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Cash for Votes: Evidence from India

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Abstract

This paper investigates the prevalence of vote-buying in democratic elections where stringent restrictions on corporate donations to political parties exist. We combine data from state assembly elections in India with household-level consumer expenditure surveys (conducted by NSSO) over the period 2004-11. Exploiting a difference-in-differences methodology, we estimate the effects elections have on the consumption of various household items: food, clothes, education-related, etc. Moreover, there is heterogeneity in such consumption adjustments across households. Our estimates suggest that legal sources of funds are not sufficient for generating such "spikes" in consumption and indicate the role of the hidden economy in politics.

JEL codes: D12, D72, H40.

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Non-technical summary

Campaigning in elections is costly. In countries without public funding of election campaigns, such financing necessarily relies on donations by private corporations and individuals. If, furthermore, such donations are subject to restrictive legal limits – which is often the case in several developing nations – then it suggests that election campaigns must be frugal. However, in reality elections in such contexts are rarely low-key: there exists ample anecdotal evidence of voters being bribed with cash or actual consumption goods prior to elections. The media often reports on cash seized during various elections. For example, in India the amounts ranged from 19.5 million INR (about 0.3 million US$) in the eastern state of Assam to 155 million INR (about 2.4 million US$) in the southern state of Tamil Nadu during the 2014 parliamentary election. There is, however, a clear lack of hard evidence of the extent and form of vote-buying. This is unsurprising, largely because neither political parties nor voters have any incentives for revealing any details regarding the cash (and "kind") that changes hands.

In this paper, we propose a methodology to empirically assess the nature and extent of vote-buying using data on elections from all the major Indian states. Our approach to the problem is novel: we look at the *consumption patterns* of households and examine how they vary before and after elections. The idea is to capture the actual change (presumably, rise) in expenditure by the voters as a result of any cash transfer they might receive from the campaigning parties.

Our two key sources of data are the National Sample Survey (NSS) rounds on household consumption expenditure, conducted during 2004-2012, and state assembly elections data for that period. Each NSS consumption module contains detailed information on the surveyed households' monthly consumption expenditure on over 300 different commodities. Each of these survey rounds takes a year to complete and covers all states. For every surveyed household we have information on the date of the survey. Combining this with the data on state assembly elections, we are able to ascertain whether a household is reporting on consumption close to elections. Given that in a particular year only some states have elections, we have a sample with different groups: there are households that reported their consumption just a few days before they voted and those that did so many days before or after voting. In fact, we construct 'time windows' of different lengths prior to election dates to see how the consumption pattern changes. We compare these groups with the 'reference group', which comprises households in neighbouring (non-election) states that were surveyed on similar dates. In this manner, we tackle the main challenge regarding identification since the timing of surveys is *independent* of that of state assembly elections.

We find that households tend to spend more on a range of staples, and, to an extent, on 'intoxicants'. The expenditures on education-related items (books, school uniforms, etc.) increase too. Moreover, the effects are quite substantial. Take the case of pulses: there is an increase in consumption of pulses worth around 50 INR per-capita for households surveyed close to election dates. Given that the average per-capita monthly spending on pulses is around 460 INR, this implies about a 10% increase. These "spikes" disappear with (chronological) distance from elections. Using our estimates, the approximate monetised value of the consumption spikes in a district on average turns out to be 2,900 million INR. This figure, when aggregated over a 5–year period (to allow for *all* states to have elections) comes to around 9% of India's GDP. These estimates are too substantial to be explained by legal public spending and indicates the presence of the "black economy" in Indian elections.

1 Introduction

Elections are a hallmark of any well-functioning democracy. But for any political party, participating in a nation-wide election requires significant amount of funds: there is the issue of campaigning where the political parties and their candidates attempt to "reach out" to the voters. So how are these campaign funds generated? The answer varies from country to country. In countries where norms regarding election financing are ambiguous, and/or legal limits on corporate donations to political parties are markedly small, the issue of mobilising sufficient funds by contesting parties assumes a certain degree of complexity. This is not to say that considerable funds are *not* generated; there exists ample anecdotal evidence of voters being bribed with cash or actual consumption goods prior to elections. The media often reports on cash seized during various elections. For example, in India the amounts ranged from 19.5 million INR (about 0.3 million US$) in the eastern state of Assam to 155 million INR (about 2.4 million US$) in the southern state of Tamil Nadu during the 2014 Lok Sabha election.[2]

There are several country-specific studies (e.g., Gingerich (2010) on Brazil, Eggers and Hainmueller (2009) on Britain and Akhmedov and Zhuravskaya (2004) on Russia), which look at the connection between election financing and corruption. In the case of India, Kapur and Vaishnav (2013) show that firms in the construction sector face short-term liquidity crunches during elections, reflected in lower level of activity in building and construction. However, this lull disappears post-elections, suggesting that the finances in this sector may be used for vote-buying.[3] Clearly, the idea of vote-buying goes against the very notion of a "free and fair" election. While it is acknowledged that vote-buying goes on quite unabashedly in many developing countries, there is a clear lack of hard evidence of the extent and form of vote-buying. This is unsurprising, largely because neither political parties nor voters have any incentives for revealing any details regarding the cash (and "kind") that changes hands. The lack of transparency in this matter is especially acute in developing countries like India where political parties lack any recourse to public funds for contesting elections. Furthermore, this creates incentives for opportunistic business agents to both influence election outcomes and enjoy perks by forming clientelistic relations with politicians and political parties. It is plausible that some of the money used in these contexts arises from donations, which forms the 'black money' stocks in the country. Given that the legal limits are low enough to be binding in most cases, opportunistic donors find it profitable to use their black money holdings to back their favoured party/parties.

In this paper, we tackle some of these issues head-on. First, we propose a methodology to empirically assess the nature and extent of vote-buying using data on elections from all the major Indian states. Our approach to the problem is novel: we look at the *consumption patterns* of households and examine how they vary before and after elections. The idea is

[2] Lok Sabha is the lower house of Parliament in India.

[3] We defer a more detailed treatment of the related literature to a different section.

to capture the actual change (presumably, rise) in expenditure by the voters as a result of any cash transfer they might receive from the campaigning parties. Our exercise enables us to identify *which* particular commodities are susceptible to such election-related cash flows. Relatedly, we identify considerable heterogeneity in expenditure patterns based on differences in household-level incomes. Next, we provide a simple theoretical model to interpret these differences in patterns of expenditure by the various income classes. Finally, based on the estimates from our regressions we provide a rough measure of the "black money" operating in elections.[4]

We start with the question: *what do people spend their election-related cash transfer on?* After all, households consume a variety of items on a daily basis. Moreover, this is not just any cash transfer: it is a form of bribe and this fact is evident to both the donor and recipient. So where does one start? We pay particular attention to items highlighted in the anecdotal evidence, namely, liquor, meat, and items of clothing like saris. We also look at expenditure items such as health and education, which can be considered as having the potential to affect an individual's productivity and hence might show a different response (if any). These items conceptually are a benchmark against which one can evaluate the movement in expenditure of the "pure" consumption items (as opposed to those having a functional role like enhancing productivity) like liquor and clothes.

A key challenge confronting us is this: how can one be sure that the comparison of consumption patterns of households before and after elections actually reflects the role of elections? What if other factors systematically change, which confounds the causal link from elections to change in expenditures? Our empirical strategy takes cognisance of this matter.

Our two key sources of data are the National Sample Survey (NSS) rounds on household consumption expenditure, conducted during 2004-2011, and state assembly elections data pertaining to that period. Each NSS consumption module contains detailed information on the surveyed households' monthly consumption expenditure on over 300 different commodities. Additionally, each of these survey rounds takes a year to complete and covers all states. For every surveyed household we have information on the date of the survey. Combining this with the data on state assembly elections, we are able to ascertain whether a household is reporting on consumption close to elections. Given that in a particular year only some states have elections, we have a sample with different groups: there are households that reported their consumption just a few days before they voted and those that did so many days before or after voting. In fact, we construct 'time windows' of different lengths prior to election dates to see how the consumption pattern changes. We compare these groups with the 'reference group', which comprises households in neighbouring (non-election) states that were surveyed on similar dates. In this manner, we tackle the main challenge regarding identification since the timing of surveys is independent of that of state assembly elections.

[4]It is tempting to call this aggregate measure an unbiased estimate of the black economy in the country. However, only a fraction of the black economy makes itself salient in elections; non-negligible portions of it are active in other forms which are unrelated to elections. Therefore, our estimate is clearly a lower bound.

Our prior here is that if there is no vote-buying, then the timing of election should not matter for patterns in consumption of any commodities. Our first significant observation is that household consumption is *indeed* impacted by elections. This, by itself, suggests that vote-buying is commonplace in India. We find that households tend to spend more on a range of staples, and, to an extent, on 'intoxicants'. The expenditure on education-related items (books, school uniforms, etc.) shows an significant increase too. When viewed as shares (as opposed to absolute expenditures), there is a similar pattern for these items (clothes, pulses and education). In fact, the effects are sharper when viewed as ratios for items like medical expenses and local liquor. Moreover, the effects are quite substantial. Take the case of pulses: there is an increase in consumption of pulses worth around 50 INR per-capita for households surveyed close to election dates. Given that the average per-capita monthly spending on pulses is around 460 INR, this implies about a 10% increase.[5]

We also attempt to uncover which parts of the income distribution drive the different results. For that purpose, we look at some specific socio-economic groups separately. For the landless population, there is a muted effect overall (which could arise from them being wooed to a lesser extent); however, the effect for medical-related expenses is as strong as in the overall sample. For households in the bottom income quintile too there is some reduction in terms of the spikes for clothes, although here the effect on education-related expenditures is quite similar to that in the overall sample.

This begs the question as to why the consumption adjustments differ by income classes. What is the underlying mechanism driving these asymmetries? To ground our results within a standard theoretical framework and enhance our understanding, we develop a simple model where all consumption items are classified into one of the two groups. In the first group, the actual utility from consuming the item is dependent on the level of public goods/services in the area (think of items that increase the health, human capital and in general productivity of the individual). The enjoyment of the goods in the other category is purely "private" in the sense that it bears no relation to the public goods/services available to the individual consumer. Moreover, the payoff from consuming the first category of goods is higher the greater the amount of public goods; also, this positive link weakens (after some minimum level of income) as an individual gets richer since they can substitute to private options rather than relying on public ones. In such a simple setup devoid of any behavioural biases, we show the following: poorer individuals tend to disproportionately increase spending on goods which are complementary to public service provision. The others tend to spend disproportionately more on pure consumption items which are independent of the level of public goods and services. Also, these expenditure distortions should be attenuated in areas which receive more cash inflows (say e.g., in politically competitive or "swing" districts).

We perform several additional exercises: in particular, we examine whether these spikes are affected by whether the district is electorally "swing" or not. Our theory suggests that the

[5] See Table 1 and Figure 1 for more details.

spikes and more importantly the divergent patterns across socio-economic groups should be amplified in such swing areas. Indeed, we find corroboration of that in our empirical analysis. For several items which exhibit an increase in consumption expenditure, higher political competitiveness re-enforces the baseline effect. In other words, in areas where the winner had an easy victory, the pre-election spikes were smaller. Using estimates from our baseline regressions, the approximate monetised value of the consumption spikes in a district on average turns out to be 2,900 million INR. This figure, when aggregated over a 5–year period to allow for *all* states to have elections, comes to around 9% of India's GDP. We conjecture that a substantial part of this comes from the "black economy" given the stringent legal restrictions on campaign-spending in India. That said, this is only the part of the hidden economy *which is salient in elections*; the actual black economy is possibly much larger.[6]

The rest of the paper is organised as follows. Section 2 offers a discussion of the related literature while Section 3 contains the main empirical analysis. Section 4 provides the theoretical model, Section 5 contains some additional robustness checks and Section 6 discusses a natural extension of our analysis; Section 7 concludes. The appendix contains some additional regression tables and all the theoretical proofs.

2 Related Literature

Our work aims to contribute to the broad literature on election financing, political competition, and corruption concerning public funds usage. Furthermore, by virtue of documenting the variations in consumption expenditure patterns around elections, our paper also relates to various studies on micro-level consumption behaviour in developing countries — be it in the context of nutrition or health investments or conspicuous consumption/'status goods'.

On the topic of election financing, Pinto-Duschinsky (2002) begins with: "Democratic elections and democratic governance involve a mixture of high ideals and, all too often, dubious or even sordid practices. Election campaigns, political party organizations, pressure groups, and advertising all cost money. This must be found from somewhere. The financing of political life is a necessity – and a problem." Pinto-Duschinsky (2002) offers an interesting overview of the issue of financing of political campaigns, corruption involving campaign contributions, etc. across a set of countries. Scarrow (2007) assesses attempts to study the effect of money in politics in democracies other than the United States. There are some within-country studies which deal with the topic of funding of election campaigns. These studies range across countries at various stages of development. Here we discuss a select few.

Mironov and Zhuravskaya (2013) study pervasive corruption in public procurement in Russia.

[6] Recent estimates of India's hidden economy range from 20% to over 40%. See Chaudhuri et al. (2006), Kumar (2016) among others.

Using micro-level data on tunneling for the population of large Russian firms, they find that corruption exhibits political cycle: firms with procurement revenue provide shadow financing for regional elections. Another related work is Akhmedov and Zhuravskaya (2004). Using regional monthly panel data from Russia, they find that the budget cycle is sizable and short-lived; public spending shifts toward direct monetary transfers to voters. Gingerich (2010) studies the case of a gubernatorial contest in Brazil where various accounts have repeatedly stressed that a significant amount of the money used in campaigns is actually unreported to electoral authorities. Using a dataset which documents the allocation of illicit campaign funds during a gubernatorial reelection campaign in the state of Minas Gerais in 1998, Gingerich builds a profile of the typical "bought" politician. Additionally, Gingerich estimates the effect that the purchase of low-level politicians had on electoral returns in the contest. Costas et al. (2010), using data from Spain, analyzes the impact of (publicized) local corruption on electoral outcomes. They construct a database, based upon press reports published between 1996 and 2009, pertaining to corruption scandals and corruption news related to bribe-taking in exchange of amendments in land use plans. They find robust evidence of punishment by voters.

