

The Impact of Mass Bed Net Distribution Programs on Politics

Evidence from Tanzania

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Abstract

Functioning democracy requires that citizens reward politicians who deliver benefits, yet there is surprisingly little causal evidence of changes in citizen views or behavior in response to specific government programs. This question is examined in Tanzania, which has recently implemented large health programs targeting diseases such as HIV/AIDS and malaria. Tanzania's 2010–2011 national anti-malaria campaign took place concurrently with a national household survey, which enables a regression discontinuity design based on interview date to estimate the effect of

this program on the popularity of local politicians. Bed net distribution results in large, statistically significant improvements in the approval levels of political leaders, especially in malaria endemic areas. Effects are largest shortly after program implementation, but smaller effects persist for up to six months. These findings suggest that citizens update their evaluation of politicians in response to programs, especially when these services address important problems, and that the effects decay over time, but not completely.

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The Impact of Mass Bed Net Distribution Programs on Politics: Evidence from Tanzania

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I. Introduction

Do well-delivered government programs change the way citizens evaluate their political leaders? Despite the centrality of this question to the theory and practice of democracy, there is surprisingly little causal identification of links between government programs and citizen evaluation of political leaders. There is a large literature in rich countries which seeks to demonstrate that voters reward politicians for delivering economic growth ([Key, 1966](#); [Fiorina, 1981](#)). A burgeoning literature in Africa also tries to establish similar links between performance in office and voting ([Posner and Simon, 2002](#); [Weghorst and Lindberg, 2013](#)). Much more common, especially in developing countries, is the assumption that voters support politicians *not* based on their ability to foster economic growth or deliver effective social programs, but based on shared ethnic identity or in exchange for material benefits that they receive via patronage networks. Much of this literature assumes that the distribution of public spending in Africa, and in developing countries more broadly, is predominantly driven by clientelist logic, rather than according to programmatic criteria, because clientelist programs, while less effective at delivering goods and services to voters, are more likely to result in benefits to politicians. [Wantchekon \(2003\)](#), for example, shows that promising clientelist benefits is a more effective campaign strategy than committing to deliver public goods in Benin.

But even in developing countries with weak governance, some fraction of public goods are delivered on a programmatic basis. According to standard political economy models, politicians make strategic choices about whether to pursue programmatic or clientelist strategies based on the benefits (net of costs) that they expect to receive from these choices. But this cost/benefit calculus is dependent on the reaction of citizens. A critical, unresolved question in the literature is whether citizens in developing countries react to effective, programmatic policies by increasing their support for politicians associated with these policies, or whether information deficits, attribution problems, or even preferences for other styles of resource distribution prevent such reactions. If good policy and effective programs are not rewarded even when delivered, it could help explain

perverse equilibria in which voters do not reward politicians who deliver public services, so politicians in turn see no incentive to deliver these services. Voters are thus conditioned to not expect such services and do not vote for politicians who promise them.

I study the reaction of citizens to a programmatic and well-organized delivery of a social good in Tanzania; the mass distribution of insecticide-treated bed nets (ITNs) in 2008 and 2010-2011.¹ Distribution of mosquito nets is a particularly interesting policy to study in this context because it involves direct, highly visible and clearly attributable transfer of a good from the government to individuals for free. This is quite different from other government actions such as management of the economy or stewardship of a national health or education system, where the link from a given politician's effort to individual benefit is more diffuse (Harding and Stasavage, 2014). Furthermore, it is a universal program, in that all individuals (technically, all sleeping spaces) in the country were targeted.² Finally, it should be noted that these are potentially life saving products: a meta-analysis of 6 RCTs estimates that distribution of ITNs results in an average reduction in all-cause mortality in children under age 5 of 18% (Lengeler, 2004).

Tanzania conducted two mass distributions of insecticide-treated bet nets, in 2008-2009 and 2010-2011. Fortuitously, these two bed net campaigns occurred at roughly the same time as the first two rounds of the Tanzania National Panel Survey (NPS)³. The timing of National Panel Survey interviews was randomized at the strata level in two groups (an early “cycle 1” and a later “cycle 2” group), in order to accurately measure agricultural production and consumption, without the distortion of seasonality effects. The result of this unusual cross-cutting of national surveys and mass national health campaigns means that it is possible to exploit a regression discontinuity

¹For detail on the relatively smooth implementation of Tanzania’s mass ITN distribution campaigns see Bonner et al. (2011) and Renggli et al. (2013).

²Although there is good evidence that individuals in these settings are not willing to pay for even highly subsidized ITNs (Cohen and Dupas, 2010), they clearly value them, as they have been shown to use them once they have been given for free.

³The 2008-09 ITN campaign only partially overlapped with the National Panel Survey (13% of interviews took place after the ITN campaign), compared to a much more substantial overlap in 2010-11 (53% of interviews took place after the ITN campaign).

design, by comparing households which were surveyed immediately before the ITN campaign to those surveyed immediately after the campaign. This provides a unique opportunity to study the reactions of citizens, in real time and in a nationally representative sample, to a large scale public health campaign that involved direct distribution of well-known and valued items to individuals.

I find that citizens increase their approval of local politicians and government officials significantly (village chairmen, ward councilors, ward executive officers, and MPs) as a result of the 2010 net distribution campaign. Effects are notably stronger in districts which have higher underlying rates of malaria endemicity. The magnitude of effects fades sharply over time, but remain of considerable magnitude as long as one year after the program.

These findings are among the first to empirically demonstrate that citizens in developing countries change their opinions about political leaders in response to specific health programs. While changes in public support have been demonstrated (as discussed below) in some cases for cash transfer programs in Latin America and Southeast Asia, similar dynamics have not been shown via experimental or quasi-experimental design in sub-Saharan Africa, or for distribution of a health product. Moreover, the bed nets studied here are worth just a few dollars, which is just a small fraction of beneficiaries' income, in contrast to the 25 to 50 percent of income that CCT programs often represent ([Labonne, 2013](#); [Manacorda, Miguel and Vigorito, 2011](#)).

Second, in contrast to other contributions in the literature, these findings enable a clearer isolation of voter response to government actions themselves, rather than the economic benefits that governments actions can produce. The emerging literature on conditional cash transfers has an important weakness: because cash transfers represent both a program delivered by government (which could produce a rational decision to credit the government) and an immediate increase in income and living standards (which has been shown to increase approval of politicians even when they are not directly responsible for the economic growth), these studies cannot credibly isolate the mechanism at work. In other words, standard “pocketbook voting” cannot be separated from voter evaluation of a specific government benefit such as the CCT. This paper enables clearer isolation

of the mechanism by which programs translate into political support, since there is no possible pocketbook effect from an item worth just \$5.

Third, by leveraging the substantial variation in malaria prevalence across Tanzania, the analysis demonstrates that effects are larger when the problem addressed by the program is more salient, by showing that voters reward politicians more in settings with high malaria prevalence.

Fourth, contrary to some theories that voter responsiveness is a function of voter wealth or education, there is limited and relatively inconsistent evidence of variation in effect according to demographic characteristics such as gender, income, or education.

Finally, I demonstrate a “decay” dimension to the effects, demonstrating that treatment effects are very large in the immediate aftermath of the program, before fading notably over a 3 to 4 month period. This suggests that Tanzanian voters react quite similarly to voters in other settings (such as the US) in overweighting the very recent past in evaluating politicians (Bartels, 2008; Gerber et al., 2011; Healy and Lenz, 2014; Shaw and Gimpel, 2012). This temporal variation adds another dimension to this study which has been largely missing from the literature to date, and provides a relatively unique opportunity to examine the dynamic path of public opinion in response to government actions. This finding is also consistent with the literature on political budget cycles (Nordhaus, 1975; Tufte, 1978), and may also help explain some conflicting findings in the literature between those who find positive political benefits to programs such as infrastructure upgrading or CCTs and those who find no effect. This apparent inconsistency could be reconciled if heterogeneity in effect size is a function not just of the program’s effect, but when the effect was measured.

II. Related Literature

There is a large literature in developed countries which tries to ascertain whether voters support politicians based on evaluations of their overall performance (Key, 1966; Kinder and Kiewiet,

1979; Fiorina, 1981; Popkin, 1994), as a function of services or spending delivered to their community (Mayhew, 2004; Levitt and Snyder, 1997), or whether they are more likely to vote according to pre-existing loyalties, social groupings, or partisan attachments (Campbell et al., 1960; Achen and Bartels, 2016). In developing countries, the literature has focused more on patterns of clientelist distribution, with a great deal of effort devoted to establishing the ways in which public resources are targeted towards co-ethnics or co-partisans.⁴ As Golden and Min (2013) note, the response of voters to these strategies is a relatively small part of this literature. Yet they note that where this has been successfully analyzed, “studies overwhelmingly find that incumbent politicians are rewarded by voters for distributive allocations, and in particular for those that are clientelistic and from which recipients can be excluded.”

A major challenge in identifying the response of citizens to specific policies in developing countries is likely endogeneity of program targeting. However, the advent of large scale conditional cash transfers (CCTs) which either were initially randomized, or which have sharp eligibility thresholds, has led to important opportunities for causal inference and learning about the political effects of these programs. De La O (2013) shows that in Mexico, longer exposure to the *Progresa* CCT program increased voter turnout and vote share for incumbents, while Imai, King and Velasco Rivera (2016) reanalyze De La O (2013) and contest the results, finding no benefit to incumbents from *Progresa* or Mexico’s universal health insurance program, *Seguro Popular*. In Brazil, Zucco (2013) estimates positive impacts, using matching, on incumbent politicians from the *Bolsa Familia* CCT. Labonne (2013) provides evidence that incumbents benefited from a CCT program in the Philippines in municipalities where all villages received the program, compared to municipalities where only half of villages benefited, but only in competitive political environments.⁵ Baez et al. (2012) show that in Colombia, the *Familias en Accion* CCT also increased political participation and support for the incumbent party; Nupia (2012) also shows benefit for

⁴See Stokes et al. (2013) for an overview.

