix test, which we administered at the start of each lab session to measure cognitive ability. Although these differences might provide reason for concern, the election-round effect is strongly robust to controlling for the observable characteristics of the lab participants (see Appendix Table A.1). When we control for gender, education, age and the Raven's test score, generosity declines by 6.5 percentage points in the *election round* (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).[3] Moreover, as we show in Table 2, the substantial and significant increase in selfishness in the *election round* is found whether participants are female or male, young or old, high- or low-educated, and have high or low cognitive ability. While selection *on unobservables* could still be driving these results, we can certainly rule out that selection *on observables* is the driving factor behind our findings.

Election Priming

To provide further evidence of a causal effect of elections on selfishness, we also pursue a second empirical strategy that involves randomizing the situational salience of elections via experimental priming. To this end, we randomly divided the 1,018 participants in both the Kenya and Tanzania labs into two groups: a no-prime control group (N=559) and an election prime treatment group (N=459).[4] Participants in both groups are given a short, five question on-screen quiz immediately before playing the Dictator Game. Those in the control group were asked neutral questions such as "How often do you ride a

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[3]The reduction in generosity is also associated with a steep increase, of 11 percentage points, in the share of participants acting entirely selfishly (giving a zero transfer) in the *election round* (see Appendix Table A.2).

[4]Priming is a common tool in psychology and economics used to vary situational salience, in our case exposure to elections, see for instance Benjamin et al. (2010) and Callen et al. (2014).

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Table 2: 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 Ravens	44.2	37.2	-7.03***
			(2.69)
Above Median Ravens	41.7	36	-5.66***
			(2.09)
Observations	302	300	

Notes: The first and second column show average dictator 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. P-values: $^*p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

matatu (daladala for Tanzania) 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 had three priming questions related to elections, while the other two were neutral. In Kenya, the election prime questions were: "In your opinion, what share of the population voted in the last national election?", "How many political candidates are running for Presidency?";"How many political candidates are running for the office of MP in your constituency?" In Tanzania, we slightly tweaked these questions to ask about the 2010 Tanzanian election. Our randomization appears successful, since treatment and control groups are balanced on observables (see Appendix Table A.3).

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Table 3: Impact of the Election Prime on dictator transfers

	Dictator Transfer (Percent of Endowment)				
	(1)	(2)	(3)	(4)	(5)
Election Prime	-2.68*	-2.98**	-2.74**	-3.04**	-3.12**
	(1.38)	(1.38)	(1.38)	(1.38)	(1.38)
Election Round			-6.03***	-7.30***	-7.51**
			(1.78)	(1.82)	(3.10)
Tanzania			-3.44**	-3.78**	-3.76
			(1.66)	(1.71)	(2.63)
Constant	40.78***	38.61***	44.00***	42.33***	42.07***
	(0.93)	(1.23)	(1.43)	(1.72)	(2.26)
Covariates	No	Yes	No	Yes	Yes
Interacted Covariates	No	No	No	No	Yes
Observations	1018	1010	1018	1010	1010

Notes: The table presents OLS regressions with the dictator 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*. Standard errors in parentheses. P-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Covariates include age, education level, a gender indicator, and the Raven's test score. "Interacted covariates" indicates the inclusion of interaction terms of the covariates with indicator variables for the *election round* and the Tanzania round. Except for the gender indicator, the interacted covariates are demeaned.

To examine the impact of election priming on generosity, we employ standard OLS regressions. We estimate the average impact of election priming in the full sample, combining the two lab rounds in Kenya and the lab round in Tanzania. In line with how generosity declined in the *election round*, we find that the election prime leads to increased selfishness (see Table 3). Not controlling for any covariates, dictator transfers fall by 2.7 percentage points, which is an economically meaningful decline of 6.6 percent (column 1, p=0.052). This effect is robust to adding controls for gender, age, education and Raven's score for cognitive ability (column 2, p=0.031), to allowing for different base levels of generosity in the two lab rounds (columns 3 and 4) or different effects of the

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controls across lab rounds (column 5). In this final, most exhaustive specification, the election prime reduces generosity by 3.12 percentage points (p=0.024).[5]

These results provide further evidence that exposure to elections reduces generosity. While the magnitude of the decline (3 percentage points) is only half the size of the one we estimate via the cross-round comparison (6 percentage points), we note that the priming treatment is quite subtle - consisting of variation in just three purely informative questions asked to participants prior to playing the Dictator Game. Taking into account this subtlety of the prime, its impact on generosity is notable.

Discussion and Conclusion

Our findings suggest that increasing the situational salience of elections - either by moving the lab session closer to election day or by experimentally priming participants to think about elections - makes people less willing to share. To the best of our knowledge, we are the first to study and provide evidence of this impact of elections on selfishness.

Our results raise a number of questions for future research. First, to what extent can our findings be generalized? Is the link between elections and selfishness an African (or developing country) phenomenon or a more general effect? This is an empirical question that can be best addressed by replicating our design in other settings, and by studying the robustness of the result to variations in the design and setting. Second, which components of elections are most important in generating the decline in generosity that we observe?