The possibility of amassing wealth from holding some political office bears implications upon the issue of election financing. Using data from Great Britain, Eggers and Hainmueller (2009) estimate the returns to serving in Parliament. They exploit data on the estates of recently deceased politicians. Utilising both matching and a regression discontinuity design to compare Members of Parliament (MPs) with parliamentary candidates who narrowly lost, they highlight an interesting asymmetry: they find that serving in office almost doubled the wealth of Conservative MPs, but had no discernible financial benefits for Labour MPs. In a similar spirit Fisman et al (2014) study the wealth accumulation of Indian state politicians using public disclosures required of all candidates. The annual asset growth of winners is 3–5% higher than that of runners-up, a difference that holds also in a set of close elections.

India has had several restrictions on the role of corporate funding of political parties or individual politicians. Some of these restrictions had been very stringent and have subsequently been relaxed to a certain degree. Nonetheless, several caps remain, which seem inconsistent with the actual spending witnessed around elections.[7] Gowda and Sridharan (2012) provides an excellent starting point in this context. They document how India has developed complex election expenditure, political party funding, and reporting and disclosure laws. They go on to suggest that these laws may have perverse impacts on the electoral system: they tend to drive campaign expenditure underground and foster a reliance on unaccounted funds or "black money". This tends to lead to an adverse selection system, in which those willing and able to work with black money dominate politics (see also Mehta (2002)).

Kapur and Vaishnav (2013) approach the issue of funding of Indian elections from a direction different from ours. They argue that where firms are highly regulated, politicians

[7] See Bhavnani (2011), Timmons and Kumar (2009) among others.

can exchange policy discretion or regulatory forbearance for bribes and monetary transfers from firms. They focus on the role of the construction sector and hypothesize that builders will experience a short-term liquidity crunch as elections approach because of their need to re-route funds to politicians as a form of indirect election finance. They use variation in the demand for cement to investigate the presence of an electoral cycle in building activity in India consistent with their logic; they find that cement consumption does exhibit a political business cycle consistent with their hypothesis. Sukhtankar (2012) by focusing on sugar mills in India provides evidence of embezzlement in politically controlled mills during election years, reflected in lower prices paid to farmers for cane. His result complements the literature on political cycles by demonstrating how campaign funds are raised rather than used. Politicians compensate farmers upon getting elected; this provides a justification for how they can get away with stealing.

The above papers demonstrate — in different ways — how illegal funds operate in politics and emphasize the abuse of power. They, however, do not engage with the question of the extent of the malaise by providing a sense of the magnitude of these illegal funds/black money. To the best of our knowledge, the only paper which attempts to deal with such a question is Chaudhuri et al. (2006). They investigate the size of the hidden economy in Indian states over the period 1974/75—1995/96 and find that the growth in the size of the hidden economy is approximately 4% less in scheduled election years than in all other years. Recall that our approach actually relies on the orthogonality of the timing of state-level elections and NSS surveys to identify the spending from black money transfers, whereas Chaudhuri et al. (2006) use a multiple indicator multiple cause approach (MIMIC) which is an altogether different methodology. Additionally, none of the aforementioned papers study the impact of transferring such (illegal) funds to voters.[8]

Our work also bears similarities with some of the literature on consumption smoothing, health and poverty. In our work, the windfall aspect of election-related cash transfers in part reflects the liquidity constraints of some households like in Altonji and Siow (1987). Tarozzi (2005) looks at whether a sudden increase of the price of rice supplied by the Indian Public Distribution System in the southern state of Andhra Pradesh had a negative impact on child nutrition. A price rise in such a staple clearly generates a reduction in real income as households do not completely substitute away from rice. He finds that longer exposure to high prices are not accompanied by worse nutritional status, as measured by weight-for-age. In our analysis, we do find an increase in the consumption of foodgrains some days before elections. This is in line with Subramanian and Deaton (1996). Using data from the Indian state of Maharashtra, they estimate the elasticity of calorie consumption with respect to total expenditure and show that it declines only slowly with levels of living; moreover, it is bounded away from zero.[9]

[8] Akhmedov and Zhuravskaya (2004) comes closest in this regard. They document that public spending shifts toward direct monetary transfers to voters but do not study how these voters spend the money.

[9] Tarozzi et al. (2014) conduct a large-scale cluster randomized controlled trial to study the uptake

3 Empirical Analysis

We first describe our data and the identification strategy in some detail. Next, we present more details on the empirical specification.

3.1 Data and Empirical Strategy

This paper combines data from two sources: The Election Commission of India and the National Sample Survey Organisation. From the Election Commission of India we use the data of survey for state assembly elections in each district for the period 2004-12. Given that these state elections typically take place once every 5 years in an average Indian state, we roughly have two-three election cycles for each state during this 9–year period. Whenever the date of election was not clear for any specific district we have sought clarification from newspaper articles.

We match these data on the date of election from the Election Commission with data on consumption expenditures using the date of survey of the NSS consumption expenditure rounds. We have annual data from the NSS on consumption expenditure for this period. We use all NSS rounds between the 60th and the 68th, a total of seven rounds.[10]

Since elections happen in several waves or phases within a state in India, the same state may have multiple election dates. However, typically no district has an election on more than one day, so we have a unique election date for each district for each round of elections. We compare this date to the date of survey and create four groups: two for the *treated* ("before" and "after") and two for the *control* ("before" and "after"). The definition of these is dealt with in the following paragraphs.

We define an election window (δ) as the period in which households are exposed to pre-electoral cash for votes. First consider households in a state, s, that is scheduled to undergo elections in a year t. Fixing δ (at say 7 days, 10 days, 30 days, 60 days, $\ldots$), we define a household as one (potentially) exposed to cash for votes distributed pre-electorally if it's date of survey (DOS) is within δ days of the date of election (DOE). So these form the "treated" group when viewed "after" treatment (pre-election cash inflow). Therefore, the expenditure levels of these households are taken to represent the post-intervention observations. Households interviewed within $(\delta, 2\delta]$ days before the DOE are considered as observations in the exposed group, but their expenditure levels are treated as pre-exposure levels of observations;

of a health-protecting technology, insecticide-treated bednets (ITNs), through micro-consumer loans, as compared to free distribution and control conditions. They find that despite a relatively high price, 52% of sample households purchased ITNs, highlighting the role of liquidity constraints in explaining earlier low adoption rates. See also Deaton and Paxson (1998), Fafchamps and Shilpi (2008) and Jensen and Miller (2008) among others.

[10]We would have liked to extend this analysis to incorporate earlier elections however the NSS does not report the date of survey for most (except 59th) of the rounds prior to the 60th round.

so they are the "before" component of the "treated" group.

Finally, households in states that geographically share borders with state s, and do not have elections of their own election on the same DOE, are treated as unexposed households. We again identify pre-exposure and post-exposure observations symmetrically in the unexposed group by using the distance between DOS and DOE in the control states as well. Effectively, our data represent a repeated cross-sectional design with different households in the pre-exposure and post-exposure samples. Exploiting this variation in the data should ideally give us unbiased estimates of the impact of being exposed to pre-election distribution of cash on consumption activities unless there is selection into the survey by households. We discuss these concerns later.

3.2 Empirical specification

Our identification strategy basically is akin to a *difference-in-differences* approach (D-D) where the *control* and *treated* are defined as in the preceding paragraphs. As explained above, our sample depends upon the window size δ. Hence all the coefficients in the estimation equation (presented below) depend upon δ. But for ease of exposition, we omit δ from the notation. Our estimation equation therefore looks as follows:

$$y_{ist} = \beta_0 + \beta_1 Treated_{ist} + \beta_2 After_{ist} + \beta_3 Treated_{ist} * After_{ist} + \gamma \mathbf{X_{ist}} + \epsilon_{\mathbf{ist}} \tag{1}$$

where $Treated_{ist}$ is a binary indicator that indicates if household i resided in state s and had elections in year t. $After_{ist}$ is another binary indicator that takes the value 1 for household i if it was interviewed within δ days of the election in state s and is 0 for households that were interviewed between $(\delta, 2\delta]$ days of elections in state s. The outcome variable y_{ist} is some measure of consumption. In some of our models this is average household consumption expenditure, in others we look at expenditure on specific groups such as pulses, clothes or local liquor, and finally, we also look at models in which we specify the outcome to be the share of expenditure of specific items in household's overall consumption expenditure. $\mathbf{X_{ist}}$ is set of control variables at the household level.

The key parameter of interest is the coefficient on $Treated_{ist} * After_{ist}$, i.e, β_3. This captures the average increase in y_{ist} for households that were δ days or closer to the date of election after accounting for baseline patterns in consumption expenditure in the state of elections and also other changes in consumption pattern that may be occurring economy-wide.

Two points merit careful attention. They are: (i) households that are not within $[0, 2\delta]$ days of an election in a state with elections (or in a state without elections, but neighbouring a state with elections) are dropped from the sample; and (ii) households that are in states that do not have an election, and do not have neighbouring states that do not have elections are also dropped from the election.

The key identification assumption for our *difference-in-differences* framework is the assumption that in the absence of the elections the time trends we see in consumption patterns would have be the same in the unexposed group — i.e. in the neighbouring states that do not see elections. Recall that the sample, and therefore, the model's coefficients are indexed to δ. As we reduce δ i.e. look at periods closer to the election, we look at smaller samples, and as we increase δ our samples increase and thus, all our coefficients change with δ.[11] Finally, we use robust standard errors throughout our models, although, the results are very similar with bootstrapped standard errors.

From a theoretical standpoint, there is no "optimal" value of δ: which is the most appropriate window size is dictated by one's prior on *when exactly* is the cash distributed before the elections. Media reports suggest that it starts about 90 days prior to elections. But then, when do these vote-buying activities *stop*? There is usually a lot of police presence in the 48 hours before election and this limits possibilities to distribute cash. Of course, the consumption uptick (if any) would be subsequent to the cash injection (if any) but then again it could manifest itself at *any subsequent time*, starting with immediately after. Finally, one must bear in mind that the recall period for the consumption data is 30 days from the date of survey. Hence the need to study various values of δ. That said, the further we are from the election date — say, between 60 to 90 days, we should expect to see little or no difference in household consumption expenditures in states with and without elections and thus, our coefficient of interest should not be significantly different from zero. As we move closer, to the election, we should find that there are spikes in consumption expenditures for some intermediate δs.

3.3 Results

Table 1 contains some descriptive statistics for the key variables in our analysis. As mentioned above, the size of the window (δ) determines the sample and thereby the definition and size of the four groups: *treated*, *control*, *before* and *after*. In the table, we report statistics for 3 different windows: 10, 30 and 50. It is re-assuring to note that the means and dispersion of all the variables remain comparable across the different window (and hence samples).

The case of $Treated * After$ is particularly important. Although the timing of the NSS surveys are determined independently of state assembly election timings, one may be concerned with the scenario that the NSSO enumerators may not survey households in areas where elections are near. If the concern was indeed valid then our $Treated * After$ set of households would be very small relative to the total sample; and this would be especially true for short window-lengths.

[11] To keep the notation clean, we index our key coefficient of interest with δ, i.e. β_3^δ since this is what we discuss going forward, however, the other coefficients too vary with δ.

	Mean	Std. Dev	Min	Max	Mean	Std. Dev	Min	Max	Mean	Std. Dev	Min	Max
Window Size in days	[10]	[10]	[10]	[10]	[30]	[30]	[30]	[30]	[50]	[50]	[50]	[50]
Pulses (INR)	450.0	437.1	0.0	5,950.0	462.5	433.8	0.0	8,790.0	464.2	433.5	0.0	8,790.0
Fish/Meat (INR)	90.1	204.2	0.0	9,500.0	87.5	210.1	0.0	20,150.0	88.6	207.3	0.0	20,150.0
Intoxicants (INR)	19.0	107.5	0.0	4,500.0	21.9	114.8	0.0	4,500.0	22.2	125.7	0.0	15,000.0
Clothes (INR)	1,158.1	2,148.4	0.0	41,000.0	1,156.0	2,127.1	0.0	41,000.0	1,184.3	2,180.9	0.0	67,930.0
Health (INR)	287.2	3,651.6	0.0	300,000.0	332.1	4,575.9	0.0	343,000.0	326.7	4,352.7	0.0	343,000.0
Education (INR)	798.1	4,264.5	0.0	156,000.0	807.9	4,226.4	0.0	181,000.0	860.9	4,965.2	0.0	657,390.0
Treated	0.222	0.416	0.000	1.000	0.243	0.429	0.000	1.000	0.251	0.434	0.000	1.000
After	0.635	0.481	0.000	1.000	0.600	0.490	0.000	1.000	0.578	0.494	0.000	1.000
Treated*After	0.137	0.344	0.000	1.000	0.143	0.350	0.000	1.000	0.140	0.347	0.000	1.000
Hindu	0.823	0.381	0.000	1.000	0.826	0.379	0.000	1.000	0.829	0.377	0.000	1.000
General Caste	0.351	0.477	0.000	1.000	0.353	0.478	0.000	1.000	0.358	0.479	0.000	1.000
Rural	0.608	0.488	0.000	1.000	0.600	0.490	0.000	1.000	0.600	0.490	0.000	1.000
Household size	5.348	3.185	1.000	32.000	5.385	3.330	1.000	43.000	5.426	3.396	1.000	43.000

Table 1: *Descriptive Statistics.*
The sample depends upon the chosen window size. Here three different window sizes are reported, namely 10 days, 30 days and 50 days.

But Table 1 assures that such is not the case. The proportion of households in our sample who fall in the $Treated * After$ group is around 14% and is so for all of the window sizes.