⁵However when a political dynasty dominates local politics, the program has no effect on vote share.

incumbents from this program. [Manacorda, Miguel and Vigorito \(2011\)](#) also find that recipients of Uruguay's unconditional transfer program (PANES) increased their support for the incumbent. [Galiani et al. \(2016\)](#) find no electoral effect from a relatively small cash transfer program in Honduras in 2000-2001, but find clear evidence of impact from a larger transfer program that took place a decade later.

Moving away from cash transfers to distribution or subsidy of other goods, [Pop-Eleches and Pop-Eleches \(2012\)](#) find that a Romanian computer subsidy voucher program also generated support for local governments. In African contexts, [Dionne and Horowitz \(2016\)](#) show that Malawi's agricultural input subsidies benefited the ruling party. In the infrastructure sectors, [Harding \(2015\)](#) demonstrates that in Ghana, voters reward improved road maintenance. In Bangladesh, [Guiteras and Mobarak \(2015\)](#) show that citizens reward leaders in response to a (randomly assigned) sanitation promotion program, except when they were informed that local leaders were not responsible for their receipt of the program. By contrast, [De Kadt and Lieberman \(2015\)](#) show that provision of government services (water, sanitation, refuse collection) actually reduced government support in South Africa between 2000-2011. Similarly, [de Janvry, Gonzalez-Navarro and Sadoulet \(2014\)](#) show that provision of agricultural land titles actually hurt the incumbent party in Mexico, as it led to a shift in vote share to the more free-market party (which was not the party that provided the land titles), and [Larreguy, Marshall and Trucco \(2015\)](#) find that urban land titles helped federal officials, who were credited with the program, but harmed local officials, who lost clientelistic opportunities. In an experimental framework, [Bobonis et al. \(2016\)](#) show that provision of a cistern, which reduced households' vulnerability to drought and reduced the frequency of clientelist requests made to politicians, also reduced the vote share of incumbent mayors.

The overall literature therefore is somewhat mixed on voter responsiveness to direct government provision of goods and benefits: high quality studies with credible research designs showing both evidence of large benefit and no benefit to politicians. Levels of political competition, partisan alignment between local and central government, gender of recipient, and the visibility and

attributability of the benefit have all been identified as links in the causal chain from service provision to voter reaction. We return to these potential effect modifiers in section VI.

III. Politics in Tanzania

Tanzania is an interesting setting in which to study the relationship between health programs and politician approval, as it has been one of the largest recipients of health sector aid in the world in recent years, and it has been among the leading countries in sub-Saharan Africa in implementation of malaria control programs. This influx of health sector aid has been facilitated in part by an unusual level of political stability. Since gaining independence in 1964, Tanzania has been ruled by a single party, Chama Cha Mapinduzi (CCM). After founding president Julius Nyerere's retirement in 1985, politics began to open up and in 1995, Tanzania had its first multi-party election. Yet despite having multiparty elections, CCM has never won less than 60% of the presidential vote or 70% of the seats in Parliament. Tanzania was in effect a single party state for much of the post-independence period, and the legacy of the single party state is still present in the way that CCM manipulates formal rules and distributes state resources to entrench its dominance ([Hoffman and Robinson, 2009](#)). But as CCM's inherited historical legitimacy as the party of independence and Julius Nyerere fades, national politics is growing more competitive, and CCM is increasingly eager to demonstrate that it is delivering services to voters to justify its continued rule. This desire has intensified since CCM's legitimacy has been called into question due to a number of high-level corruption scandals involving senior party cadres.

Since approval ratings of various political figures are key outcome variables, in this section I describe their roles in the Tanzanian political system. It is a centralized, strongly executive-dominated system. The President and members of parliament are both directly elected every 5 years.⁶ At the subnational level, regional and district commissioners are centrally appointed, while

⁶A separate, smaller group of MPs are appointed.

the district council is comprised of elected representatives (known as ward councillors) from each ward. Below the district, ward executive officers are civil servants, while ward councillors are elected politicians. Similarly at the village level, there is an elected village chairman, and an appointed village executive officer, who is effectively a civil servant (Lierl, 2014). Below the village level, there are semi-official party structures known as ten cells, which are formally separate from, but in practice often closely intertwined with, the state apparatus.⁷

IV. Malaria Control Programs in Tanzania

Malaria is a major contributor to ill health in sub-Saharan Africa, which accounts for 70 percent of total global malaria morbidity. Malaria control efforts have been progressively scaled up across Africa over the past 10 years, with particularly large increases in funding following the creation of the Global Fund for AIDS, TB, and Malaria and the US President's Malaria Initiative. One critical intervention that has been scaled up widely is the distribution of insecticide treated bed nets (ITNs) for prevention of malaria. ITNs have been well documented as a highly efficacious intervention in a number of clinical trials (Lengeler, 2004), leading Ministries of Health and donor partners to collaborate in scaling up coverage for the most at risk groups, mainly children under 5 and pregnant women. Large scale ITN distribution programs have been associated with reductions in mortality and morbidity in a number of settings (Pathania, 2014; Lengeler, 2004). ITN distribution programs in Tanzania started out with relatively small-scale pilots in the late 1990s in several districts, which were seen as highly effective in reducing malaria-related mortality and morbidity

⁷Despite these formal structures, the state is quite weak at lower levels in Tanzania. Tucker et al. (2010) observe that while “In theory, government in Tanzania extends down to the mtaa (neighborhood) in urban areas and kitongoji (village) in rural areas. These lowest rungs of government connect to the district through division and ward levels. In practice, organized local government tends not to exist below the district level. Rather, while local government officials, such as Ward Executive Officers and Village Executive Officers exist, they are not integrated into the district government, but are left to find their own solutions to local problems with the resources they can gather on their own initiative.”

(Schellenberg et al., 2001). In 2004, a national voucher scheme was created (Njau et al., 2009), but comprehensive national coverage was not achieved until the mass free distribution campaigns of 2008-2011.

This paper focuses on one of these mass distribution campaigns: the 2010-11 Universal Coverage Campaign (UCC). After several years of a voucher program which enabled subsidized access to nets for pregnant women and infants (the Tanzania National Voucher Scheme or TNVS), ITN coverage was still not reaching the policy goal of universal access, leading policymakers to consider free distribution, for which Tanzania won funding for from the Global Fund for AIDS, TB, and Malaria. In the first free distribution campaign, nets were given to all children under 5 (the 2008-09 under 5 catch up campaign). Subsequently, in 2010-2011, additional nets were distributed to cover all household members, by giving one net to each household for every sleeping space not already covered with an ITN.⁸ Demographic and Health Surveys show that these campaigns were associated with increases in ITN coverage, as Tanzania reached over 80 percent ITN ownership by 2011.

The procedures for national net distribution campaigns in Tanzania were first established during the 2008 under 5 “catch up” distribution campaign. Using donor funding, the Tanzanian government contracted NGOs to manage implementation. The NGOs trained local government officials, who in turn supervised community volunteers, who registered all households and then distributed the nets (Bonner et al., 2011). The nets themselves were manufactured by A to Z Textiles Ltd., a domestic Tanzanian manufacturing firm. The country was divided into zones, which were covered progressively starting in regions with the highest malaria prevalence. The manufacturer, A to Z Textiles, distributed the nets to the village level, where they were stored temporarily. Village Executive Officers then were in charge of selecting, training, and managing four village volunteers in identification, registration, and distribution of free net vouchers for all under 5s in the

⁸Technically the nets distributed in these campaigns are long lasting insecticidal nets, or LLINs, rather than ITNs

community, and oversight of net storage and distribution from the storage space to the distribution site. NGOs did social mobilization and information activities before and during the registration (such as theater, videos, and public meetings). Distribution took place over weekends, and caregivers were instructed to bring their vouchers to the distribution site, where they were exchanged for nets. One month after distribution, the Red Cross (or district health staff in areas where the Red Cross was not active) followed up at households with a campaign to encourage correct bed net use. Over 8.7 million nets were issued, and the total program cost was \$63 million (Bonner et al., 2011).

The modality of the Universal Coverage Campaign, which lasted from July 2010 to October 2011, was quite similar. Distribution and logistics were handled by the manufacturer and NGOs. Ward executives and village executives were again charged with identifying and supervising in each village four community volunteers, who over a five day period visited each home and registered each sleeping space⁹. The volunteers issued coupons for each space that warranted a net, which were redeemable at the distribution point during the three day campaign period. The Tanzanian Red Cross again conducted a hang up encouragement campaign approximately one week after distribution. In total, over 17 million nets were distributed. The total cost of the 2010 campaign was \$96.4 million, financed primarily by the Global Fund for AIDS, Tuberculosis and Malaria. (Renggli et al., 2013)

V. Research Design

Here I describe the data, the identification strategy, and estimation approaches that I use to identify the causal effects of national ITN distribution on citizens' views of their political leaders in Tanzania.

⁹According to Renggli et al. (2013), "A sleeping space was defined as any bed, sleeping mat or floor space that could be potentially covered by a net."