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[5]The strength of the priming effect is heterogeneous across lab rounds. The effect is quite strong in Tanzania, and weaker in the Kenya *non-election round*. This difference is at most borderline significant, though (see Appendix Table A.4). In the Kenya *election round*, we find no priming effect. Following the intuition on the impact of priming in Benjamin et al. (2010), we believe this may be because elections are already salient to participants attending the *election round* sessions, so the marginal impact of priming is likely quite weak.

Several features of elections and the mobilization campaigns that surround them provide plausible explanations: clientelism and the example set by rent-extracting politicians; the winner take all nature of (especially presidential) contests; the focus on material issues during election campaigns; the "us" versus "them" nature of political appeals; among several other channels. Our design does not make it possible to tease apart these different explanations. Further research will be required on which aspects of the political context are more salient in amplifying selfishness during elections.[6] The results of such investigations can provide guidance on the sorts of interventions and institutional frameworks that may be most helpful in mitigating the negative effects of elections on generosity.

Third, the laboratory environment we exploit in our study has important advantages for measurement and experimental control, but it also has limitations. In particular, it raises questions about the relation between our observed increase in selfishness in the lab and how people interact outside of the lab setting. For instance, we might want to know how long the negative effects of exposure to elections on generosity last. Even more importantly, we can ask whether the focus on self-interest during elections leads to democracy-undermining outcomes such as an increase in social divisions, a reduced willingness to accept close election outcomes, or an amplification of inefficient special interest policies, to the detriment of Pareto-improving policy choices. Our results on the impact of elections on selfishness therefore provide the foundation for an important

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[6]One alternative possibility is that our findings are driven by the association between elections and conflict. Since exposure to violence may alter preferences (e.g., Voors et al. (2012); Callen et al. (2014)), this hypothesis appears potentially salient - especially for Kenya, whose recent history of political violence cast a dark shadow during the run-up to the 2013 election. Interestingly though, we find that the priming impact is, if anything, more negative in Tanzania than in Kenya (see Appendix Table A.4). This is not what we would have expected to find had the association between elections and violent conflict been the primary driver of the decline in generosity.

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agenda for future research.

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Appendix A   Supplementary Tables and Figures

Figure A.1: Commodity prices in Kenya
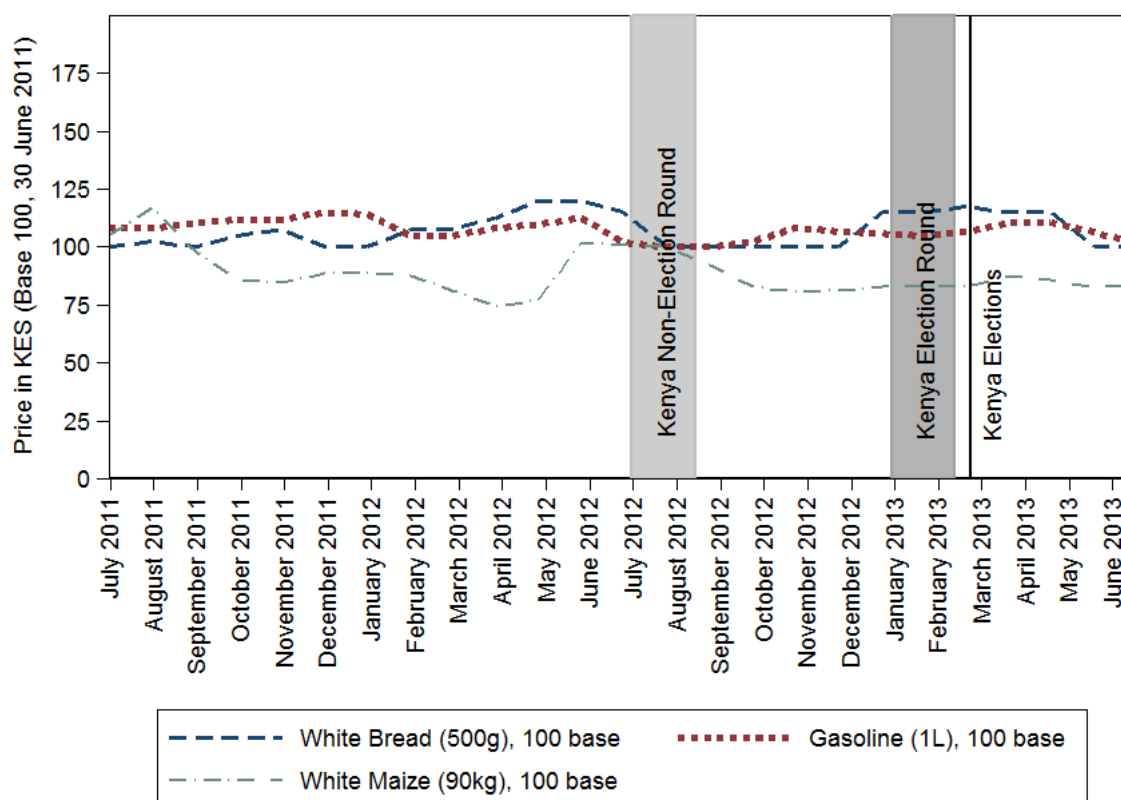
Notes: Data source for Kenya is FEWS NET (USAID) and the Kenya Ministry of Agriculture, Livestock, and Fisheries. The Kenya Non-Election Period was from July to August 2012. The Kenya Election Period was from January to February 2013. The Kenyan national elections took place in March 2013.