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	(15)	(20)	(30)	(40)	(50)
Treated	59,754.078***	51,109.195***	72,032.380***	66,385.638***	63,658.911***	60,426.595***
	(5,844.695)	(4,636.521)	(5,508.697)	(3,142.690)	(2,418.831)	(1,982.072)
After	4,176.255***	7,609.200***	9,747.963***	6,102.393***	604.963	3,523.830***
	(1,364.210)	(1,092.711)	(938.887)	(791.028)	(766.505)	(682.590)
Treated*After	-5,429.554*	2,586.182	1,963.976	15,589.066***	23,485.790***	17,620.095***
	(3,125.309)	(2,503.439)	(2,258.064)	(1,612.535)	(1,502.956)	(1,420.176)
Hindu	4,677.540***	3,246.190**	2,942.338**	3,303.899***	4,180.666***	3,954.121***
	(1,560.047)	(1,383.495)	(1,242.854)	(1,005.138)	(881.181)	(799.652)
General caste	13,445.326***	11,507.481***	11,401.604***	11,609.241***	11,529.472***	11,184.900***
	(1,474.864)	(1,212.862)	(1,081.773)	(896.520)	(806.901)	(726.982)
Rural	-13,092.340***	-11,856.131***	-10,901.277***	-9,313.898***	-9,579.038***	-9,600.019***
	(1,324.224)	(1,088.265)	(973.313)	(797.846)	(741.498)	(670.058)
Household Size	-2,457.063***	-2,197.823***	-2,114.184***	-1,962.998***	-2,035.284***	-1,935.537***
	(157.239)	(123.999)	(110.192)	(85.970)	(82.285)	(72.573)
Treated*Voteshare	-110,101.292***	-99,642.683***	-152,599.167***	-172,903.479***	-188,169.399***	-174,038.380***
	(13,360.373)	(10,289.154)	(11,924.751)	(6,685.702)	(5,333.545)	(4,375.438)
Constant term	37,645.794***	32,580.114***	28,734.521***	27,837.180***	31,735.126***	28,677.609***
	(2,315.649)	(1,836.887)	(1,588.140)	(1,261.735)	(1,247.183)	(1,108.539)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.022	0.021	0.027	0.037	0.043	0.046

Table 2: *D-D regressions: Monthly Per-capita Expenditure.* The dependent variable in every column is the household's monthly per-capita expenditure which has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

As a starting point, we document the effect of elections on the household monthly per-capita expenditure. Table 2 contains results for window lengths of 10, 15, 20, 30, 40 and 50.[12] The overall pattern is the following: the main coefficient of interest, namely that on *Treated*After* starts off with being negative and significant for a window size of 10 and then turns insignificant and finally positive and highly significant for 30 onwards.

At first glance, the negative coefficient on *Treated*After* in column (1) seems counter-intuitive and contrary to the idea of vote-buying. However, there is a simple explanation for this phenomenon. Take the case of a window size of 10. The "treated and before" group consists of households in a state with elections and where the survey was conducted between

[12]There is nothing special about these particular choices of windows; we have done the analysis for practically all integers from 7 to 60. We report just these 6 for brevity. Results for other window lengths are available upon request.

11 and 20 days (both days included, so a period of 10 days) before the elections. Now, if the cash transfers are actually made say 2 months or so in advance, then a significant proportion of the increase in expenditure may well accrue in what is being termed as the "before" period. In fact, anecdotal evidence suggests that such inducements (monetary or otherwise) are actually supplied several months in advance.[13] So it is hardly surprising that the households surveyed within 10 days of the upcoming election date would not show any higher level of expenditure when compared with those in the 11–20 day period. Longer exposure windows — especially 3 weeks or so — make the separation between recipients and non-recipients more in line with what the anecdotal evidence suggest. Our results for larger windows (see columns (4)—(6)) are consistent with this.[14]

Next we attempt to probe this effect in greater detail. We consider various broad categories of goods out of the variegated items in the consumption basket of a typical Indian household.

3.4 Main results

We study six major categories of consumption which typically account for a large share of a household's monthly consumption. They are: (i) pulses (various types of lentils, chick-peas, etc.) (ii) fish, meat and other animal products, (iii) intoxicants (alcoholic drinks, narcotic drugs, etc.), (iv) clothes (saris, dhotis, etc.), (v) health-related expenditures and (vi) education-related expenditures.

3.4.1 Spending shifts (absolute levels)

Using the same identification strategy as for the regressions with household monthly per-capita expenditure as outcome (see Table 2), we observe the effect of elections on expenditure on each of the six categories listed above. Figures 1 and 2 contain visual depictions of the coefficient of interest, i.e., *Treated*After*.[15] As can be seen from the figures, the effect of elections on spending on pulses seems to follow the general pattern underlying the change in monthly per-capita expenditure: the coefficient on *Treated*After* is positive and significant for window sizes of 30 and larger. The corresponding effect on fish and meat consumption is different. In fact, it is not statistically significant for window sizes of 30 and larger.

The pattern for "intoxicants" which is largely driven by local liquor (or"tharra") exhibits some difference when contrasted with pulses. Here the effect is positive and significant for relatively shorter windows (10 to 20) and then becomes insignificant for longer windows.

[13]See e.g., the article in Economic Times on parliamentary elections in India which can be accessed at http://economictimes.indiatimes.com/news/politics-and-nation/political-parties-wooing-electorates-with-liquor-cash-household-benefits-and-drugs/printarticle/51858290.cms

[14]There is also the possibility of (sub)conscious under-reporting in the period very close to the election date, especially if the household has indeed received such bribes.

[15]The regression tables on which these figures are based can be found in the Appendix.

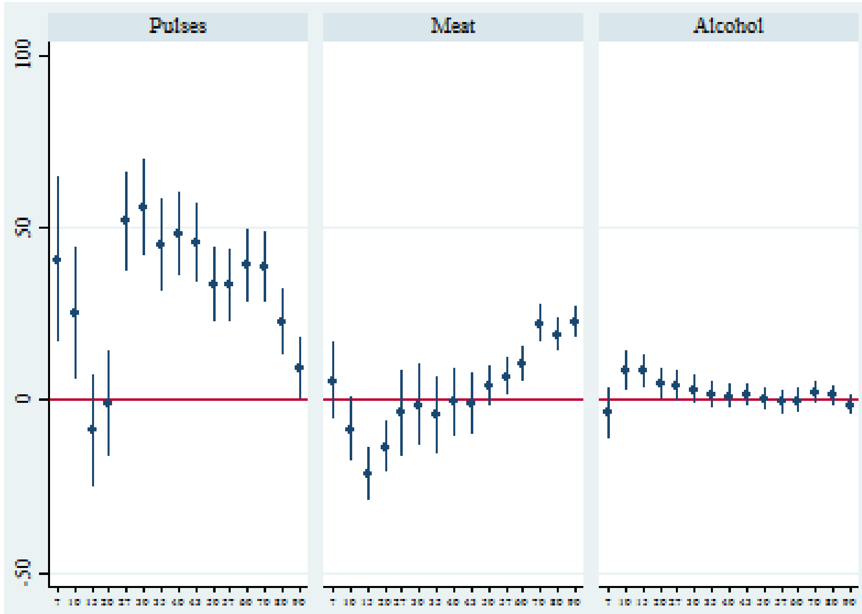

Figure 1: *Consumption shifts (absolute terms, units: INR) for Pulses, Fish/Meat and Intoxicants:* The dots denote the coefficient of interest which captures the effect of elections, namely, *Treated*After*. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

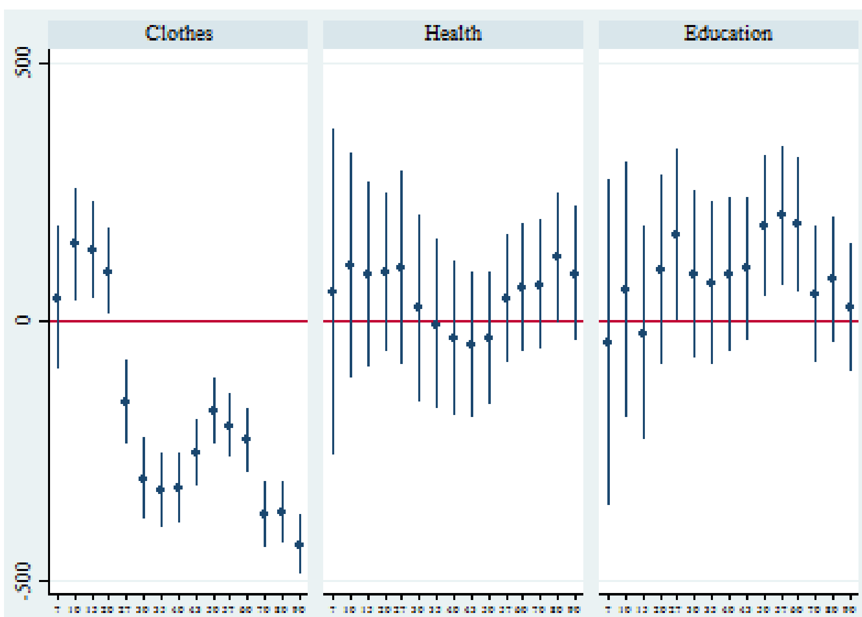

Figure 2: *Consumption shifts (absolute terms, units: INR) Clothes, Health and Education:* The dots denote the coefficient of interest which captures the effect of elections, namely, *Treated*After*. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

This suggests that the supply of these items are increased *closer* to the election dates, which again appears consistent with media accounts. The pattern for clothes echoes that of liquor consumption for the relatively shorter windows (10 to 20), after which the coefficient on *Treated*After* becomes negative and significant. We will re-examine and remark upon this specific category later. When it comes to health-related expenditures, elections do not seem to induce any specific spending sprees. The same can (almost) be said for education-related expenditures, where the coefficient is statistically significant only for relatively large windows (around 50 and higher).

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	0.040***	0.035***	0.012***	-0.023***	-0.025***	-0.026***
	(0.007)	(0.005)	(0.004)	(0.003)	(0.002)	(0.002)
After	-0.007***	-0.003**	0.001	0.000	0.001	0.000
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treated*After	0.015***	0.000	-0.007***	0.010***	0.010***	0.008***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Hindu	-0.005***	-0.003*	-0.002	-0.002	-0.001	-0.001
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
General caste	-0.026***	-0.026***	-0.027***	-0.027***	-0.027***	-0.027***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Rural	0.044***	0.043***	0.042***	0.039***	0.040***	0.042***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Household Size	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated*Voteshare	-0.191***	-0.150***	-0.080***	-0.030***	-0.022***	-0.023***
	(0.014)	(0.012)	(0.009)	(0.006)	(0.004)	(0.004)
Constant term	0.125***	0.122***	0.118***	0.123***	0.120***	0.123***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.064	0.061	0.060	0.059	0.061	0.066

Table 3: *D-D regressions: Pulses (Proportion of MPCE).* The dependent variable in every column is the proportion of the household's monthly per-capita expenditure which is spent on pulses. This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

On the whole, it is clear that there is some heterogeneity in terms of the changes in consumption of the six different categories around elections. But what is perhaps more striking is the very fact that *any* kind of changes in consumption actually exist in a democracy with "free and fair elections".

3.4.2 Spending patterns (relative to overall level)

Next we focus on spending patterns on the very same six categories when viewed as a ratio of the total household monthly consumption expenditure. In other words, we seek to isolate any changes in the *pattern* and not just the *level* of spending for these six groups.

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	0.003*	0.003*	-0.002**	-0.009***	-0.009***	-0.010***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
After	-0.000	0.002***	0.003***	0.001***	0.001***	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated*After	-0.001	-0.003***	-0.004***	0.000	-0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Hindu	-0.016***	-0.016***	-0.016***	-0.016***	-0.016***	-0.016***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
General caste	0.000	-0.001	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Rural	0.004***	0.004***	0.004***	0.004***	0.003***	0.004***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Household Size	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated*Voteshare	-0.031***	-0.025***	-0.011***	-0.001	0.001	0.002**
	(0.004)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
Constant term	0.041***	0.039***	0.037***	0.039***	0.038***	0.039***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.045	0.045	0.045	0.046	0.044	0.045

Table 4: *D-D regressions: Fish+Meat (Proportion of MPCE).* The dependent variable in every column is the proportion of the household's monthly per-capita expenditure which is spent on fish and meat. This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

Table 3 reports the results for the first category (pulses) and for the six different window sizes as before (10, 15, 20, 30, 40 and 50 days). As the coefficients on *Treated*After* in the different columns (window sizes) show, the effect is similar to those in levels. So, not only is there an increase in the consumption of pulses in *absolute terms* but there is also an increase in the proportion of pulses consumed *relative to overall household consumption* (see in particular columns (4)—(6) in Table 3). Moving on to fish and meat consumption, we find a decline in the proportion consumed for some window sizes (see Table 4). The effect on the relative proportion of intoxicants (again primarily driven by local liquor) is recorded in Table 5. It mirrors that of the absolute levels; the coefficient on *Treated*After* is positive and significant for window sizes of 10, 15, 20 and 30.

Once again, as reported in Table 6, the pattern for clothes matches that of liquor consumption for the relatively shorter windows (10, 15 and 20) after which the coefficient on *Treated*After* becomes negative and significant. Also, the coefficients are larger in size compared to those for liquor.[16]

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	0.000	-0.000	-0.001	-0.001	-0.000	-0.001**
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
After	-0.002***	-0.002***	-0.001***	-0.001***	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated*After	0.002***	0.002***	0.001**	0.001**	0.000	0.000
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Hindu	0.001	0.001**	0.001**	0.001***	0.000**	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
General caste	-0.003***	-0.003***	-0.004***	-0.003***	-0.004***	-0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Rural	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Household Size	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated*Voteshare	-0.004*	-0.004**	-0.002	-0.003**	-0.002**	-0.002**
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Constant term	0.009***	0.008***	0.008***	0.008***	0.008***	0.007***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.010	0.009	0.009	0.009	0.009	0.009

Table 5: *D-D regressions: Intoxicants (Proportion of MPCE).* The dependent variable in every column is the proportion of the household's monthly per-capita expenditure which is spent on intoxicants (various types of alcoholic drinks and drugs). This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

When looking at the proportionate spending on health-related items, we notice a positive effect of elections for short window lengths (see in particular, columns (1) and (2) in Table 7). There is no significant effect for longer window sizes. The pattern for education-related items is much more responsive to elections; Table 8 contains some results. In fact, for a large majority of window sizes, the coefficient on *Treated*After* is positive and significant.

All in all, there seems to be a rise in the relative proportion of pulses consumed as well as intoxicants (largely local liquor or "tharra") and also in education-related items and to a much lesser degree some health-related items. Fish and meat consumption show a relative decline for some window sizes while clothes demonstrate a rise for shorter windows and a fall for longer ones. Hence, the proportions do shift considerably for a wide range of household consumption items.

[16]The coefficients in the case of clothes is about 20 times larger than that for intoxicants (compare tables 5 and 6 column by column).