Data

The main data sources are the 2008-09 and 2010-2011 rounds of the National Panel Survey, a large multi-topic household survey conducted in Tanzania by the National Bureau of Statistics with support from the World Bank. 3,200 households were interviewed in 2008-09, and these households, plus any new households that split off from the original sample, were re-interviewed in 2010-2011. The main focus of the survey was household consumption, poverty, and agricultural production, but there was also a governance module, which was asked of one single randomly selected adult household member. Helpfully for this paper's identification strategy, the timing of survey visits were randomized. The 2008-2009 LSMS survey documentation notes that "within each zone, each district and each region were visited at 3 separate (randomly assigned) points during the year, so as to account for seasonal fluctuations." Communication with LSMS survey staff confirms that this randomization into early or later survey dates was maintained as much as possible in the 2010/2011 follow up survey.¹⁰ This randomization, combined with the staggered roll out of the bed net campaigns, enables an "as-if" random RD design. Dates of the ITN distribution campaigns in each zone are obtained from program documents provided by one of the implementing NGOs, Mennnonite Economic Development Associates, and from Bonner et al. (2011) and Renggli et al. (2013).¹¹

I use outcome variables from this governance survey module, which asked voters for their level of approval of 8 public officials: village chairman, village executive officer, ward executive officer, ward councilor, headmaster, extension officer, police officer, and member of Parliament. I create binary outcome variables for each political leader, excluding the sectoral functionaries (extension officer, headmaster and policeman), and create standardized versions of the continuous

¹⁰The "cycle" variable, which denotes whether a respondent was interviewed in the earlier or later survey cycle, correlates at 0.83 across the two rounds.

¹¹It is worth noting that randomization of survey dates is not required for the validity of the RD identification strategy, which is specifically designed to recover causal estimates of the effect of endogenous independent variables.

outcome variables (scale 1-4) for ease of interpretation. *Politician approval*, the principal conceptual outcome, is measured for five different leaders: village chairman, village executive officer, ward executive officer, ward councillor, and member of parliament.¹² Between 70 and 80 percent of respondents report that they either approve or strongly approve of these leaders in the 2010-2011 round of the NPS. The key independent variable is whether respondents or anyone in their household slept under a free net in the night before the survey (“*any free net*”). Variable definitions for the various measures of bed net ownership and use are provided in the appendix.

Variation in Access to ITNs

I exploit the variation in possession of a free bed net driven by whether the 2010-11 ITN distribution campaign happened before or after an individual was surveyed by the NPS. Thanks to the randomized roll out of the survey, in conjunction with the phased introduction of the ITN distribution campaign, I can compare citizens who were surveyed just prior to receiving a net with those who were surveyed just after receiving a net.

One data issue is that although we know the month and year of all survey interviews, we do not know the exact *day* on which the survey was conducted. The month and year is recorded but the survey day is erased for anonymity. Therefore I impute a date of the 15th day of the month to all observations. I define respondents as treated if the survey team arrived within 3 months (90 days) after the mass bed net distribution campaign (such that the *post* variable = 1), omitting the 15 days before and 15 days after the survey date to account for uncertainty about the exact survey day of the month¹³. Respondents who were surveyed within the 90 days before the ITN campaign arrived in their village are control (*post* = 0).¹⁴ Later I relax this assumption to test multiple definitions of

¹²The question is: “Overall, would you say you approve or disapprove of the job your [official] is doing?” (Possible responses are: Strongly approve, approve, disapprove, strongly disapprove, or don’t know). In the main specification I create a binary outcome variable by coding strongly approve and approve as 1, and strongly disapprove and disapprove as 0.

¹³In later robustness checks I include these 15 days before and after the inputted survey date.

¹⁴This bandwidth was chosen to maximize comparability with estimates from the 2008 National

the treatment variable (including with bandwidths of 30, 60, 90, 120, 150 and 180 days). A second data issue is that since the ITN campaign started in the southern portion of the country, taking the better part of a year to cover the whole country, there is, by construction, a higher percentage of “*post*” observations in southern and southern highlands zones. Therefore the main estimates are presented both with and without zone fixed effects.

Figure 1 demonstrates that the 2010 universal coverage campaign dramatically increased ITN ownership. Individuals surveyed after the campaign occurred in their community have dramatically higher rates of net ownership and use and are much more likely to report that a member of their household received a free net (or free ITN) than those interviewed before the campaign occurred.

I can identify the causal impact of the bed net distribution campaign under the assumption that respondents just before or just after the campaign are comparable in expectation. However, it is reasonable to think that individuals surveyed quite a long time before or long after the campaign might be different. To address such concerns, I only compare treated cohorts surveyed shortly before the campaign to control cohorts surveyed just after. Our main analysis focuses on a “bandwidth” of 90 days on either side of the campaign period, but results are highly robust to windows of as little as 30 days on either side of the campaign. Given the plausibly exogenous timing of the campaign’s arrival in a given district combined with the survey’s randomized timing, this allows us to recover causal estimates.

Estimation Strategies

In this section, I provide estimates of the effect of bed net distribution on the main outcome variable of interest: approval ratings of local government officials and political leaders. I first present results graphically, and then with reduced form and instrumental variables specifications. Figure 2 shows the graphical representation of the impact of ITN distribution on approval ratings for 5 political leaders: village executive officer, village chairman, ward executive officer, ward chairman, and Panel Survey and ITN campaign, which are discussed further below.

MP. In regression form, I use two approaches to estimate the effect of receiving on bed net on approval of local politicians. I first estimate the reduced form effects of the bed net distribution campaign by estimating the following OLS model:

$$Politician\ approval_{ic} = \delta post\ campaign_c + \xi_{ic}, \quad (1)$$

This model is analogous to the intent-to-treat (ITT) analysis of a randomized experiment.¹⁵. However, simply being surveyed after the ITN campaign does not predict perfectly receipt of a net. Therefore in a separate set of estimates, I use a post-campaign survey date as an instrument for receipt of a net. In this model, I estimate the effect of post-campaign survey date on net receipt in a first stage regression:

$$any_free_net_{ic} = \delta post\ campaign_c + \xi_{ic}, \quad (2)$$

Then we estimate a structural model using two-stage least squares (2SLS):

$$Y_{ic} = \beta any_free_net_{ic} + \varsigma_{ic}. \quad (3)$$

For the IV estimation to be valid, several conditions must hold. First, there must be a strong and highly significant relationship between the instrument (post campaign survey date, *post*) and the endogenous variable (*any_free_net*). As Figure 1 shows, being surveyed after the ITN campaign took place (*post*=1) increases net ownership by 30 to 40 percentage points for any definition of net ownership, receipt, or use. The resultant first stage *F* statistic, ranging from 19 to over 100, in all cases comfortably exceeding the threshold value of 10 (Staiger and Stock 1997). Second,

¹⁵Robust standard errors are clustered at the district level. In additional models we add zonal fixed effects, and a linear time trend and the time trend interacted with the *post* variable.

the exclusion restriction requires that our instrument only affects political opinions through the mechanism of exposure to the bed net campaign, which seems quite plausible given the role of survey date randomization in generating the discontinuity.

VI. Results

In this section I present the main finding: that malaria control efforts in Tanzania have resulted in substantial public opinion gains for local politicians. Graphical evidence and regression estimates are provided for each outcome of interest. The regression tables provide our reduced form and IV estimates. I then confirm the robustness of these estimates.

Main Estimates

The main results are that, in the reduced form specifications, village chairman, the ward councilor, ward executive, and MP all see significant, substantive increases in their approval ratings. The approval increase they all receive is on the order of 7 to 13 percentage points, from base approval rates of 70 to 80 percent. The effects are substantively large and highly significant: for village chairman, for example, I can reject that the coefficient equals zero with 99% confidence. With zone fixed effects, results are of the same magnitude for village chair and councillor, but are smaller and not significant for the village and ward executives, and the MP.

Panel C shows the continuous measure of approval (1-4, with 1 representing strong disapproval and 4 strong approval), for which I create a standardized measure of approval for ease of interpretation. Using this measure, there are large and highly significant increases in approval for all outcomes in the main specification (between 0.24 and 0.39 standard deviation increases). Panel D uses the panel dimension of the data and controls for the respondent's approval of the same leader in the 2008 survey. In the IV specifications (Panel E), the same patterns of significance are present (for all except the village executive officer there is a significant treatment effect), and the effect

sizes are 2 to 3 times larger. The positive estimates for village chairmen and councillor are the most robust results; they are significant at virtually all bandwidths, and in models which include zonal fixed effects, and in models which control for the running variable (“time since campaign”) and the interaction of this variable with the “post” (treatment) variable. These two positions are also elected political leaders, unlike the village and ward executives, which are appointed positions. Therefore I focus on these results in the discussion that follows.

Robustness Checks

This section relaxes several model assumptions to test the robustness of the findings presented above. Figure 3 shows balance graphically on pre-treatment variables (household consumption, age, gender, and urban residence), and appendix table A2 presents regressions showing that none of these are statistically significant differences at the discontinuity.

Next I present a series of placebo tests to ensure that the discontinuity does not reflect an artifact of survey timing. There is a natural placebo test which is generated by the structure of the distribution campaign. In each zone, there was a process of household registration which took place between two and eight months before the actual ITN distribution. We can also use the registration household visits as a “placebo” campaign to test whether the change in views represents the effect of contact with state officials or a generic signal that the state was taking some action (as in the registration campaign), versus the actual effect of bed net receipt. In contrast to the bed net distribution, we find no positive treatment effect of the registration campaign. In fact there is if anything a slightly negative effect of visits which do not result in a tangible benefit, although this is not significantly negative except for the ward executive officer (see Table A4.) I also generate placebo campaigns 100 and 200 days before the real campaign and 100 and 200 days after. In 25 total placebo tests (5 outcomes x 5 placebos) only two coefficients are statistically significant at $p < 0.05$.

Results are also robust to changing the bandwidth to 60 or 120 days (rather than 90) or to a

full year (see Table 4). Controlling for pre-treatment characteristics (gender, age, consumption quintiles, and urban residence), all results come through in reduced form and IV. Finally, although Figure 2 shows no apparent trend on either side of the discontinuity, I also control for the value of the running variable and its interaction with the treatment for village chair approval at a range of bandwidths (see Figure 6).

Heterogeneous Effects

In this section, I focus on heterogeneous effects according to issue salience, aspects of the political environment, demographic characteristics, and conclude with heterogeneity by time of follow up. The implications of these patterns of heterogeneity are then considered in light of unresolved issues in the existing literature in section VII.