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Table A.1: Dictator transfers and proximity to elections

| | Dictator Transfer (Percent of Endowment) | | | | | |
	(1)	(2)	(3)	(4)	(5)	(6)
Election Round	-6.02***	-6.60***	-6.44***	-6.09***	-6.15***	-6.52***
	(1.64)	(1.65)	(1.73)	(1.65)	(1.67)	(1.73)
1(Female)		4.31**				4.46**
		(1.68)				(1.79)
Years of Education			-0.20			-0.01
			(0.25)			(0.29)
Age				0.06		0.06
				(0.07)		(0.09)
Raven's Test Score					-0.39	0.54
					(0.82)	(1.00)
Constant	42.62***	40.36***	43.05***	42.67***	42.70***	40.24***
	(1.16)	(1.45)	(1.29)	(1.16)	(1.17)	(1.69)
Observations	602	602	602	602	602	602

Notes: The table presents OLS regressions with the dictator 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 gender indicator, the control variables are demeaned. Standard errors in parentheses. P-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Dictators giving a zero transfer and proximity to elections

| | Dictator Transfers Zero | | | | | |
	(1)	(2)	(3)	(4)	(5)	(6)
Election Round	0.11***	0.12***	0.11***	0.11***	0.11***	0.11***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
1(Female)		-0.02				-0.03
		(0.03)				(0.03)
Years of Education			-0.00			-0.00
			(0.00)			(0.00)
Age				-0.00		-0.00
				(0.00)		(0.00)
Raven's Test Score					-0.01	-0.02
					(0.01)	(0.02)
Constant	0.08***	0.09***	0.08***	0.08***	0.08***	0.10***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Observations	602	602	602	602	602	602

Notes: The table presents OLS regressions with as the dependent variable an indicator variable for whether the decision maker gives a zero transfer. Data are pooled from the Kenya *non-election* and *election rounds*. Except for the gender indicator, the control variables are demeaned. Standard errors in parentheses. P-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table A.3: Balance across control and treatment group

	Control	Election Prime	Difference
Age	33.1	33	-.14
	[11.2]	[11.4]	(.71)
Female	.5	.6	.03
	[.499]	[.496]	(.03)
Years of Education	9.5	9.3	-.18
	[3.34]	[2.95]	(.2)
Raven's score (demeaned)	0	.1	.1
	[3.73]	[3.65]	(.23)
Observations	559	459	

Notes: The table shows average values, with standard deviations in brackets, for the variables in each column for the control and the priming treatment group. The third column estimates the difference between the two and has standard errors in parentheses. None of the differences are statistically significant.

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Table A.4: Heterogeneous impact of the election prime across lab rounds

	Dictator Transfer (Percent of Endowment)			
	(1)	(2)	(3)	(4)
Election Prime	-3.04**	-1.00	-2.18	-2.18
	(1.38)	(1.76)	(2.49)	(2.49)
Tanzania * Election Prime		-5.23*	-4.06	-4.06
		(2.82)	(3.33)	(3.33)
Election Round * Election Prime			2.37	2.16
			(3.53)	(3.55)
Tanzania	-3.78**	-1.53	-2.11	-2.00
	(1.71)	(2.09)	(2.27)	(3.06)
Election Round	-7.30***	-7.27***	-8.44***	-8.34**
	(1.82)	(1.82)	(2.53)	(3.52)
Constant	42.33***	41.37***	41.98***	41.59***
	(1.72)	(1.79)	(2.01)	(2.50)
Election Prime in Tanzania		-6.23	-6.23	-6.23
		(2.20)	(2.21)	(2.21)
Election Prime in Election Round			0.19	-0.01
			(2.50)	(2.53)
Covariates	Yes	Yes	Yes	Yes
Interacted Covariates	No	No	No	Yes
Observations	1010	1010	1010	1010

Notes: The table presents OLS regressions with the dictator transfer, as a percentage of the endowment, as the dependent variable. Data are pooled from the Tanzania round and the two Kenya rounds. Standard errors in parentheses. P-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Covariates include age, education level, a gender indicator, and the Raven's test score. Interacted covariates indicates the inclusion of interaction terms of the covariates with indicator variables for the election round and the Tanzania round. Except for the gender indicator, the interacted covariates are demeaned. The coefficient for Election Prime in Tanzania sums the first two coefficients in the column. The coefficient for Election Prime in Election Round sums the first and third two coefficients in the column.

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