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	-0.327***	-0.297***	-0.274***	-0.187***	-0.165***	-0.179***
	(0.020)	(0.015)	(0.009)	(0.005)	(0.004)	(0.004)
After	0.032***	0.017***	0.009*	0.007	0.003	-0.038***
	(0.007)	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)
Treated*After	0.037***	0.041***	0.020**	-0.070***	-0.083***	-0.037***
	(0.011)	(0.009)	(0.008)	(0.008)	(0.007)	(0.006)
Hindu	-0.021***	-0.020***	-0.015***	-0.020***	-0.020***	-0.022***
	(0.007)	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)
General caste	0.018***	0.021***	0.022***	0.015***	0.013***	0.013***
	(0.006)	(0.005)	(0.005)	(0.004)	(0.003)	(0.003)
Rural	-0.018***	-0.013***	-0.019***	-0.007*	-0.010***	-0.010***
	(0.006)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)
Household Size	-0.005***	-0.006***	-0.005***	-0.005***	-0.005***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Treated*Voteshare	0.710***	0.600***	0.574***	0.563***	0.553***	0.522***
	(0.046)	(0.035)	(0.021)	(0.014)	(0.012)	(0.011)
Constant term	0.277***	0.287***	0.283***	0.284***	0.281***	0.321***
	(0.012)	(0.010)	(0.009)	(0.007)	(0.007)	(0.006)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.010	0.008	0.009	0.012	0.014	0.016

Table 6: *D-D regressions: Clothes (Proportion of MPCE).* The dependent variable in every column is the proportion of the household's monthly per-capita expenditure which is spent on clothes (saris, dhotis, shirts, trousers, etc.). This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	-0.011	0.006	-0.013	-0.016***	-0.016***	-0.021***
	(0.018)	(0.016)	(0.009)	(0.005)	(0.004)	(0.004)
After	-0.018***	-0.010**	-0.001	-0.005	-0.004	-0.008**
	(0.007)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)
Treated*After	0.027***	0.014*	0.007	0.003	-0.003	0.003
	(0.009)	(0.008)	(0.006)	(0.006)	(0.005)	(0.005)
Hindu	0.002	0.005	0.005	0.001	-0.001	-0.001
	(0.005)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
General caste	0.009*	0.009**	0.009***	0.009***	0.007***	0.010***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Rural	0.006	0.004	0.002	0.002	0.000	0.000
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)
Household Size	0.000	0.000	0.000	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Treated*Voteshare	-0.033	-0.063*	-0.007	0.009	0.026***	0.022***
	(0.041)	(0.036)	(0.020)	(0.011)	(0.009)	(0.008)
Constant term	0.037***	0.034***	0.026***	0.033***	0.034***	0.039***
	(0.011)	(0.008)	(0.006)	(0.006)	(0.005)	(0.005)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.000	0.000	0.000	0.000	0.000	0.000

Table 7: *D-D regressions: Health-related (Proportion of MPCE).* The dependent variable in every column is the proportion of the household's monthly per-capita expenditure which is spent on health-related items (medicines, check-ups, etc.). This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	0.022	0.021	-0.045***	-0.056***	-0.052***	-0.056***
	(0.021)	(0.017)	(0.010)	(0.007)	(0.007)	(0.007)
After	-0.008	0.000	0.004	-0.009	-0.024***	-0.039***
	(0.008)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)
Treated*After	0.032***	0.009	0.019**	0.025***	0.016**	0.025***
	(0.012)	(0.010)	(0.009)	(0.008)	(0.007)	(0.007)
Hindu	0.004	0.014*	0.018***	0.023***	0.025***	0.022***
	(0.009)	(0.008)	(0.007)	(0.006)	(0.005)	(0.005)
General caste	0.057***	0.062***	0.063***	0.060***	0.059***	0.064***
	(0.007)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)
Rural	-0.093***	-0.090***	-0.091***	-0.088***	-0.084***	-0.087***
	(0.007)	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)
Household Size	-0.002*	-0.002**	-0.002*	-0.002***	-0.003***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treated*Voteshare	-0.165***	-0.133***	0.003	0.021	0.025*	0.008
	(0.044)	(0.036)	(0.021)	(0.014)	(0.014)	(0.013)
Constant term	0.162***	0.149***	0.139***	0.148***	0.158***	0.179***
	(0.016)	(0.012)	(0.011)	(0.010)	(0.009)	(0.009)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.014	0.013	0.013	0.012	0.011	0.012

Table 8: *D-D regressions: Education-related (Proportion of MPCE).* The dependent variable in every column is the proportion of the household's monthly per-capita expenditure which is spent on education-related items (school fees, books, tuitions, etc.). This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

Next, we conduct another set of tests to ensure that our basic hypothesis of vote-buying *prior to elections* remains consistent with its internal logic.

3.4.3 Post-election patterns

If what we observe in these household-level consumption data is indeed driven by pre-election attempts at vote-buying, then such patterns should eventually disappear days after the conduct of elections. Of course, there may be a significant delay before such spikes in consumption completely ebb away. Furthermore, the final levels may still be (marginally) higher owing to the one-time injection of cash; however, the effect on the proportions should disappear with the passage of time.

To examine this, what we do is the following. We construct windows which chronologically are fully post-elections. Take any window length; say 10, for the sake of illustration. So we are effectively comparing households interviewed within the date of election and 10 days *since that date of election* with those households surveyed between 11 and 20 days since the date of election. So these, respectively, constitute our *before* and *after* of the"treated" group. It is important to bear in mind that in these two contiguous intervals of time, the recall period (for the monthly consumption expenditure on various items) of 30 days captures time-frames prior to the election.

If our hypothesis about vote-buying before elections is indeed correct, we should expect to see some positive impact on consumption for smaller time windows and possibly negative or insignificant effects for larger ones. Negative coefficients are possible as we end up comparing households who still are benefiting from cash transfers circa election dates (as the "before" group) with households whose survey dates are so much later than the election date that even the 30-day recall period keeps them sufficiently far way from their election date (as the "after" group).

Figures 3 and 4 depict the coefficient on *Treated*After* for the same six consumption categories for varying window lengths; here the outcome is the actual expenditure rather than viewed as a proportion of the overall expenditure. Starting with pulses, we see that there is a positive effect which persists for some windows and then turns negative and statistically significant. The pattern for fish and meat consumption is broadly similar although there is no evidence of the spike persisting at all for even very short windows.

For intoxicants, the effect is as expected with a positive and significant "spike" for the 10-day window and then insignificant coefficients thereafter. For clothes there is some evidence of negative effects for some windows which become insignificant as we go far away from the election date. Broadly similar patterns emerge for health- and education-related expenditures as well.

The results which obtain when the outcome of interest is the proportion of spending on each

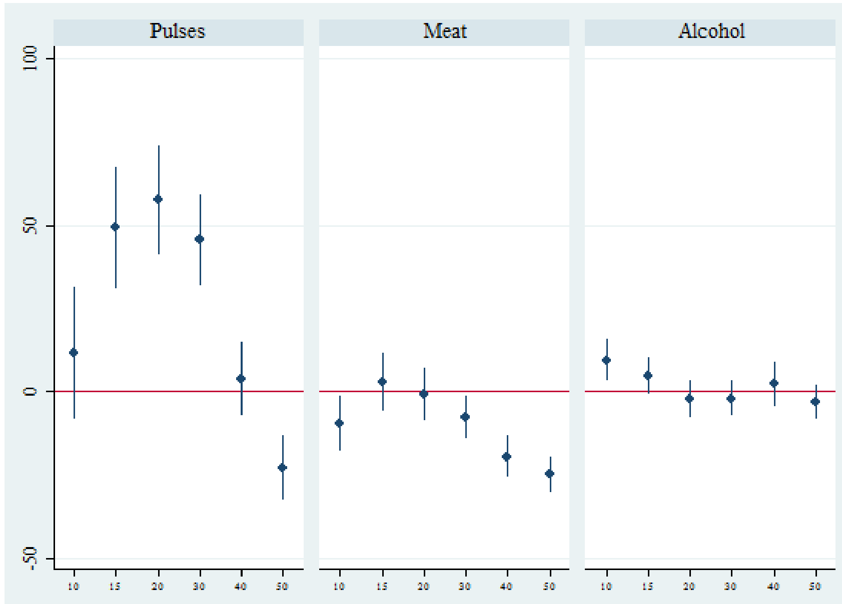

Figure 3: *Consumption shifts after elections (absolute terms, units: INR) Pulses, Fish/Meat and Intoxicants:* The dots denote the coefficient of interest which captures the effect of elections, namely, *Treated*After*. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

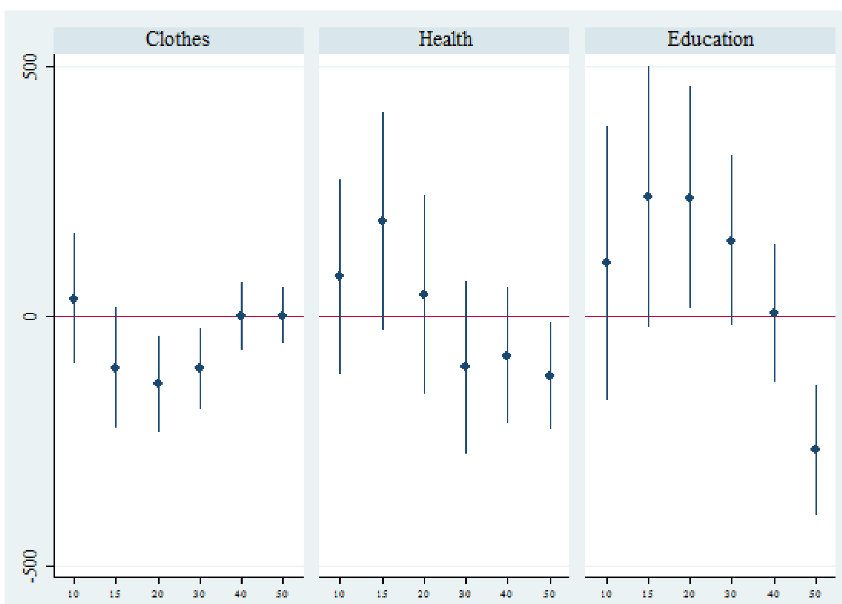

Figure 4: *Consumption shifts after elections (absolute terms, units: INR) Clothes, Health and Education:* The dots denote the coefficient of interest which captures the effect of elections, namely, *Treated*After*. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

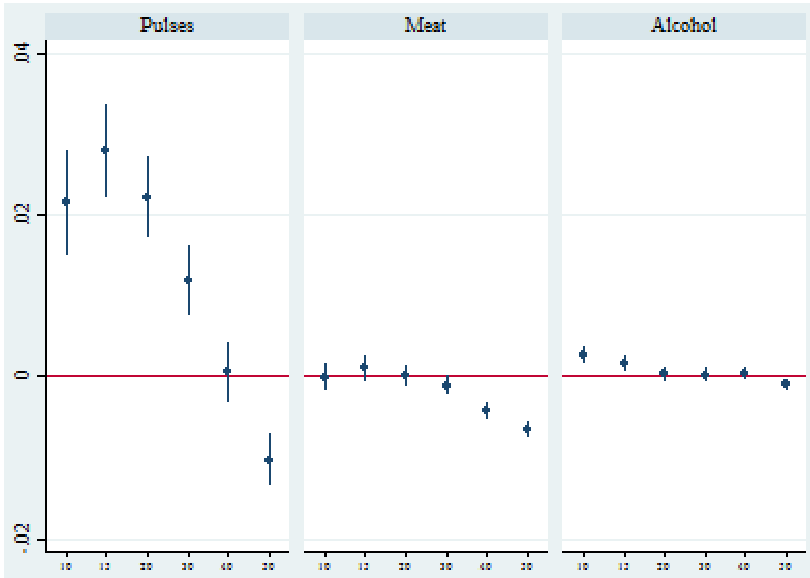

Figure 5: *Consumption shifts after elections (changes in proportions) Pulses, Fish/Meat and Intoxicants:* The dots denote the coefficient of interest which captures the effect of elections, namely, *Treated*After*. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

category (see Figures 5 and 6) is in line with the previous ones.

On the whole, these results corroborate our original hypothesis: the spikes in consumption that we observed are basically a pre-election phenomena which eventually disappear as we move further and further away from election dates.

Next, we explore another aspect of heterogeneity in terms of pre-election spending patterns.

3.4.4 Different sub-categories of households

We re-do our analysis for two distinct subsets of households: one is the set of landless households and the other belong to the lowest tercile (so the poorest 33% of the total sample of households).

Our objective here is two-fold. First, we wish to check if the relatively deprived households actually show any evidence of consumption-adjustment or is it solely limited to their wealthier counterparts. Secondly, if these deprived households too are targeted (in terms of transfers in cash and/or kind) how *do they* adjust their consumption patterns? Do they follow the same pattern as the richer households or behave otherwise. In the interest of brevity, we simply discuss the results where the outcome is the proportional spending on each of the six categories studied so far.

First, we report the results for the landless households. Figures 7 and 8 display the core

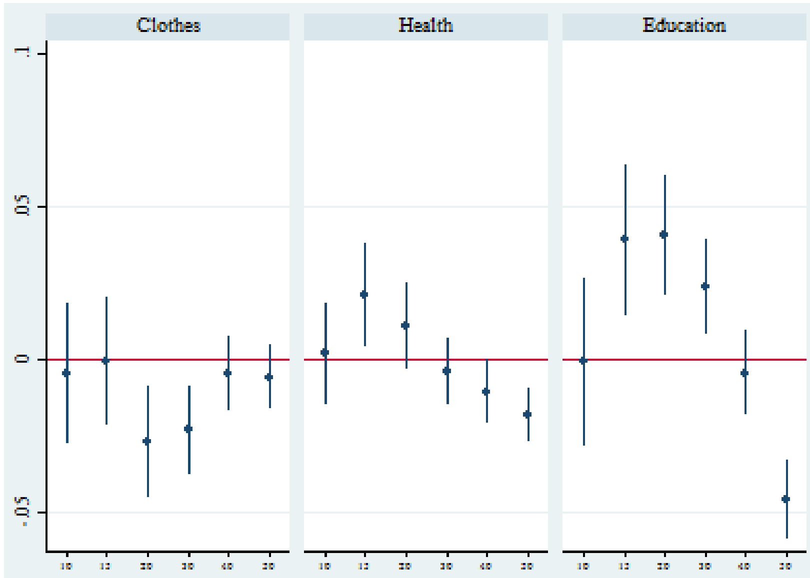

Figure 6: *Consumption shifts after elections (changes in proportions) Clothes, Health and Education:* The dots denote the coefficient of interest which captures the effect of elections, namely, *Treated*After*. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

findings. Start with pulses; here we find that if anything the effect of elections on the proportion of expenditure on pulses is greater than in the case of the overall sample. For most of the windows, the effect is positive and highly significant. Moving on to fish and meat consumption, we find some positive effects for short window sizes (10 and 15) and no significant effects for the others. This is rather different for the total sample where the effect if anything is *negative* for some window sizes (see Table 4). The story is similar for intoxicants. This is in contrast with the case of all households (see Table 5) where the coefficient on *Treated*After* is positive and significant for several window sizes. The pattern for clothes is exactly the same as for the overall sample: it is positive and significant for short window lengths and turns negative and significant for the longer ones (30 days onwards).