Heterogeneity by issue salience

An important dimension that would be likely to interact with treatment if citizens were indeed evaluating politicians and updating their views of them based on the bed net program would be the salience of the issue of malaria control to them personally or to their family. A clear potential driver of salience is the severity of malaria infection or frequency of transmission at the time of net distribution. There is notable variation in transmission of malaria intensity across the country, ranging from relatively limited transmission in highland areas to extreme year-round endemicity in other zones. Using district level measures of malaria intensity created by Chaki et al. (2013), I create an indicator variable for whether a district is above or below median malaria intensity, and interact the “above median” intensity indicator with treatment. In Table 5 we see evidence that the treatment effect is markedly larger when malaria is a salient problem. The interaction is highly significant in the main specification, and somewhat attenuated in the zonal fixed effects model, although the effect is still significant ($p < 0.05$) for MP and close to significant ($p < 0.12$)

for village chair. Perhaps more strikingly, districts can be divided further by malaria prevalence, into samples where the malaria prevalence measure is less than 10, between 10 and 20, between 20 and 30, and above 40. Divided up in this way, and including zonal fixed effects, the pattern of increasingly large treatment effects as malaria incidence increases is clearly visible in Figure 4. For all officials except the ward councillor, a sharp upward trend in treatment effects is visible for the 30-40% prevalence and above 40% prevalence districts, while treatment effects are zero or even negative in the 0-10% prevalence districts.

Heterogeneity by time of follow up

The durability of politician gains resulting from program implementation is a key question. On one hand, if large and persistent gains result from programs such as this one, it raises the question as to why such systematic delivery of such items or services on a frequent basis is not particularly common in developing country settings which are democratic and where politicians are very focused on winning elections. On the other hand, if benefits fade over time, then the lack of consistent delivery of programs to voters make sense. What would be expected instead would be a pattern of energetic service delivery in the immediate runup to elections, coupled with lackluster services in most normal non-election periods. While all results in this paper are short run (in the sense that over the course of approximately one year the ITN distribution campaign visited all zones of Tanzania), we can still leverage variation in the time elapsed between the ITN campaign and the survey interview to see if there is any fadeout pattern visible in the data, or whether effects are roughly constant over time.

If we simply examine effects starting at 30 days post-campaign and expand the bandwidth in 15 days increments until we reach the main specification bandwidth of 90 days, it becomes clear that the effect fades over time. As figure 5 demonstrates, the effect sizes are quite large at 30 days (reaching 15 pp for the village chairman and 31 pp for councillor). Yet the magnitude of the effect has decayed notably by 90 days: the effect of the campaign on councillor approval, for example,

has decreased from 31 percentage points at 30 days to 13 percentage points at 90 days. This pattern is also present with zonal fixed effects, where all officials see large and highly significant at 30 days which are attenuated over time. Yet while the effects diminish, the effects on the ward councillor and MP are still significant at 180 days (see Appendix Figure 7).

While such dynamic effects of voter responsiveness have not been well-studied in developing country contexts, this pattern is quite consistent with the literature on the short run impact of campaign ads in American politics (Gerber et al., 2011), and with broader “myopic retrospection” findings, again from the US, which show that voters judge politicians only on economic growth in election years, rather than by averaging over the four years of a presidential term (Bartels, 2008; Huber, Hill and Lenz, 2012; Healy and Lenz, 2014).¹⁶ It is also consistent with Galiani et al. (2016), who find that in Honduras, the electoral effects of a cash transfer program are larger for the group that received a larger fraction of their payments closer to an election, holding total transfer amount constant.

Heterogeneity by socioeconomic status and demographics

This section considers theoretical predictions and empirical findings which suggest that citizen responsiveness to government actions is likely to vary by individual characteristics such as education, income, or gender. For example, Wantchekon (2003) finds that women are less susceptible to clientelist appeals and Baez et al. (2012) find that they respond more positively than men to Colombia’s CCT program. Stokes et al. (2013) find that increasing education, industrialization and economic growth undermined clientelism and vote buying in the United States and Great Britain, while Weghorst and Lindberg (2013) find that education is associated with being a swing voter in Ghana. Despite theoretical predictions that wealthier or more educated citizens would be more likely to reward politicians for specific actions (such as vote buying, clientelism, or programmatic

¹⁶See also Bechtel and Hainmueller (2011) who find that voter gratitude in Germany erodes over time; as well as Zucco (2013). This literature is reviewed in Healy and Malhotra (2013).

policies) we see no evidence of treatment interaction by per capita consumption levels, education, or gender (Table A5).¹⁷

Heterogeneity by partisan alignment of central and local government

A final dimension of heterogeneity relates to voters' ability to correctly attribute credit for policies and programs in contexts of divided government and/or federalism. Following an earlier literature on retrospective voting (Powell and Whitten, 1993), several studies have focused on the partisanship of elected officials at central and regional or local levels as a potential modifier of citizen responsiveness to government actions. Divided control of government between different parties can affect citizen response, because citizens may not completely understand which party deserves credit for the program, either because of they have limited information levels about policy details in general, or because policy responsibility is genuinely shared by multiple actors in a political system. For example, in Pop-Eleches and Pop-Eleches (2012), voters changed their opinion of local government (which implemented the program) but not of the responsible national politicians or the central government (which started and funded the program).¹⁸ The opposite pattern is present in Brazil, where Zucco (2013) finds that voters rewarded the president, but not legislative candi-

¹⁷We can also compare the effect of the 2008 Under 5 catch up ITN campaign to the 2010 universal coverage campaign. Like the 2010 campaign, the 2008 net distribution campaign also was contemporaneous with the a round of the National Panel Survey (the 2008-2009 round), meaning that the same RD identification strategy can be implemented, although the first stage treatment is notably smaller. The increase in respondents living in a household where someone used a free net the previous night increases by 13 percentage points rather than 44 percentage points in 2010. The increase is 22 percentage points when restricting analysis to households with children under 5, who were the target of the campaign. Interestingly, however, there is *no* political benefit to politicians in the aftermath of this campaign. There is no treatment effect whether we test this for the whole sample, or when we restrict the regression to households that had at least one child under 5 in 2008, or with a continuous measure of approval as the outcome variable, or when including controls or varying the bandwidth. However in addition to the weaker first stage, this comparison is not as well powered as that of 2010, since only 13 percent of respondents were interviewed after the campaign, compared to 49 percent after the 2010 campaign.

¹⁸Note however that the effect was driven by local governments who were aligned with the coalition in power at the national level.

dates, for *Bolsa Familia*, and in Uruguay, where voters do not reward parliament or local councils, just the President and the incumbent government. A different dimension of heterogeneity is highlighted by [Larreguy, Marshall and Trucco \(2015\)](#), who find that central government leaders gained in popularity from a land titling program, but local leaders lost support, since it undermined clientelist strategies. Partisan differences are also important to the theoretical framework of [Imai, King and Velasco Rivera \(2016\)](#), who argue that in cases where programs are supported by both (or all) relevant parties, such as Mexico’s Seguro Popular health reform and Progresa CCT program, there is no logical reason why voters would reward specific politicians (i.e. incumbents) rather than out-of-power parties who also support the program.

In Tanzania, the ruling party, CCM, has held power at the central level continuously since independence, and the vast majority of local (village-level) governments are also controlled by CCM. The ITN distribution was predominantly an initiative of the central government and international donors (although local leaders participated in implementation), so if voters are “correctly” attributing credit where it is due, there should be a reduced treatment effect in opposition controlled localities, since the opposition had no power in the central government or the Ministry of Health, and an increased treatment effect in CCM-controlled settings.

The National Panel Survey collected information on local political leaders, such as their bureaucratic or elected post and their partisan affiliation. This enables an examination of the interaction of treatment with villages where a) all recorded village level officials are from the ruling party (this represents 43% of villages), and b) villages where more than 50 percent of village officials listed on the community leader roster are from opposition parties (14% of villages). For these models, attention is restricted to village level leaders, since information about the partisan composition of higher levels of the political system (i.e. ward councils) is not available in the dataset. There is no evidence that citizens boost approval of CCM officials more than opposition, and similarly no evidence that they credit opposition leaders any less, even though the opposition had no clear responsibility for the program at higher policy levels (Table [A6](#)).

VII. Discussion and Conclusion

The question of whether voters change their views of political leaders in response to government programs is a central question in political science, and despite a growing literature, it remains an unresolved one. Similarly, the question of whether there are political ramifications to the large scale global health programs which have massively increased in scale over the past decade is largely unexamined in the public health literature. This paper demonstrates large effects to the delivery of a life-saving health intervention. It also identifies patterns of differential responsiveness which shed light on ongoing debates in the literature about the nature of this response. A first point of note is the substantively large political benefits for politicians as a result of the provision of a relatively low value preventive health product. Ex ante one might have expected that since the bed nets distributed cost less than \$10 (while the CCTs in question give transfers of between 25 and 50 percent of annual income), there would be no detectable treatment effect, or comparably much smaller effects, of net distribution.

A number of factors, identified in the literature as important modifiers of such effects, might contribute to this unexpected effect. Analysts such as [Harding \(2015\)](#), [Harding and Stasavage \(2014\)](#) and [Mani and Mukand \(2007\)](#) have argued that the tangibility or visibility of a benefit is a key factor enabling citizens to give credit to politicians. Mosquito nets are both visible on a daily basis (one sleeps under a net every night) and tangible.¹⁹ A related point may be the universality of the program. Because the epidemiological logic of the program required targeting *every household in the country*, Tanzania’s 2010-11 bed net campaign can be thought of as a high “treatment dose” compared to other programs such as CCTs which by design only reach the poor. A further contributing factor could be the ease of attribution in Tanzania, given the unified control of central and local government by CCM. Especially in the case of a program which is financed

¹⁹ Another potential factor, suggested by the literature, is reversibility: unlike fixed infrastructure such as roads or buildings, ITNs are a good that could be taken away, and so may engender greater loyalty from voters in order to forestall this ([Stokes et al., 2013](#)).

and planned centrally, but for which local officials are active in the front line operation, credit allocation could be difficult for voters if partisan control was divided between central and local government. This is rarely the case in Tanzania, which is strongly dominated by the ruling party at all levels of government.

Finally we document a significant fading of the effect over time. This is an intrinsically important pattern, since such dynamic effects, if clearly understood, must shape the calculations of politicians about the timing or frequency of such programs. But it may also help explain variation in the effects identified in this literature. This suggests that handouts to voters, whether clientelistic or as part of organized programmatic policies, are not an all purpose strategy for politicians. Delivered far in advance of elections, their effect could wear off completely and result in no particular benefit at election time. For example, an explanation for the null response to *Progresa* found by Imai, King and Velasco Rivera (2016) could be that effects are large in the short run but weaker in the long run. If this were true, comparison of a group that recently got the program to one that got it longer ago could produce a null result, not because the program did not affect voters, but rather because the group that got it longer ago (notwithstanding that they have received a higher “dose”) has already had some fadeout, while the newly treated group is in the middle of their bounce in approval rating for politicians.²⁰ Similarly, the apparent unresponsiveness of South African voters to infrastructure and sanitation improvements (De Kadt and Lieberman, 2015) could be because of the time dimension, since that analysis examines changes in infrastructure and service delivery over a 10 year period.²¹

To conclude, we note that it is commonly understood that governments do not deliver services effectively in developing countries because there are powerful political forces pushing politicians

²⁰This does not apply for the null result that they find for the *Seguro Popular* health program, which compared a treatment group to a completely untreated control group.

²¹However we note that Pop-Eleches and Pop-Eleches (2012) find significant results two years after the program, and Manacorda, Miguel and Vigorito (2011) find relatively small decrease in effect size between surveys conducted during the PANES cash transfer program and those conducted several months after the program was largely completed.

and bureaucrats to use resources for their personal gain, their political survival, or both. But what if there were also large political rewards to effective service delivery, at least for some classes of goods, or types of public services? Different political equilibria could emerge. The apparent incentive compatibility that is identified here between the interests of large, life saving global health programs and local politicians offers hope that under certain conditions, other paths are possible. Uncertainty about the nature and the magnitude of citizen reaction to effective programs may be one barrier to efforts in support of such programs on the part of political leaders. This paper quantifies the benefits to one such program in Tanzania, and in doing so may point to the way to a broader research agenda on the political returns to delivering critical life saving interventions. More work is needed to estimate the political benefits of other critical interventions, such as antiretroviral treatment for HIV/AIDS, that are high on the global health agenda but are too often underprovided in developing countries.

Table 1: Summary statistics

	Obs.	Mean	Std. dev.	Min.	Max.
Dependent variables					
Village chair approval	757	0.82	0.38	0	1
Village executive officer approval	569	0.83	0.37	0	1
Councillor approval	553	0.81	0.39	0	1
Ward executive approval	418	0.80	0.40	0	1
MP approval	551	0.75	0.44	0	1
Control variables					
female	757	0.58	0.49	0	1
age	749	40.0	15.2	16	95
urban	757	0.29	0.45	0	1
consumption per capita (Tsh)	745	809,007	704,072	72,744	6,960,015

Table 2: impact of campaign on net ownership

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	any ITN	slept w/ITN	ITN pct	any net	free net	free ITN	any free net
post	0.361*** (0.0387)	0.363*** (0.0432)	0.320*** (0.0489)	0.188*** (0.0449)	0.406*** (0.0488)	0.439*** (0.0449)	0.406*** (0.0409)
constant	0.430*** (0.0258)	0.348*** (0.0256)	0.335*** (0.0262)	0.714*** (0.0422)	0.253*** (0.0287)	0.163*** (0.0217)	0.373*** (0.0276)
N	1345	1345	1345	1345	1345	1345	1345

Robust standard errors clustered at district level

* p<.1, ** p<.05, *** p<.01

Table 3: impact of campaign on politician approval, 2010 survey

	(1) village chair	(2) village exec	(3) councillor	(4) ward exec	(5) MP
Panel A: Reduced form, binary approval (0-1)					
post	0.116*** (0.0308)	0.0677* (0.0363)	0.132** (0.0566)	0.0827 (0.0534)	0.132** (0.0559)
constant	0.760*** (0.0283)	0.797*** (0.0310)	0.740*** (0.0508)	0.749*** (0.0502)	0.677*** (0.0454)
N	757	569	553	418	551
Panel B: Reduced form with zone fixed effects					
post	0.101** (0.0386)	0.0201 (0.0513)	0.0995*** (0.0329)	0.00211 (0.0476)	0.0149 (0.0325)
constant	0.757*** (0.0488)	0.858*** (0.0575)	0.809*** (0.0347)	0.866*** (0.0467)	0.815*** (0.0372)
N	757	569	553	418	551
Panel C: Reduced form, continuous approval (standardized)					
post	0.336*** (0.0817)	0.241** (0.0963)	0.355** (0.142)	0.389*** (0.131)	0.340** (0.163)
constant	-0.153** (0.0668)	-0.0688 (0.0788)	-0.163 (0.115)	-0.172 (0.121)	-0.181 (0.114)
N	757	569	553	418	551
Panel D: Reduced form controlling for 2008 approval, binary approval					
post	0.0943*** (0.0287)	0.0575 (0.0364)	0.0803 (0.0495)	0.107** (0.0482)	0.115** (0.0473)
constant	0.663*** (0.0426)	0.771*** (0.0334)	0.742*** (0.0629)	0.646*** (0.0549)	0.605*** (0.0547)
N	755	567	418	553	551
Panel E: IV estimates (binary)					
any free net	0.267*** (0.0687)	0.145** (0.0733)	0.301** (0.122)	0.181 (0.116)	0.271*** (0.104)
constant	0.663*** (0.0517)	0.745*** (0.0546)	0.627*** (0.0950)	0.677*** (0.0934)	0.586*** (0.0759)
N	757	569	553	418	551

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 4: Robustness checks

	(1) village chair	(2) village exec	(3) councillor	(4) ward exec	(5) MP
Panel A: Reduced form with controls					
post	0.111*** (0.0287)	0.0443 (0.0320)	0.109** (0.0472)	0.0723 (0.0473)	0.125** (0.0483)
constant	0.660*** (0.0451)	0.787*** (0.0527)	0.831*** (0.0598)	0.811*** (0.0970)	0.708*** (0.0625)
N	737	554	540	404	531
Panel B: Reduced form with no 30 day omitted period					
post	0.104*** (0.0283)	0.0793** (0.0321)	0.106** (0.0509)	0.0930** (0.0460)	0.120** (0.0494)
Constant	0.761*** (0.0260)	0.797*** (0.0282)	0.754*** (0.0480)	0.742*** (0.0446)	0.694*** (0.0444)
N	972	755	719	534	698
Panel C: 60 day bandwidth					
post	0.107*** (0.0390)	0.0121 (0.0363)	0.153** (0.0569)	0.0739 (0.0515)	0.0923 (0.0567)
constant	0.767*** (0.0353)	0.832*** (0.0298)	0.760*** (0.0491)	0.767*** (0.0406)	0.711*** (0.0390)
N	497	363	347	258	355
Panel D: 120 day bandwidth					
post	0.105*** (0.0312)	0.0503 (0.0384)	0.133** (0.0557)	0.100* (0.0520)	0.123** (0.0508)
constant	0.770*** (0.0282)	0.787*** (0.0334)	0.739*** (0.0536)	0.737*** (0.0499)	0.680*** (0.0425)
N	1125	849	831	618	819

Notes: Controls in Panel A include age, gender, per capita consumption and urban residence. Robust standard errors are clustered at district level

* $p < .1$, ** $p < .05$, *** $p < .01$

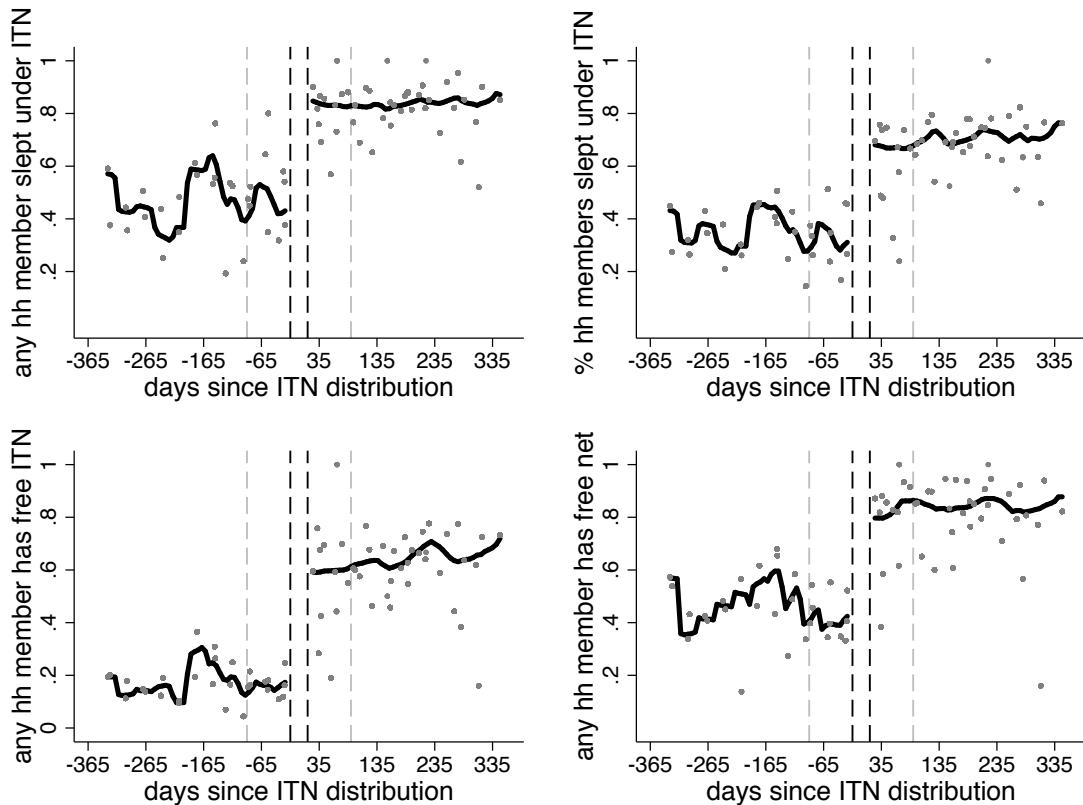
Table 5: Impact of the ITN campaign on politician approval by district level malaria prevalence