For expenditure on health-related items, the pattern again resembles that of the total sample: the coefficient on *Treated*After* is positive and significant for the short windows (10 and 15 days) and turns insignificant after that. What is notable is that the *size* of these coefficients (when statistically significant) are almost three times as large as those for the overall sample. This is also true for the case of education-related expenditures.

On the whole, we find that the consumption patterns of landless households too are affected by elections. While there exist similarities in the adjustments to those by the total sample of households, some important distinctions emerge. They are:
(a) Some effects appear stronger/larger in this subset: specifically, spending on pulses, fish-meat, health- and education-related items.

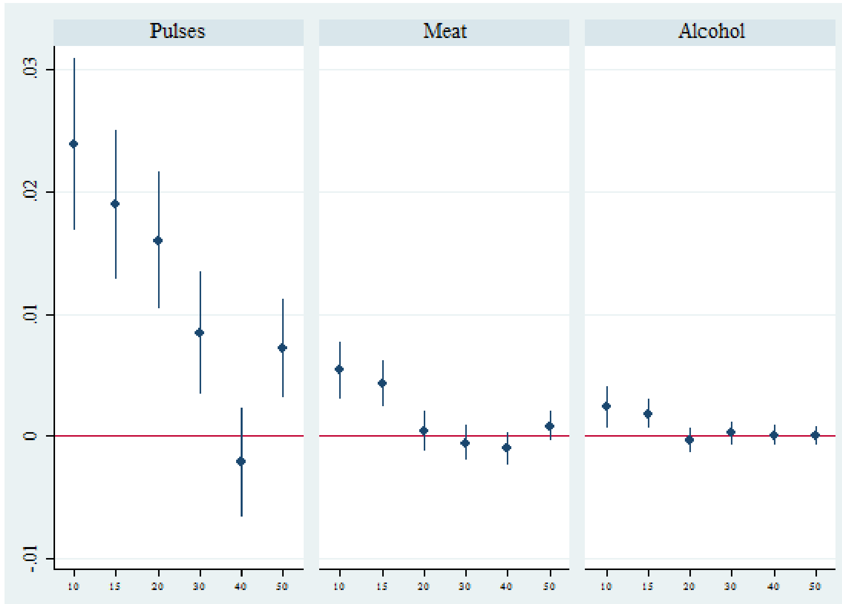

Figure 7: *Consumption shifts for the landless households (changes in proportions) Pulses, Fish/Meat and Intoxicants:* The dots denote the coefficient of interest which captures the effect of elections, namely, *Treated*After*. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

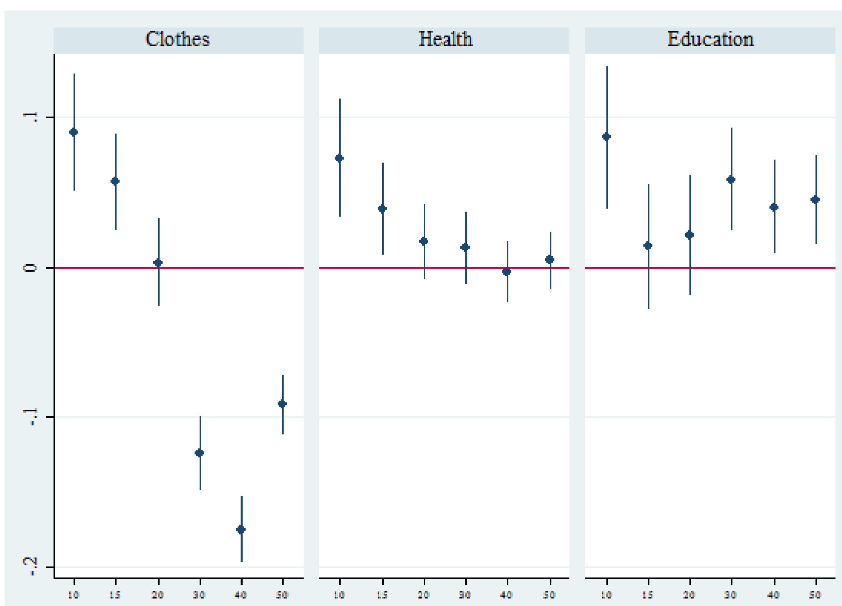

Figure 8: *Consumption shifts for the landless households (changes in proportions) Clothes, Health and Education:* The dots denote the coefficient of interest which captures the effect of elections, namely, *Treated*After*. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

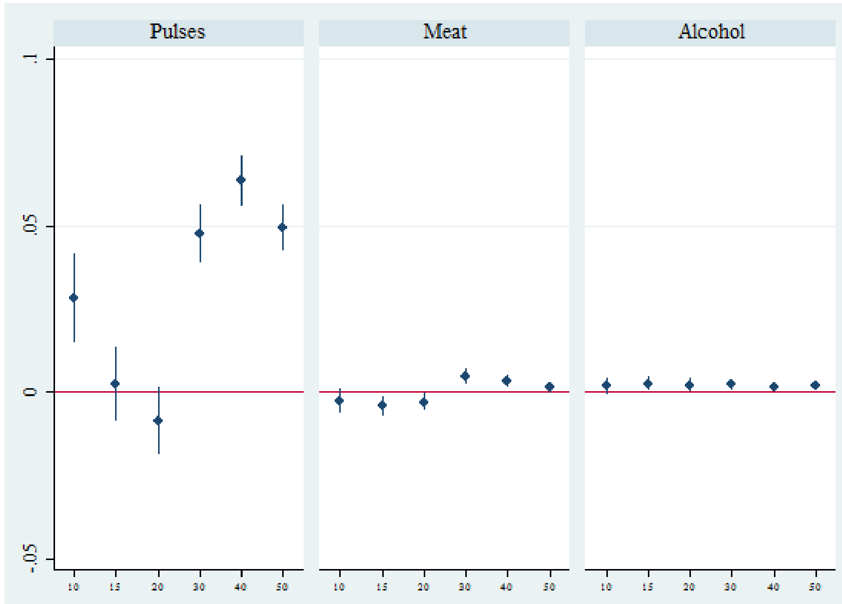

Figure 9: *Consumption shifts for the lowest tercile households (changes in proportions) Pulses, Fish/Meat and Intoxicants:* The dots denote the coefficient of interest which captures the effect of elections, namely, *Treated*After*. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

(b) Some effects appear more muted in this subset: specifically, spending on intoxicants.

Figures 9 and 10 pertain to the households in the lowest tercile and display the coefficient on *Treated*After* for the same six categories for varying window lengths. Here too, we note that the patterns are often closer to those of landless households than to the overall sample. Of course, the pattern for intoxicants, health- and education-related items for the lowest tercile are more like those for the overall sample than like the landless. But for the remaining ones, the similarity with the landless household results are stronger. On the whole, this is not a surprise as the correlation between the set of landless households and those in the lowest tercile is rather high; this follows from the fact that the landless households in any developing country tend to be highly impoverished.

The puzzling asymmetries noted above (specifically, under points (a) and (b)) motivate our simple theoretical analysis which is fleshed out in section 4.

4 Theory

We build a simple model to study the optimal consumption bundle of an individual and track its transformation when he experiences an income change. In particular, we are interested in the potential heterogeneity in responses based on (initial) income levels. The idea behind

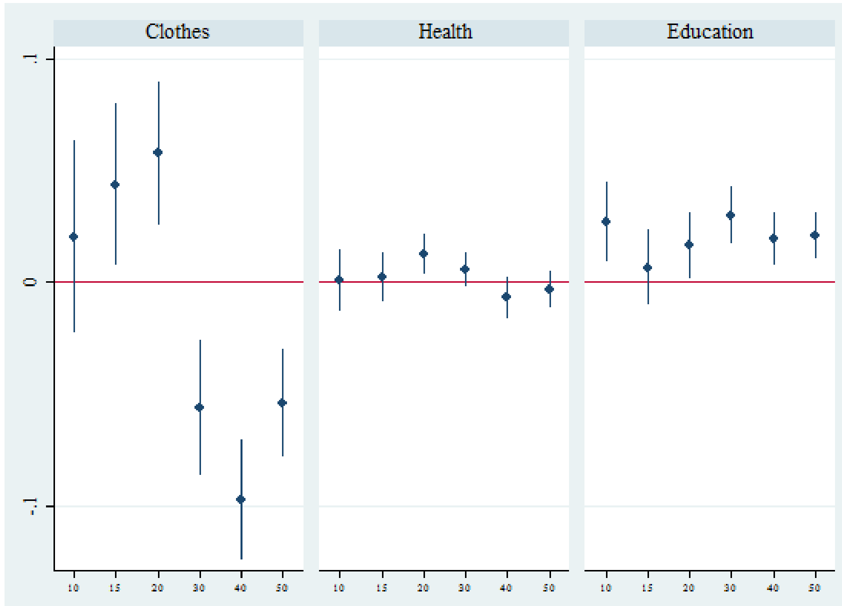

Figure 10: *Consumption shifts for the lowest tercile households (changes in proportions) Clothes, Health and Education:* The dots denote the coefficient of interest which captures the effect of elections, namely, *Treated*After*. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

the exercise is primarily to gain an understanding of how spending on various types of consumption items may be affected when voters receive cash transfers prior to elections. In our setup, we recognise that the consumption adjustments may be different for different income groups.

4.1 The Model

Consider a typical individual with income level y. He can spend his income on two types of consumption goods. For any good in the first category, the utility that the individual gets from consuming depends upon the level of public goods and services in the individual's area of residence.[17] As an example, think of expenditure on education (say buying books for school/college). This "consumption" spending yields higher payoff when there are educational institutions of good quality in the individual's village/town/city. For the second category of goods, the payoff from consumption is independent of the the level of public goods and services in the individual's area of residence. So these are purely private in the sense that their enjoyment does *not* depend upon any type of public service delivery. So many consumption items like food items (rice, meat, etc. or even alcohol and tobacco) and clothes would fall into this category.

[17]This, in turn, depends upon the effort level of the elected politician. More on this later.

We will denote the consumption of the first category by h and the second by c. Let the price of c be normalised to unity and let p denote the (relative) price of h; then the budget constraint is given by the following:

$$c + ph \leq y.$$

To keep the formulation simple, we consider only two possible levels of public goods/services: "high" or "low". Of course, which of the two levels will obtain depends upon how much effort the elected politician exerts. For now assume that π denotes the probability of the "high" level and hence $(1 - \pi)$ the "low" one. As mentioned earlier, the actual utility from consuming any given unit of h is higher when the levels of public goods/services is "high". This is captured by a function $\lambda(.)$ defined over income y such that $\lambda(y) > 1$ for every possible income level y. The overall payoff from consuming the bundle (c, h) is given by

$$u(c) + \pi\lambda(y)v(h) + (1 - \pi)v(h)$$

which can be re-written as

$$u(c) + v(h)[\pi\lambda(y) + (1 - \pi)].$$

Here u and v are strictly increasing and strictly concave functions and $u = v$ is permissible. Hence, given π and y, the individual's problem is to maximise the above objective function subject to his budget constraint.

It is clear that the budget constraint will bind, so we can write c as $y - ph$. Moreover, the objective function is concave in (c, h) and so the first-order conditions are both necessary and sufficient. Note, the FOC w.r.t h is given by the following:

$$v'(h)[\pi\lambda(y) + (1 - \pi)] = pu'(y - ph) \tag{2}$$

From equation (2) we have that there is a unique solution — call it h^* — with $h^* > 0$ as long as $v'(0) = \infty$. To see why, observe that the LHS of equation (2) is falling in h while the RHS is rising in h since both u and v are strictly concave. Moreover, the LHS exceeds the RHS at $h = 0$. So $c^* = y - ph^*$.

Next we examine how the bundle (c^*, h^*) changes with π.

Observation 1. *For any given level of income y, h^* is increasing in π.*

The intuition behind the result above comes from the complementarity between the payoff from h and the level of the public goods/services. An increase in π implies that the marginal utility from consuming h is higher relative to that from c since the magnification effect of $\lambda(y)$ becomes more salient. An immediate corollary of Observation 1 is that c^* falls with π for any given income y.

Consider the function $\lambda(y)$ which captures — for an individual earning y — the complemen-

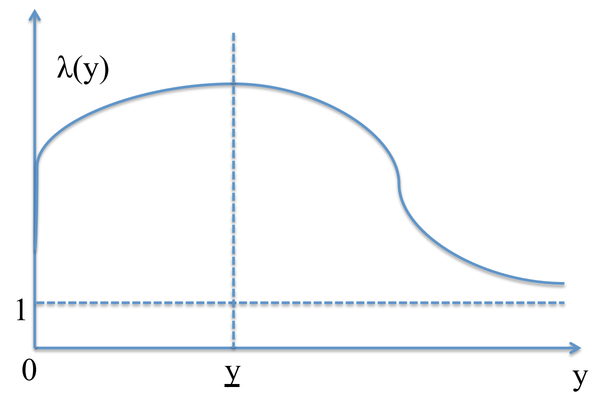

Figure 11: *The variation in returns from h by income:* the shape of $\lambda(y)$.

tarity between utility from h and the level of public goods and services. In principle, this magnification effect could vary with the income y. It is plausible to think of a threshold level of income — call it $\underline{y}$ — such that for all $y \geq \underline{y}$, $\lambda(y)$ is actually weakly decreasing in y while remaining above unity everywhere. The idea is that consuming h yields greater payoff when the level of public goods and service is "high" but this additional gain is lower, higher the income of the individual. To continue with our example of spending on education (books, school supplies), this means that the richer one becomes the *lower* is the dependence on public schools as private schools become affordable. In a sense there is an erosion of the complementarity between this type of (individual) spending and the public investment made by the politician in that area.

In a similar vein, it is possible that for incomes lower than this threshold ($\underline{y}$) the magnification effect is actually rising with incomes. The idea here is that the ultra-poor simply do not have the means to benefit from certain public goods and services as they cannot afford to make the complementary private investments. But as their incomes are augmented, they become more and more able to reap the benefits arising from spending on h goods.

In sum, it is reasonable to assume that $\lambda(y)$ is weakly increasing in y for $y < \underline{y}$ and then becomes weakly decreasing thereafter. We assume that there is at least some $y > \underline{y}$ where $\lambda'(y) < 0$. Recall, we have that $\lambda(y) > 1$ for every y. Figure 11 depicts such a scenario.

A natural benchmark would be the case of $\lambda(y) = \overline{\lambda} > 1$ for every y. In what follows, we will contrast the results with our chosen specification of $\lambda(y)$ with those from the benchmark.

Next we ask how the consumption bundle (c^*, h^*) changes as the budget constraint is relaxed. This could arise due to cash transfers from the competing political parties prior to elections in order to influence the individual's voting behaviour.

Observation **2.** *For any given level of income $y \geq \underline{y}$, c^* increases whenever y increases.*

This is also true in the benchmark case.

Now we ask how h^* changes with income. Again, we begin with $y \geq \underline{y}$. Consider $y_2 > y_1 \geq \underline{y}$. Let the optimal choice of h under y_i be denoted by h_i for $i = 1, 2$. By equation (2), we have

$$v'(h_i) = \frac{pu'(y_i - ph_i)}{\lambda(y_i)\pi + (1 - \pi)}.$$

Notice that $u'(y_2 - ph_2) < u'(y_1 - ph_1)$ by Observation 2 and the strict concavity of u. Also, $\lambda(y_2) \leq \lambda(y_1)$. Therefore, one *cannot* claim that $h_2 > h_1$. So an increase in income starting from $\underline{y}$ and above does not — in general — lead to an increase in consumption of h– category goods. However, for certain values of π an increase in h^* can be guaranteed.

Observation **3.** *For any given level of income $y \geq \underline{y}$, h^* increases with y as long as $\pi < \underline{\pi}$, i.e., π is sufficiently small.*

Observation 3 informs that when an individual is sufficiently pessimistic about the level of public goods provision then he actually *increases* the consumption of the complementary goods when provided with additional income. Although this might seem counter-intuitive, the logic behind the result is quite straight-forward. When the individual's income rises, there are two opposing effects on the consumption of h. First is the standard "income effect" which tends to increase h^*. The other effect arises because of the drop in the magnification factor $\lambda(y)$: this tends to decrease h^*. If the individual believes that π is close to 0, then the rise in income will not decrease the magnification factor $\lambda(y)$ by too much. Hence, the gains from consuming h will not be sufficiently diluted (by the income gain) to actually overturn the income effect. For $\pi \geq \underline{\pi}$, it is not guaranteed that h^* will always increase with y: here it may be that the dilution of the magnification factor can dominate over the income effect.

It is useful to compare this scenario with that of the benchmark case of $\lambda(y) = \overline{\lambda}$. In the latter case, it is always true that h^* increases with y; one does not need to qualify it with any restriction on π. Moreover even for $\pi < \underline{\pi}$, the increase in h under the benchmark is higher because there is no dilution in terms of a dip in $\lambda(y)$.

The above discussion is summarised in the following proposition.

Proposition **1.** *Consider any level of income $y \geq \underline{y}$. For an individual earning y, any increase in income will definitely result in an increase in c^*. There may be an increase in h^* but that will be strictly lower than under the benchmark scenario. So c^*/ph^* is higher than under the benchmark. This asymmetry is more pronounced when individuals are more optimistic, i.e., $\pi \geq \underline{\pi}$.*

We now focus on the case where incomes are lower than the threshold $\underline{y}$. Here $\lambda'(y) \geq 0$. To see how the consumption of h changes with y consider $\underline{y} > y_2 > y_1$. Let the optimal choice of h under y_i be denoted by h_i for $i = 1, 2$. By equation (2), we have

$$v'(h_i) = \frac{pu'(y_i - ph_i)}{\lambda(y_i)\pi + (1 - \pi)}.$$

Suppose $y_2 - ph_2 \leq y_1 - ph_1$. Then immediately it follows that $h_2 > h_1$ since $y_2 > y_1$ by definition. Now suppose $y_2 - ph_2 > y_1 - ph_1$. Then $u'(y_2 - ph_2) < u'(y_1 - ph_1)$ by the strict concavity of u. Also, $\lambda(y_1) \leq \lambda(y_2)$. Therefore, $v'(h_1) > v'(h_2)$ and we have $h_2 > h_1$ by the strict concavity of v.

Unlike in the case where $y \geq \underline{y}$, h^* unambiguously increases with income for all $\pi \in [0, 1]$. This basically stems from the fact that there is no dilution of the magnification effect; in fact, the magnification effect of λ if anything grows *stronger* with increasing income and thus reinforces the standard "income effect". This discussion forms the basis of the the following proposition.

PROPOSITION **2.** *Consider any level of income $y < \underline{y}$. For an individual earning y, any increase in income which keeps him below $\underline{y}$ will definitely result in an increase in h^*. There may be an increase in c^* but that will be strictly lower than under the benchmark scenario. So c^*/ph^* is lower than under the benchmark. This asymmetry is more pronounced when individuals are more optimistic, i.e., for higher π.*

Propositions 1 and 2 record the distortion — vis-a-vis the benchmark — in the consumption pattern of h^* (alternatively, c^*) when an individual faces an income change. What is particularly notable is the *direction* of the distortion on either side of the income threshold of $\underline{y}$. Moreover, this is accentuated when individuals are more optimistic about the efficacy of the public goods/services which is — in turn — a reflection of the efforts of the politician.

Armed with these results we can tackle the question of how cash transfers to voters, prior to elections, affect their spending patterns. Any such transfer made with the intention of influencing the voting behaviour of any individual has precisely the effect of a one-time increment in income. For sure, an individual may choose to save the entire amount in which case one would not record any change in the consumption level and patten. But with the premise that at least *some* part of the incremental income is devoted to consumption, we cannot escape the realm of Propositions 1 and 2.

4.1.1 The determination of π

So far we have treated π, the probability that the level of public goods/services is "high", as exogenous. Indeed, from the perspective of an individual voter optimally deciding on his consumption bundle, it is. However, this probability does get determined endogenously via the politician's effort. In the context of vote-buying prior to elections, *which* politician are we referring to: the incumbent or the eventual winner in the election? It is the eventual winner that determines the level of public goods and services subsequent to elections, so one can think of the individual voters as choosing their consumption bundle as a best response to the effort level they expect from this politician.

Suppose that effort level e by a politician is between 0 and 1 with effort being costly. Let $c(e)$ be a twice differentiable, strictly convex and increasing function with $c(0) = 0$ and $c'(1) = \infty$. Higher the effort, the greater the possibility that the level of public goods/services is "high". Specifically, we can treat the chosen level of e — call it e^* — as the probability that the level of public goods/services is "high". So $e^* = \pi$.

Why would an elected politician exert costly effort? Effort generates a higher level of public goods and services. By Observation 1, this implies a greater spending on h category goods. If spending on such goods actually increases the productivity levels of the individuals (say, by making them more skilled via education or healthier via better provision of medical facilities) then their earning capacities increase and the area as a whole prospers. Now this may be intrinsically valuable to a politician. For the more cynically-minded, this may have a functional importance: a more prosperous populace implies more rents to be captured by the ruling politician. So a higher e induces a higher h and this increases the return to the politician. A simple reduced form way of capturing this is to assume that the returns from effort to a politician is given by a increasing and (weakly) concave function $R(e)$ with $R'' \leq 0$ and $R(0) = 0$.

The politician's problem is to maximise $R(e) - c(e)$ by choosing e. The solution e^* satisfies the FOC given by $R'(e) = c'(e)$. Since $c'(1) = \infty$, we have $e^* \in (0, 1)$.

Clearly the returns depend upon other factors, the extent to which the place is electorally "swing" being one of them. One simple way to include that aspect would be to redefine the total returns as $\rho R(e)$ where $\rho > 1$ is a measure of the electoral worth arising from being swing (think of the worth of a non-swing district as being normalised to unity). Here the marginal return to effort (i.e., $\rho R'(e)$) is higher the larger the parameter ρ. Hence, this factor would have a level effect on π; specifically, more "swing" implies e^* and hence π is higher. In terms of our results (Propositions 1 and 2), this reflects a more pronounced asymmetry in the pattern of consumption relative to the benchmark for income levels on either side of the threshold $\underline{y}$. Additionally, one would expect greater amounts of vote-buying and hence income increments in swing areas which further serves to re-inforce the asymmetry.

In sum, our theory demonstrates how cash transfers to voters may bring about varied consumption adjustments among the recipients based on their income levels *even in a setup devoid of any behavioural biases*. In particular, poorer individuals tend to disproportionately increase spending on goods which are complementary to public service provision. The others tend to spend disproportionately more on pure consumption items which are independent of the level of public goods and services. Interestingly, such divergence obtains in the face of similar expectations about the level of public goods/services provision.

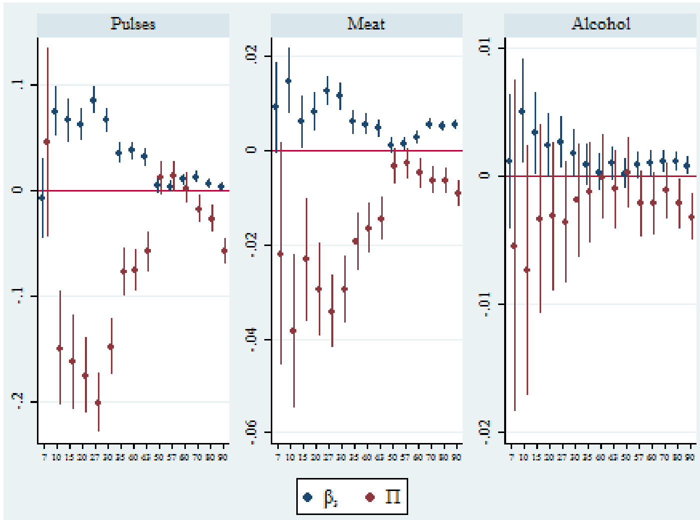

Figure 12: *Consumption shifts: Political competition (changes in proportions) Pulses, Fish/Meat and Intoxicants:* The dots denote the two sets of coefficients of interest: the baseline one in blue, namely, *Treated*After* and that of political competition, namely, *Treated*After*Voteshare* in red. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

5 Robustness checks: Political Competition

One implication of our theory is that places which are more "valuable", electorally-speaking, should witness greater inflow of cash prior to elections and as a consequence, exhibit more pronounced adjustments in consumption. Moreover, the asymmetries between the rich and the non-rich households should become — if anything — more salient. The political economy literature (e.g., Lindbeck and Weibull (1987), Dixit and Londregan (1996, 1998) and more recently, Arulampalam et al (2009), Mitra and Mitra (2016) among others) documents the importance of "swing" electoral districts — those areas where neither of the contesting parties are assured of victory — on the nature of targeting of economic benefits. By the same logic, one may hypothesize that swing districts might actually also witness higher levels of vote-buying.[18]

We examine this possibility empirically. The basic goal is to check if these consumption spikes are at all affected by the district being electorally "swing", and if they are, to ascertain in which direction the effect goes. To ensure brevity and the easy comparison with earlier results, we simply report the outcomes in terms of proportions rather than absolute shifts in consumption.

Figure 12 pertains to the overall sample and contains two sets of coefficients for each of the three categories: pulses, fish/meat and intoxicants. This is done for a range of window sizes,

[18]In terms of our model, more swing means a higher value for the parameter ρ.

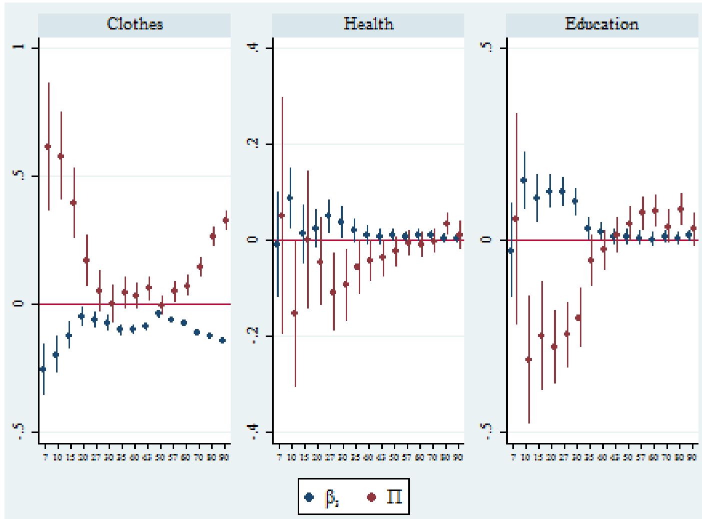

Figure 13: *Consumption shifts: Political competition (changes in proportions) Clothes, Health and Education:* The dots denote the two sets of coefficients of interest: the baseline one in blue, namely, *Treated*After* and that of political competition, namely, *Treated*After*Voteshare* in red. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

as before.The first set of numbers are the coefficients on our familiar variable *Treated*After*; the second set is the interaction of this term with the *Winner's voteshare* variable. The idea is that the coefficient on *Treated*After*Winner's voteshare* will indicate the effect higher political competition — as captured by a lower value of *Winner's voteshare* — has on the household's consumption pattern circa election time. Figure 13 contains the analogous plots for the other three categories: clothes, health and medical expenditures.[19]

From the figures 12 and 13, it is clear that the original sign and significance of *Treated*After* continues to prevail for all the six categories of consumption. By and large, the coefficients are positive and significant for pulses, fish/meat, intoxicants, health and education (like before). For clothes, this turns negative and significant. Moreover, the coefficient on *Treated*After*Winner's voteshare* for clothes is positive and significant for some of the (shorter) windows; this suggests that the consumption spikes for clothes is actually larger in places where the winner achieves a convincing victory. In all the other categories, this coefficient is negative whenever significant and thus in line with our intuition and our theory.