	(1) village chair	(2) village exec	(3) councillor	(4) ward exec	(5) mp
Panel A: effect in higher malaria prevalence districts					
post x high malaria	0.149*** (0.0553)	0.235*** (0.0603)	0.177* (0.0890)	0.0817 (0.0958)	0.256*** (0.0698)
high malaria	-0.134** (0.0505)	-0.181*** (0.0454)	-0.181** (0.0833)	-0.160* (0.0888)	-0.236*** (0.0585)
post	0.0399 (0.0448)	-0.0536 (0.0472)	0.0612 (0.0538)	0.0691 (0.0582)	-0.00841 (0.0610)
constant	0.823*** (0.0371)	0.875*** (0.0261)	0.820*** (0.0395)	0.822*** (0.0473)	0.797*** (0.0278)
N	741	560	540	412	536
Panel B: effect in higher malaria prevalence districts, zonal fixed effects					
post x high malaria	0.107 (0.0692)	0.132 (0.0964)	0.0404 (0.0887)	0.0965 (0.0943)	0.240*** (0.0889)
high malaria	-0.0691 (0.0655)	-0.0401 (0.0800)	-0.0399 (0.0543)	-0.201** (0.0882)	-0.238*** (0.0874)
post	0.0378 (0.0699)	-0.0676 (0.0858)	0.101* (0.0582)	-0.0290 (0.0712)	-0.130** (0.0603)
constant	0.782*** (0.0774)	0.862*** (0.0696)	0.808*** (0.0335)	0.967*** (0.0893)	0.940*** (0.0622)
N	741	560	540	412	536

Robust standard errors clustered at district level in parentheses

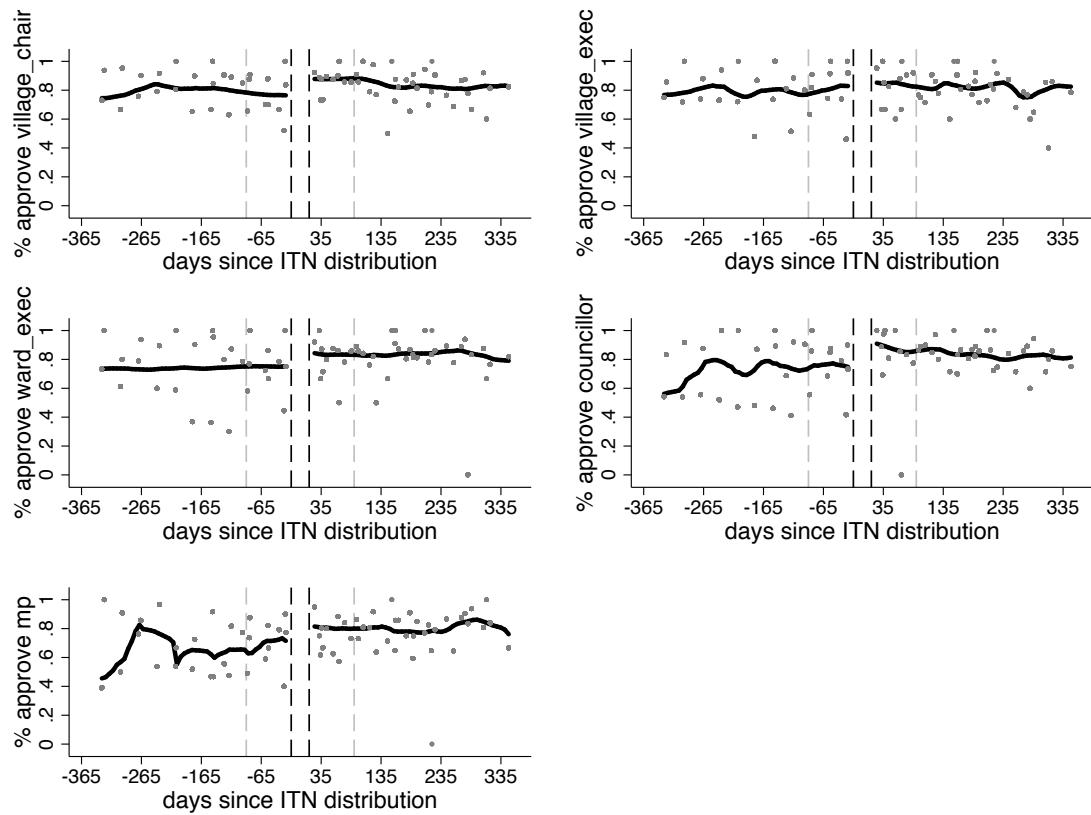
* $p < .1$, ** $p < .05$, *** $p < .01$

Figure 1: ITN access by survey date



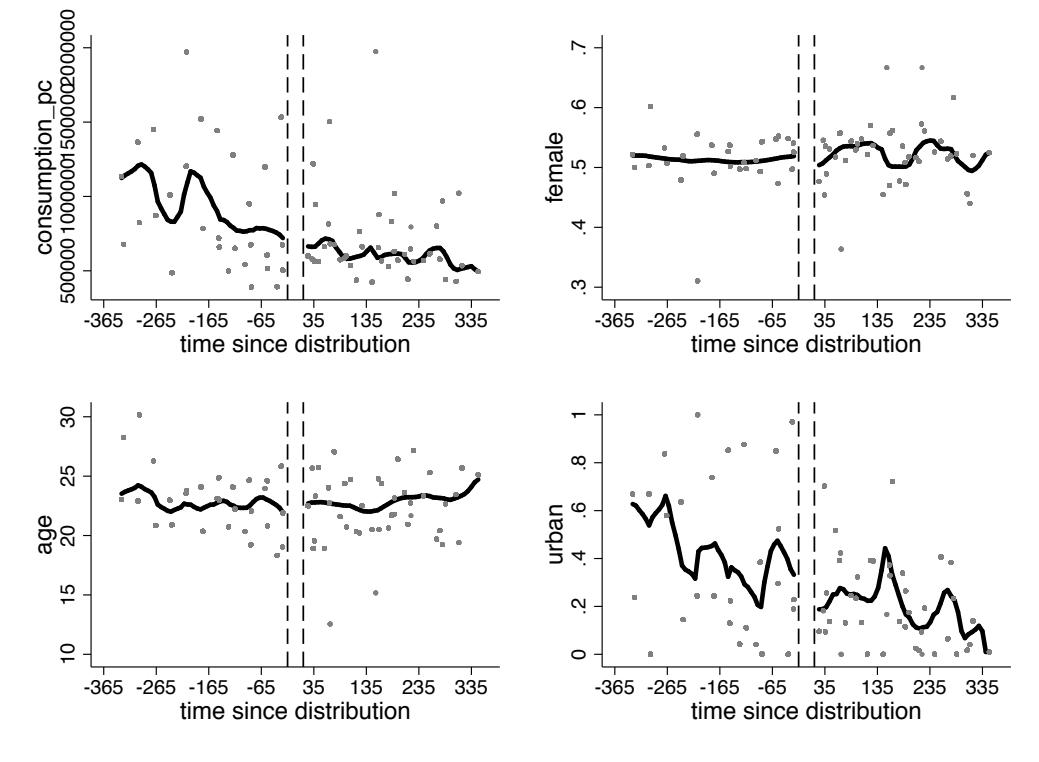
Notes: Each gray dot represents average ITN ownership or use for a given value of the “days since ITN distribution” variable (survey date minus campaign date). Black lines are local polynomials fitted either side of the survey date (indicated by the vertical dashed line). The vertical gray dashed lines indicate the bandwidth used for the main analysis. Variable definitions are provided in the appendix. The x-axis shows the number of days elapsed between respondent’s interview date and the date of ITN campaign in the respondent’s zone.

Figure 2: Politician approval by survey date



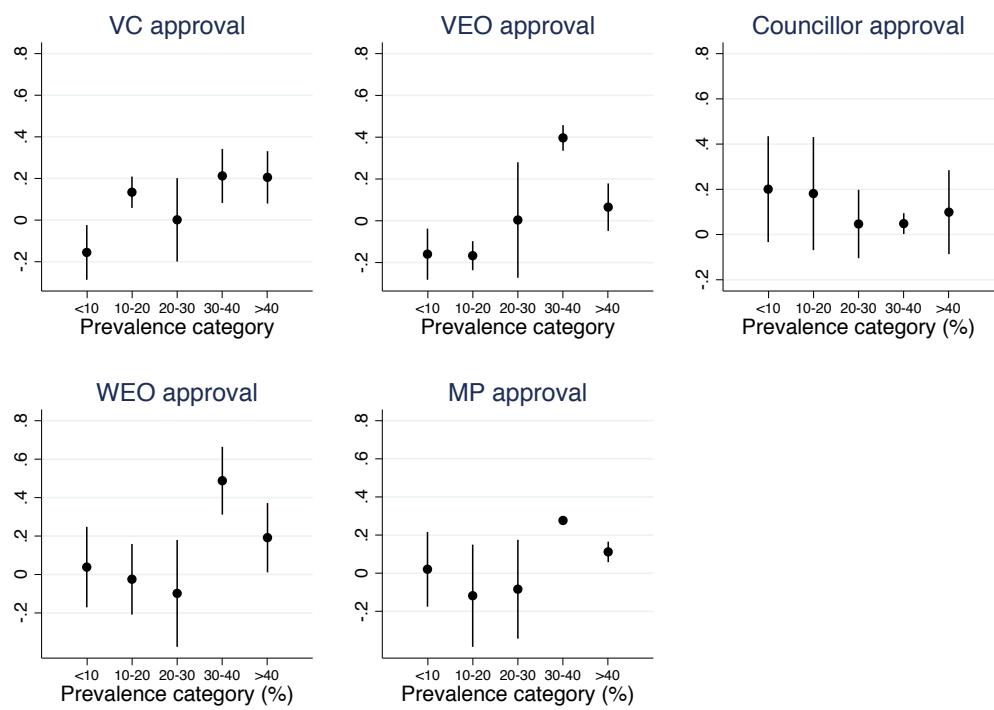
Notes: See Figure 1.