Moving on to the sub-sample analysis, we first document the patterns for the landless households. Figure 14 is the exact counterpart of Figure 12. Here we note that for pulses, the effect if anything is stronger than in the baseline with the coefficient on *Treated*After* being positive and highly significant and that on *Treated*After*Winner's voteshare* being negative and highly significant. The case of fish/meat consumption is quite similar. For intoxicants,

[19]The regression tables on which these two sets of figures are based can be found in the Appendix.

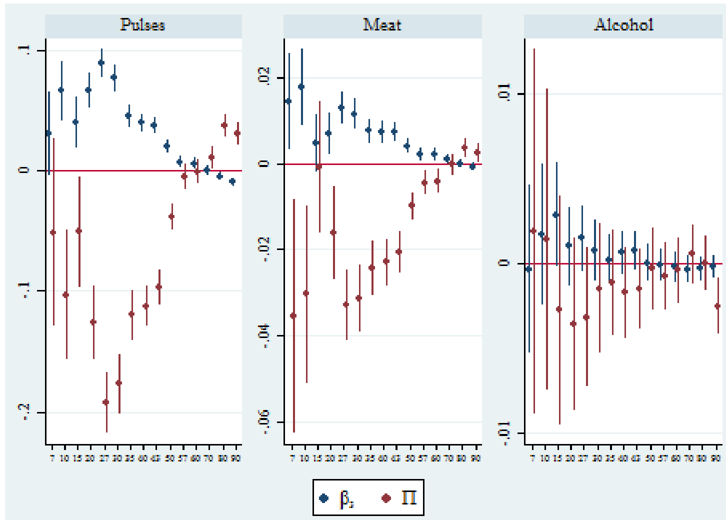

Figure 14: *Consumption shifts for landless households: Political competition (changes in proportions) Pulses, Fish/Meat and Intoxicants:* The dots denote the two sets of coefficients of interest: the baseline one in blue, namely, *Treated*After* and that of political competition, namely, *Treated*After*Voteshare* in red. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

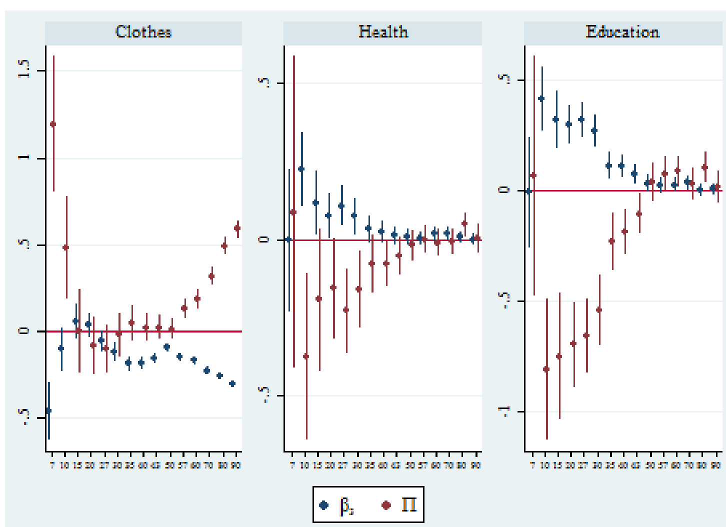

Figure 15: *Consumption shifts for landless households: Political competition (changes in proportions) Clothes, Health and Education:* The dots denote the two sets of coefficients of interest: the baseline one in blue, namely, *Treated*After* and that of political competition, namely, *Treated*After*Voteshare* in red. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

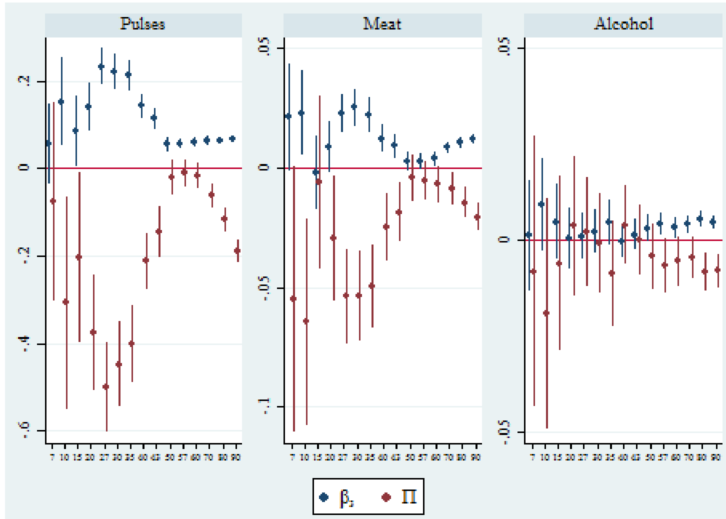

Figure 16: *Consumption shifts for the lowest tercile households: Political competition (changes in proportions) Pulses, Fish/Meat and Intoxicants:* The dots denote the two sets of coefficients of interest: the baseline one in blue, namely, *Treated*After* and that of political competition, namely, *Treated*After*Voteshare* in red. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

the effects are like in the baseline with the coefficient on *Treated*After*Winner's voteshare* being insignificant for the various window sizes we examined. Turning to Figure 15, we find that for clothes, the landless households exhibit a pattern similar to the baseline one, with the coefficient on *Treated*After*Winner's voteshare* being insignificant for several window sizes. The pattern for health expenditures is more pronounced than in the baseline as our intuition suggests and the same is true for education-related expenditures.

Figures 16 and 17 record the analogous plots for the households belonging to the lowest tercile. The overall pattern is similar to what is observed for the landless households: either the effects on *Treated*After* are accentuated vis-a-vis the baseline results with typically a negative coefficient on *Treated*After*Winner's voteshare* or they are similar to the baseline with the coefficient on *Treated*After*Winner's voteshare* being insignificant.

On the whole, it appears that political competition exaggerates the impact of vote-buying in some cases (like pulses, fish/meat consumption and in some cases health and education) and does nothing in some others. The only exception is the category for clothes, where increased political competition in fact leads to a lowering of expenditure (in proportion terms).

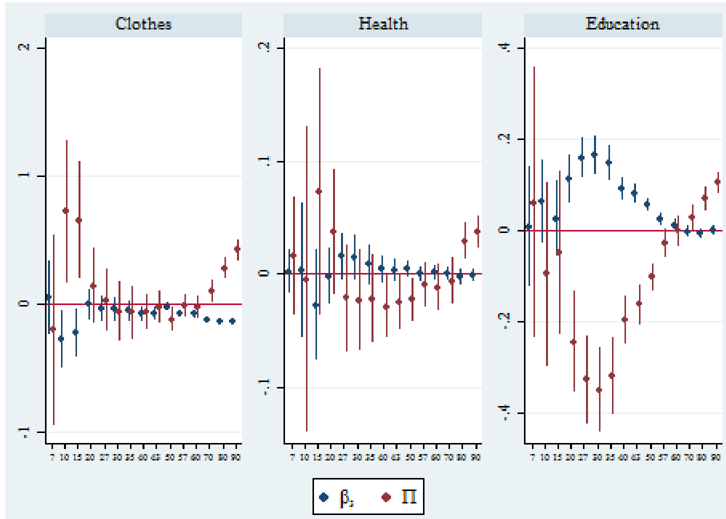

Figure 17: *Consumption shifts for the lowest tercile households: Political competition (changes in proportions) Clothes, Health and Education:* The dots denote the two sets of coefficients of interest: the baseline one in blue, namely, *Treated*After* and that of political competition, namely, *Treated*After*Voteshare* in red. The 95% confidence interval is also shown. The horizontal axis denotes the different window sizes (in *days*).

6 Relation to the "hidden economy"

Our findings have clearly pinpointed some movements in household-level consumption patterns around the time of elections. Typically, we have observed a rise in the absolute level of consumption for a broad category of items. Some of the crucial questions which arise here are the following: *what is the reason behind these spikes? What drives them?* We have tried to argue that this behaviour on the households' part is brought about by either direct cash inflows from political parties or increased supply of these items by them; so — in effect — some form of "vote-buying". This begs the question as to where do these cash injections come from? Who finances them? Clearly somebody has to pay for these transfers given the economist's time-honoured and well-vindicated axiom of "no free lunch".

One obvious source of funds could well be legitimate re-targeting of social programs, employment-based poverty reduction schemes (such as the National Rural Employment Guarantee Scheme (NREGA)). Also, the state-level ministers (Members of the Legislative Assemblies (MLAs)) do have funds (collectively known as "Members of the Legislative Assemblies Local Area Development funds" abbreviated as MLALADs) at their disposal which are meant to be spent on developing their respective constituencies. Perhaps it is these funds which are being (mis)used to influence voters?

Our analysis does not rule out this possibility; indeed, it may very well be a source of these "vote-buying" funds.[20] What we will now examine is whether these funds collectively *can* explain the total rise in the consumption of the households around elections. To get a rough estimate of the monetary equivalent of the rise in consumption, we consult Table 2which contains our results for the movements in monthly per-capita expenditure at the household level. For window sizes of 30 onwards, we find a positive and significant coefficient on *Treated*After* (see columns (4)—(6)). Considering the most conservative estimate among these leaves us with an amount of $15,589.07$ INR. In other words, the consumption of a household on average rises by an amount in excess of $15,000$ INR. Can such a rise be solely finances by MLALADs?

The maximum amount available to an MLA (under the MLALADs scheme) is 40 million INR per year. Assuming that the MLA accumulates all of this over the 5-year term for vote-buying, yields 200 million INR. Given that an average state assembly constituency has a population ranging from 0.2 to 0.3 million, this implies at most 1000 INR per-capita for vote-buying. This is far below our estimate of $15,589.07$, thus implying that there *must* be unaccounted sources of funds which are plowed into these vote-buying activities. In other words, a significant portion of this vote-buying is sponsored by what is termed as "black money" or the "hidden economy". Once again, on the basis of our conservative estimate, we can say that on average in a state assembly constituency about 2900 million INR comes from the "hidden economy".

What does this figure mean for the country as a whole? India has a total of 4120 state assembles.[21] To be sure, all of these constituencies do not go to elections at the *same* time.[22] But suppose they did. What would such a nation-wide polling imply in terms of exhibition of black money? Straightforward calculation yields that the component of the hidden economy' which shows up in vote-buying is approximately 12 trillion INR. This is about 9% of India's GDP (which is approximately 134 trillion INR). Moreover, this is only a partial measure of the "black economy", i.e, the part that is active in political vote-buying; the entire "hidden economy" is possibly much larger. In fact, our estimate of 9% is not inconsistent with Chaudhuri et al. (2006) who find that the hidden economy amounts to about 20.35% of India's GDP for the period 1974/5–1995/6.

7 Conclusion

In this paper, we take a close look at the issue of vote-buying in a thriving democracy. Given that the practice of vote-buying is illegal, there is a clear paucity of hard evidence on the matter. We circumvent this challenge by focusing on a related but different channel: the

[20] Although, anecdotal evidence suggests that these MLALAD funds are often under-utilised.

[21] This information is available at the Election Commission of India's website. Follow link: http://eci.nic.in/eci_main1/seat_in_legislativeassembilies.aspx

[22] In fact, if they did, our identification strategy would require substantial modifications.

effect of elections on the consumption pattern of households. The aim is to observe the household's consumption expenditure on various items and track any changes which might occur around elections. The timing of state assembly elections being decided independently of the NSSO household-level consumer expenditure surveys allow for a clear identification of these election-specific effects.

Our results confirm the suspicions which hound most political economy scholars: there is a clear spike in the consumption of several categories of items for a vast majority of households. This is true for several time windows (which determine which households are to be considered "close" to elections in a chronological sense, and which ones are not) The categories range from various food items (including liquor) to clothes to even health- and education-related goods and services. Moreover, the responses of the households are quite heterogeneous based on some household characteristics: specifically, the poorer households behave differently from their non-poor counterparts. We provide a simple theory to account for such persistent differences.

At a very basic level, our study engages with the question of how households behave when they receive money, aside from their regular earnings. Of course, one has to account for the kind of money injections: temporary or permanent, legal versus illegal, and so on. In general, there is limited understanding of the choice of expenditure items when households are exposed to unexpected income; our work sheds some light on this.

Our analysis can — in principle — be extended to address the issue of clientelism. It may be possible to check if areas where the incumbent politician/party returns to power exhibit a different level and pattern of consumption as compared to where the incumbent loses. Nonetheless, the very evidence of vote-buying can be turned on its head to query about the sources of such funds. We perform a rough back-of-the-envelope calculation on the basis of our regression estimates and find that these funds constitute over 9% of India's GDP and simply cannot be accounted for in terms of the legal sources of funds available to such politicians (eg., MLALAD funds). This figure of 9% should be considered at best a partial estimate of the hidden economy in India. We clearly do not capture the myriad ways in which "black money" flows in the many arteries of a nation's economic networks.

Our work bears implications for the debate on election financing in general and underlines the question of whether public funding of elections is a necessity in developing countries.