Figure 3: Covariate value by survey date



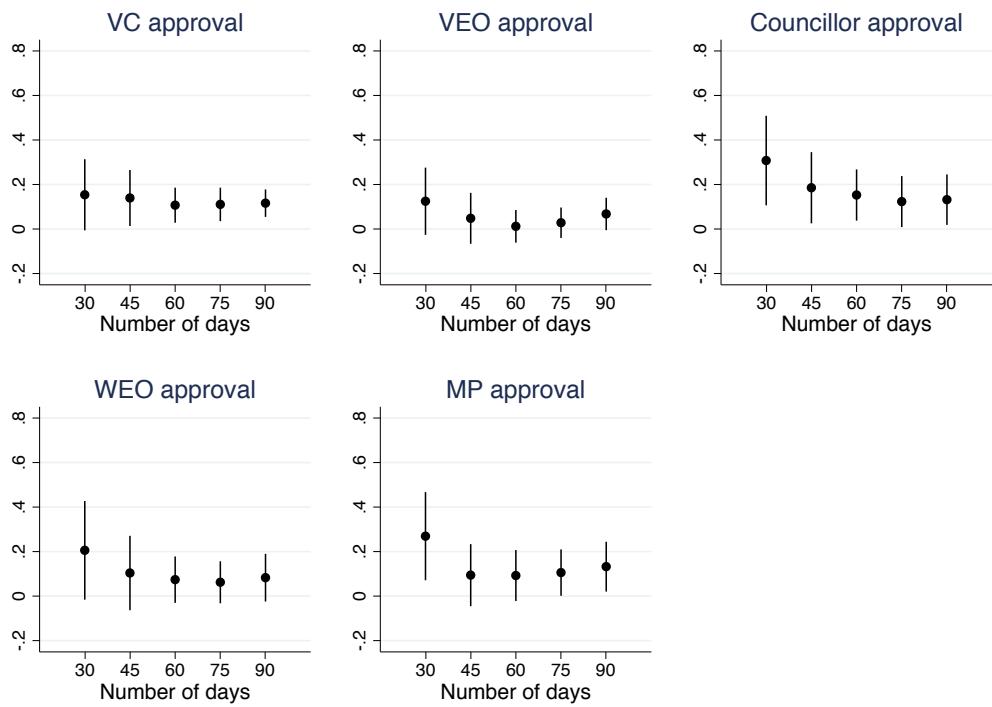
Notes: See Figure 1.

Figure 4: Treatment effects by malaria prevalence



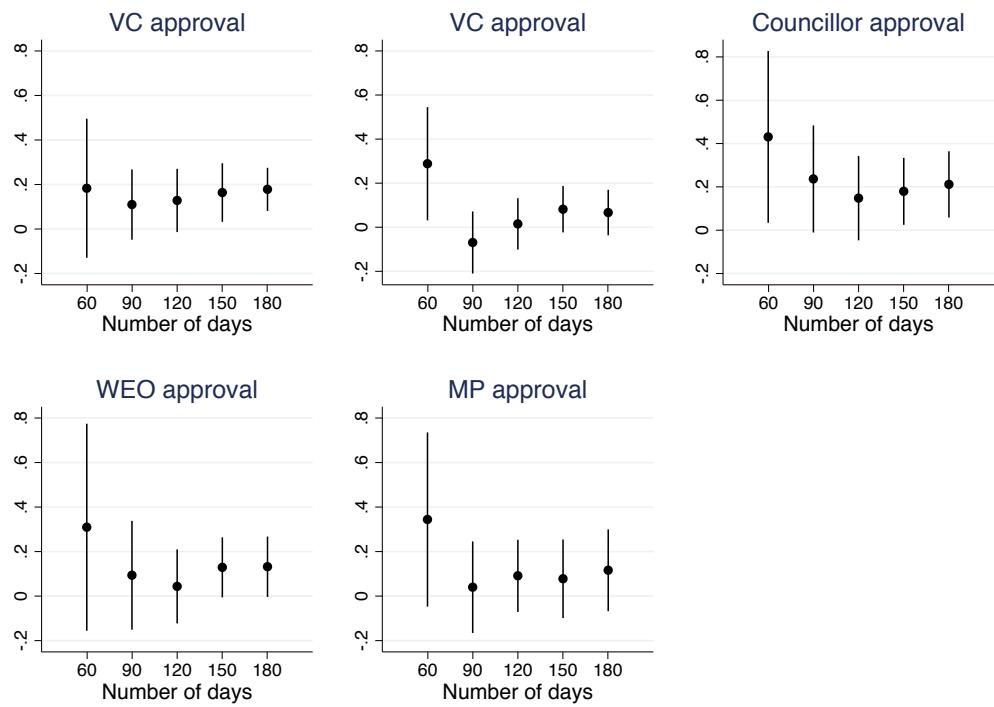
Notes: Plots represent treatment effects for each of the officials, by category of malaria prevalence (0-10%, 10-20%, 20-30%, 30-40%, and >40% prevalence), based on district level malaria prevalence in Chaki et al. (2013). Zonal fixed effects are included in all regressions.

Figure 5: Politician approval by time since campaign date



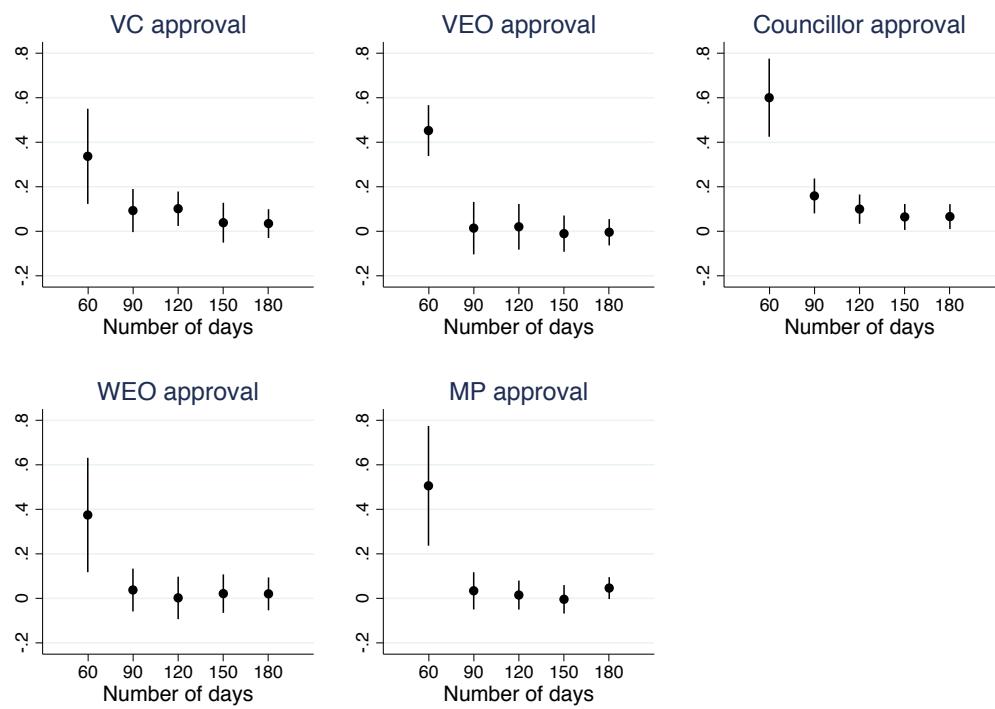
Notes: Unadjusted treatment effects. No time trends, controls or fixed effects included in regressions.

Figure 6: Politician approval with time trend, by bandwidth



Notes: Regressions include linear time trend and linear time trend interacted with the variable indicating post campaign survey date.

Figure 7: Politician approval with zonal fixed effects, by bandwidth



Notes: Regressions include zonal fixed effects but are otherwise unadjusted.

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Variable definitions

The following variables all come from the 2008-2009 and 2010-2011 National Panel Surveys, Chaki et al. (2013), or Renggli et al. (2013).

post. This is the main treatment variable. Coded 1 if the respondent was interviewed after the bed net distribution campaign took place in the respondent's zone of residence.

any ITN. Coded as 1 if any household member slept under an ITN the previous night

slept w/ITN. Coded as 1 if the respondent slept under an ITN the previous night

ITN_pct. Percentage of household members who slept under an insecticide treated net (ITN) the previous night.

any_net. Coded as 1 if any household member slept under a bed net (ITN or untreated) the previous night.

free net. Coded as 1 if the respondent slept under a bed net (ITN or untreated) the previous night that they had received for free.

any free net. Coded as 1 if any member of the household slept under a bed net (ITN or untreated) the previous night that they had received for free.

free ITN. Coded as 1 if any member of the household slept under an ITN the previous night that they had received for free.

Politician approval variables: *village chair, village_exec, weo, councillor, MP.* The main approval variables are binary indicators; they are coded as 1 if the respondent reports that he or she approves or strongly approves of the leader and 0 if the respondent disapproves or strongly disapproves. A continuous approval variable is also created, which is a standardized transformation of the 1-4 (strongly approve, approve, disapprove, strongly disapprove) approval rating variable.

opposition. Coded 1 if more than 50 percent of village level leaders listed on the village leaders roster in 2010 were opposition party members (opposition parties are CHADEMA, CUF, or “other party”).

ruling party. Coded 1 if all village level leaders listed on the village leaders roster in 2010 were CCM party members.

high_malaria. Indicator coded 1 if the predicted population-adjusted parasite prevalence in children aged 2-10 years as of 2000 is above the median for all districts. (The median in 2000 was 23.15). Estimates of malaria prevalence by district are taken from Chaki et al. (2013). This source provides malaria estimates for 102 districts (this incorporates urban councils and districts in 14 cases). In cases where an urban council is conflated with its rural “pair” district I follow the source by assigning the same malaria prevalence to both entities. National Panel Survey enumeration areas are plotted onto districts using the shape files obtained from Tanzania National Bureau of Statistics (2012). In cases where districts have split between the publication of the shapefile and district classification used by Chaki et al. (2013), districts are assigned the malaria prevalence of their parent district as listed by Law (2012).

consumption_pc. Household consumption per capita, in 2010 Tanzanian Shillings.

urban. Indicator coded 1 if respondent household is located in an urban area.

female. Indicator coded 1 if respondent is female.

Table A1: impact of campaign on politician approval, 2008 round

	(1) village chair	(2) village exec	(3) councillor	(4) ward exec	(5) MP
Panel A: Reduced form					
post	-0.0332 (0.0413)	0.0110 (0.0550)	0.0428 (0.0497)	0.0179 (0.0493)	-0.0586 (0.0580)
constant	0.787*** (0.0252)	0.646*** (0.0414)	0.583*** (0.0368)	0.525*** (0.0382)	0.570*** (0.0449)
N	533	533	533	533	533
Panel B: Reduced form with controls					
post	-0.0206 (0.0409)	0.0374 (0.0531)	0.0531 (0.0495)	0.0403 (0.0485)	-0.0647 (0.0561)
constant	0.821*** (0.0516)	0.731*** (0.0697)	0.586*** (0.0597)	0.589*** (0.0654)	0.489*** (0.0818)
N	533	533	533	533	533
Panel C: IV estimates					
ITN receipt	-0.111 (0.140)	0.0369 (0.182)	0.143 (0.162)	0.0599 (0.162)	-0.196 (0.191)
constant	0.793*** (0.0304)	0.644*** (0.0481)	0.575*** (0.0432)	0.522*** (0.0448)	0.581*** (0.0524)
N	533	533	533	533	533

Robust standard errors clustered at district level

* $p < 0.1$, ** $p < .05$, *** $p < .01$

Table A2: covariate balance between respondent pre and post campaign

	(1)	(2)	(3)	(4)
	consumption_pc	female	age	urban
post	-60416.1 (178707.2)	-0.00233 (0.0382)	0.0117 (1.765)	-0.157 (0.114)
constant	925771.5*** (151239.3)	0.588*** (0.0312)	39.35*** (1.017)	0.419*** (0.0985)
N	894	915	907	915

robust standard errors clustered at district level

* p< .1, ** p< .05, *** p< .01

Table A3: impact of post-campaign survey date on missing response

	(1)	(2)	(3)	(4)	(5)
	village chair	village exec	councillor	ward exec	MP
post	-0.0330 (0.0691)	-0.0252 (0.0779)	0.00390 (0.0635)	-0.108* (0.0603)	-0.000680 (0.0485)
constant	0.190*** (0.0385)	0.391*** (0.0464)	0.394*** (0.0382)	0.600*** (0.0307)	0.398*** (0.0318)
N	915	915	915	915	915

robust standard errors clustered at district level

* p< .1, ** p< .05, *** p< .01

Table A4: Placebo campaigns

	(1) village chair	(2) village exec	(3) councillor	(4) ward exec	(5) MP
Panel A: registration event placebo					
registration placebo	-0.0208 (0.0312)	-0.0152 (0.0474)	-0.0445 (0.0577)	-0.0927** (0.0456)	0.0521 (0.0583)
Constant	0.823*** (0.0283)	0.839*** (0.0305)	0.792*** (0.0375)	0.807*** (0.0350)	0.681*** (0.0550)
N	624	463	432	318	426
Panel B: 100 day placebo					
100 days placebo	0.0163 (0.0299)	0.0229 (0.0360)	0.0755 (0.0479)	0.0493 (0.0390)	0.0804* (0.0432)
constant	0.812*** (0.0203)	0.821*** (0.0258)	0.768*** (0.0416)	0.774*** (0.0324)	0.701*** (0.0344)
N	2135	1633	1601	1159	1601
Panel C: 200 day placebo					
200 days placebo	0.000328 (0.0400)	-0.0258 (0.0429)	-0.0419 (0.0450)	0.0364 (0.0346)	-0.000432 (0.0401)
constant	0.820*** (0.0258)	0.838*** (0.0278)	0.843*** (0.0276)	0.824*** (0.0264)	0.809*** (0.0270)
N	555	476	474	362	481
Panel D: 100 day prior placebo					
100 days prior placebo	-0.0394 (0.0430)	0.0444 (0.0411)	0.0207 (0.0714)	0.0443 (0.0562)	0.0685 (0.0569)
constant	0.813*** (0.0378)	0.792*** (0.0462)	0.758*** (0.0664)	0.742*** (0.0655)	0.649*** (0.0516)
N	632	454	463	317	466
Panel E: 200 day prior placebo					
200 days prior placebo	-0.0349 (0.0277)	-0.0472 (0.0356)	-0.0451 (0.0429)	-0.0717 (0.0608)	-0.155*** (0.0563)
constant	0.843*** (0.0312)	0.836*** (0.0430)	0.795*** (0.0668)	0.812*** (0.0513)	0.798*** (0.0550)
N	492	353	374	242	371

robust standard errors clustered at district level

* p < .1, ** p < .05, *** p < .01

Table A5: impact of campaign on hhs by gender, education, and income, with zonal fixed effects

	(1) village chair	(2) village exec	(3) councillor	(4) ward exec	(5) MP
Panel A: post-campaign survey x gender					
post x female	-0.0328 (0.0496)	0.0106 (0.0641)	0.0324 (0.0598)	0.0246 (0.0726)	-0.0748 (0.0706)
post	0.124** (0.0498)	0.0223 (0.0641)	0.0852* (0.0462)	-0.00928 (0.0572)	0.0567 (0.0467)
female	0.0991** (0.0445)	0.0689 (0.0462)	0.00655 (0.0496)	-0.0268 (0.0658)	0.109* (0.0593)
constant	0.700*** (0.0572)	0.816*** (0.0646)	0.804*** (0.0431)	0.878*** (0.0581)	0.758*** (0.0466)
N	757	569	553	418	551
Panel B: post-campaign survey x years of school					
post x years school	-0.00381 (0.00719)	-0.0175** (0.00804)	-0.0000406 (0.00669)	0.000623 (0.00829)	-0.00609 (0.00978)
post	0.131** (0.0545)	0.107 (0.0654)	0.101* (0.0535)	0.00499 (0.0667)	0.0375 (0.0640)
years school	-0.0101* (0.00590)	0.00548 (0.00593)	-0.00128 (0.00476)	-0.00342 (0.00614)	0.00174 (0.00873)
constant	0.807*** (0.0560)	0.830*** (0.0640)	0.813*** (0.0493)	0.876*** (0.0532)	0.811*** (0.0551)
N	737	552	536	403	533
Panel C: post-campaign survey x above median per capita consumption					
post x consumption	-0.0383 (0.0433)	0.00573 (0.0599)	-0.0441 (0.0574)	0.118 (0.0962)	-0.117 (0.0715)
post	0.114** (0.0433)	0.0171 (0.0562)	0.116*** (0.0433)	-0.0280 (0.0557)	0.0837* (0.0440)
above median consumption	0.00211 (0.0338)	-0.0461 (0.0524)	-0.0148 (0.0413)	-0.172** (0.0749)	0.0204 (0.0617)
constant	0.757*** (0.0505)	0.874*** (0.0613)	0.814*** (0.0414)	0.906*** (0.0577)	0.810*** (0.0432)
N	745	561	546	410	538

Robust standard errors clustered at district level

* p<.1, ** p<.05, *** p<.01

Table A6: impact of campaign in CCM vs opposition villages

	(1) village chair	(2) village chair	(3) village exec	(4) village exec
post x ruling party	0.00874 (0.0689)	0.0148 (0.0746)	-0.0391 (0.0661)	-0.0446 (0.0739)
post	0.100** (0.0488)	0.127* (0.0672)	0.0832 (0.0594)	0.0826 (0.0731)
ruling party	-0.0182 (0.0612)	-0.0449 (0.0568)	0.0157 (0.0592)	0.0338 (0.0679)
constant	0.783*** (0.0458)	0.732*** (0.0638)	0.793*** (0.0548)	0.828*** (0.0722)
zonal fixed effects	no	yes	no	yes
N	624	624	485	485
post x opposition	0.0265 (0.0867)	-0.000860 (0.0808)	0.180 (0.176)	0.218 (0.157)
post	0.107*** (0.0342)	0.131 (0.0784)	0.0557 (0.0373)	0.0376 (0.0804)
opposition	-0.0333 (0.0801)	0.00403 (0.0715)	-0.191 (0.162)	-0.229 (0.144)
constant	0.775*** (0.0307)	0.728*** (0.0736)	0.816*** (0.0303)	0.880*** (0.0844)
zonal fixed effects	no	yes	no	yes
N	624	624	485	485

Standard errors in parentheses

* p< .1, ** p< .05, *** p< .01