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Appendix

Tables

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	324.301***	299.592***	180.408***	25.598***	11.330**	2.995
	(22.980)	(18.208)	(12.282)	(6.313)	(4.796)	(4.274)
After	-26.006***	-18.190***	-10.386**	-0.962	7.625**	15.438***
	(5.186)	(4.448)	(4.284)	(3.585)	(3.128)	(2.940)
Treated*After	25.398***	-8.750	-1.133	55.658***	48.068***	33.484***
	(9.685)	(8.213)	(7.728)	(7.079)	(6.264)	(5.617)
Hindu	11.905**	14.486***	16.684***	17.289***	14.656***	12.266***
	(5.870)	(5.119)	(4.710)	(4.010)	(3.581)	(3.336)
General caste	43.336***	50.558***	44.775***	45.708***	47.472***	50.883***
	(4.726)	(4.083)	(3.798)	(3.212)	(2.851)	(2.638)
Rural	-10.647**	-8.361**	-5.568	-9.761***	-11.392***	-9.673***
	(4.590)	(3.929)	(3.612)	(3.071)	(2.739)	(2.547)
Household Size	52.231***	52.369***	53.988***	52.275***	51.851***	51.039***
	(1.195)	(1.067)	(1.047)	(0.882)	(0.771)	(0.691)
Monthly per-capita exp. (log)	-82.602***	-81.083***	-79.641***	-78.951***	-79.427***	-78.744***
	(0.694)	(0.630)	(0.615)	(0.530)	(0.469)	(0.437)
Treated*Voteshare	-997.749***	-871.228***	-603.567***	-340.847***	-297.991***	-268.371***
	(52.765)	(41.507)	(28.256)	(16.678)	(13.101)	(10.948)
Constant term	802.618***	782.166***	761.704***	757.922***	761.613***	755.354***
	(11.145)	(9.751)	(9.374)	(7.951)	(7.202)	(6.670)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.322	0.319	0.308	0.304	0.305	0.301

Table 9: *D-D regressions: Pulses (absolute terms, units: INR).* The dependent variable in every column is the household's monthly per-capita expenditure on pulses. This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	57.382***	58.339***	23.125***	-21.236***	-25.040***	-30.065***
	(11.355)	(10.098)	(6.637)	(3.480)	(2.790)	(2.692)
After	2.467	14.648***	13.268***	5.482***	4.187**	2.704*
	(3.027)	(2.350)	(2.199)	(1.875)	(1.657)	(1.585)
Treated*After	-8.589*	-21.368***	-13.472***	-1.483	-0.508	3.861
	(4.678)	(3.912)	(3.827)	(6.049)	(5.014)	(2.899)
Hindu	-68.531***	-72.763***	-73.300***	-76.424***	-74.872***	-75.651***
	(4.590)	(3.862)	(3.462)	(3.341)	(2.872)	(2.640)
General caste	24.295***	20.146***	17.432***	16.545***	16.545***	17.887***
	(2.828)	(2.365)	(2.151)	(1.847)	(1.625)	(1.527)
Rural	-21.384***	-18.331***	-16.993***	-17.800***	-19.744***	-19.094***
	(2.607)	(2.180)	(1.976)	(1.703)	(1.513)	(1.425)
Household Size	6.203***	6.206***	6.270***	6.331***	6.221***	5.928***
	(0.485)	(0.416)	(0.388)	(0.327)	(0.327)	(0.291)
Monthly per-capita exp. (log)	-11.461***	-10.563***	-10.086***	-9.023***	-9.225***	-8.970***
	(0.306)	(0.272)	(0.264)	(0.339)	(0.289)	(0.236)
Treated*Voteshare	-214.742***	-191.266***	-114.835***	-25.363	-16.646	-15.056**
	(25.906)	(22.811)	(15.077)	(17.710)	(13.455)	(7.534)
Constant term	209.836***	196.138***	193.285***	192.600***	195.730***	197.306***
	(5.810)	(4.665)	(4.314)	(3.596)	(3.297)	(3.097)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.054	0.055	0.052	0.046	0.047	0.047

Table 10: *D-D regressions: Fish+Meat (absolute terms, units: INR).* The dependent variable in every column is the household's monthly per-capita expenditure on fish and meat. This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	4.197	0.192	-1.817	-3.422	-2.445	-2.186
	(6.630)	(5.102)	(3.401)	(2.220)	(1.633)	(1.726)
After	-10.224***	-8.053***	-5.169***	-4.431***	-1.213	-0.370
	(1.727)	(1.430)	(1.366)	(1.182)	(0.989)	(1.097)
Treated*After	8.415***	8.276***	4.796**	2.911	1.031	0.251
	(2.920)	(2.357)	(2.270)	(2.116)	(1.832)	(1.755)
Hindu	1.796	1.183	0.612	-0.711	-0.218	-0.229
	(1.796)	(1.582)	(1.472)	(1.343)	(1.161)	(1.133)
General caste	-4.879***	-4.672***	-5.615***	-4.922***	-4.513***	-3.380***
	(1.431)	(1.246)	(1.148)	(1.008)	(0.885)	(0.994)
Rural	-1.767	-1.637	-0.340	0.138	0.175	0.062
	(1.444)	(1.262)	(1.165)	(1.012)	(0.881)	(0.923)
Household Size	0.787***	0.929***	1.007***	0.897***	0.953***	0.928***
	(0.224)	(0.196)	(0.182)	(0.153)	(0.142)	(0.130)
Monthly per-capita exp. (log)	-2.145***	-1.964***	-1.902***	-1.759***	-1.875***	-1.677***
	(0.116)	(0.107)	(0.109)	(0.108)	(0.099)	(0.126)
Treated*Voteshare	-27.023*	-21.042*	-11.710	-8.215	-5.934	-6.767*
	(15.143)	(11.351)	(7.747)	(5.345)	(4.159)	(3.746)
Constant term	39.081***	36.922***	35.571***	36.185***	33.933***	32.226***
	(2.707)	(2.278)	(2.124)	(1.891)	(1.709)	(1.706)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.004	0.004	0.003	0.003	0.003	0.002

Table 11: *D-D regressions: Intoxicants (absolute terms, units: INR).* The dependent variable in every column is the household's monthly per-capita expenditure on intoxicants. This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	-1,381.595***	-1,231.129***	-1,128.491***	-674.147***	-605.102***	-638.950***
	(114.596)	(92.695)	(55.584)	(36.921)	(29.224)	(27.142)
After	95.006***	74.182***	49.867**	35.126*	-2.525	-158.415***
	(29.019)	(24.222)	(22.177)	(19.173)	(17.090)	(16.406)
Treated*After	148.223***	138.119***	96.475**	-302.898***	-321.221***	-174.113***
	(55.332)	(47.239)	(42.958)	(39.210)	(34.109)	(32.310)
Hindu	39.541	23.382	20.405	1.374	-13.982	-22.184
	(34.558)	(30.513)	(27.720)	(23.597)	(21.252)	(20.233)
General caste	495.172***	504.170***	503.251***	468.838***	469.136***	483.763***
	(28.251)	(24.639)	(22.173)	(18.962)	(16.817)	(15.867)
Rural	-571.081***	-540.099***	-541.286***	-480.529***	-481.649***	-509.718***
	(26.593)	(22.999)	(20.739)	(17.594)	(15.596)	(14.900)
Household Size	195.104***	197.311***	197.121***	197.689***	198.527***	202.986***
	(6.189)	(5.247)	(4.801)	(4.104)	(3.726)	(3.700)
Monthly per-capita exp. (log)	-128.108***	-114.087***	-103.834***	-84.663***	-80.473***	-77.395***
	(3.351)	(3.093)	(2.914)	(2.648)	(2.343)	(2.317)
Treated*Voteshare	3,356.694***	2,866.243***	2,645.269***	2,420.795***	2,326.626***	2,234.375***
	(274.731)	(222.217)	(142.095)	(99.690)	(78.720)	(71.266)
Constant term	1,150.434***	1,046.162***	975.373***	815.650***	804.153***	905.650***
	(57.904)	(50.516)	(46.128)	(39.765)	(35.703)	(34.457)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.130	0.131	0.132	0.133	0.135	0.140

Table 12: *D-D regressions: Clothes (absolute terms, units: INR).* The dependent variable in every column is the household's monthly per-capita expenditure on clothes. This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	72.502	178.880	-57.600	-98.102	-96.771	-130.915**
	(279.730)	(257.077)	(148.220)	(105.635)	(78.412)	(59.849)
After	-85.070*	-66.240	-27.416	-97.954**	-65.855*	-28.944
	(48.980)	(47.483)	(41.597)	(48.973)	(37.617)	(34.968)
Treated*After	107.803	90.127	94.455	25.190	-33.982	-31.986
	(110.016)	(90.565)	(78.396)	(91.931)	(75.506)	(64.723)
Hindu	2.670	45.237	29.071	20.830	14.723	31.500
	(79.120)	(64.962)	(56.686)	(59.267)	(48.255)	(44.284)
General caste	186.591***	192.655***	192.503***	217.645***	178.467***	219.984***
	(51.175)	(46.475)	(40.484)	(45.280)	(36.590)	(35.849)
Rural	47.349	10.768	16.768	15.224	-7.433	-20.027
	(43.472)	(41.811)	(35.683)	(37.102)	(30.490)	(29.195)
Household Size	69.958***	71.708***	71.345***	90.114***	79.572***	80.888***
	(10.738)	(10.582)	(9.463)	(11.289)	(9.011)	(8.618)
Monthly per-capita exp. (log)	12.185**	23.754***	25.355***	42.720***	38.179***	41.887***
	(5.094)	(6.230)	(5.764)	(7.664)	(6.069)	(5.805)
Treated*Voteshare	-342.102	-699.348	-120.498	77.493	243.896	278.556**
	(711.991)	(596.343)	(343.393)	(221.592)	(167.301)	(125.584)
Constant term	-221.893**	-314.204***	-349.766***	-500.764***	-427.229***	-483.561***
	(103.662)	(95.577)	(85.060)	(100.189)	(82.447)	(86.879)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.004	0.004	0.004	0.005	0.004	0.005

Table 13: *D-D regressions: Health (absolute terms, units: INR).* The dependent variable in every column is the household's monthly per-capita expenditure on health-related items. This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	156.287	-73.515	-440.517***	-375.669***	-371.793***	-379.100***
	(223.665)	(182.918)	(114.878)	(82.423)	(80.200)	(71.460)
After	-64.736	5.516	54.035	-30.347	-158.504***	-258.498***
	(59.088)	(49.410)	(44.508)	(40.134)	(45.739)	(43.921)
Treated*After	60.973	-22.900	97.870	88.887	88.268	183.703***
	(124.388)	(104.424)	(92.590)	(82.189)	(75.432)	(68.500)
Hindu	182.249**	195.142***	191.129***	213.723***	125.897*	135.682**
	(70.925)	(62.723)	(55.627)	(47.513)	(70.812)	(63.522)
General caste	671.755***	682.337***	685.098***	676.238***	650.802***	698.004***
	(61.868)	(53.282)	(47.703)	(41.131)	(44.621)	(41.071)
Rural	-898.017***	-913.477***	-883.855***	-886.800***	-880.233***	-923.491***
	(57.235)	(49.659)	(43.940)	(37.990)	(40.742)	(37.466)
Household Size	172.623***	167.522***	162.865***	169.981***	167.941***	169.767***
	(12.236)	(10.087)	(8.897)	(7.830)	(7.249)	(6.620)
Monthly per-capita exp. (log)	-13.500**	0.849	7.293	22.982***	26.506***	29.475***
	(6.172)	(5.892)	(5.437)	(5.562)	(7.760)	(7.286)
Treated*Voteshare	-506.583	128.634	776.674**	650.146***	597.146***	357.809**
	(531.005)	(450.019)	(303.280)	(214.393)	(176.012)	(148.855)
Constant term	178.943	68.154	-19.509	-120.940	28.110	103.640
	(110.861)	(95.954)	(86.492)	(79.840)	(83.958)	(77.142)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.032	0.033	0.033	0.034	0.024	0.027

Table 14: *D-D regressions: Education (absolute terms, units: INR).* The dependent variable in every column is the household's monthly per-capita expenditure on education-related items. This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	0.006	0.002	-0.012**	-0.032***	-0.030***	-0.025***
	(0.008)	(0.007)	(0.005)	(0.003)	(0.002)	(0.002)
After	-0.007***	-0.003**	0.001	0.000	0.001	0.000
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treated*After	0.075***	0.066***	0.062***	0.067***	0.037***	0.004
	(0.012)	(0.010)	(0.008)	(0.006)	(0.004)	(0.003)
Hindu	-0.005***	-0.003*	-0.002	-0.002	-0.001	-0.001
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
General Caste	-0.026***	-0.026***	-0.027***	-0.027***	-0.027***	-0.027***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Rural	0.044***	0.043***	0.042***	0.039***	0.040***	0.042***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Household Size	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated*Voteshare	-0.106***	-0.070***	-0.018	0.001	-0.004	-0.027***
	(0.019)	(0.015)	(0.011)	(0.007)	(0.005)	(0.005)
Treated* After*Voteshare	-0.150***	-0.163***	-0.176***	-0.148***	-0.076***	0.011
	(0.027)	(0.023)	(0.018)	(0.013)	(0.010)	(0.008)
Constant	0.125***	0.122***	0.118***	0.123***	0.120***	0.123***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.064	0.062	0.062	0.060	0.062	0.066

Table 15: *D-D regressions (Political Competition): Pulses (Proportion of MPCE).* The dependent variable in every column is the proportion of the household's monthly per-capita expenditure which is spent on pulses. This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	-0.006**	-0.002	-0.006***	-0.011***	-0.010***	-0.010***
	(0.003)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
After	-0.000	0.002***	0.003***	0.001***	0.001***	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated*After	0.015***	0.006**	0.008***	0.011***	0.005***	0.001
	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
Hindu	-0.016***	-0.016***	-0.016***	-0.016***	-0.016***	-0.016***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
General Caste	0.000	-0.001	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Rural	0.004***	0.004***	0.004***	0.004***	0.003***	0.004***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Household Size	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated*Voteshare	-0.009	-0.014***	-0.001	0.005***	0.005***	0.003***
	(0.007)	(0.005)	(0.003)	(0.002)	(0.001)	(0.001)
Treated* After*Voteshare	-0.039***	-0.023***	-0.030***	-0.030***	-0.017***	-0.004*
	(0.008)	(0.007)	(0.005)	(0.004)	(0.003)	(0.002)
Constant	0.041***	0.039***	0.037***	0.039***	0.038***	0.039***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.045	0.045	0.046	0.047	0.044	0.045

Table 16: *D-D regressions (Political Competition): Fish+Meat (Proportion of MPCE).* The dependent variable in every column is the proportion of the household's monthly per-capita expenditure which is spent on fish and meat. This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	-0.002	-0.001	-0.001	-0.001	-0.000	-0.001*
	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
After	-0.002***	-0.002***	-0.001***	-0.001***	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated*After	0.005**	0.003**	0.002*	0.002*	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Hindu	0.001	0.001**	0.001**	0.001***	0.000**	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
General Caste	-0.003***	-0.003***	-0.004***	-0.003***	-0.004***	-0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Rural	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Household Size	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated*Voteshare	-0.000	-0.002	-0.001	-0.002*	-0.002**	-0.002**
	(0.004)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
Treated* After*Voteshare	-0.007	-0.003	-0.003	-0.002	-0.000	0.000
	(0.005)	(0.004)	(0.003)	(0.002)	(0.002)	(0.001)
Constant	0.009***	0.008***	0.008***	0.008***	0.008***	0.007***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.010	0.009	0.009	0.009	0.009	0.009

Table 17: *D-D regressions (Political Competition): Intoxicants (Proportion of MPCE).* The dependent variable in every column is the proportion of the household's monthly per-capita expenditure which is spent on intoxicants. This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	-0.195***	-0.217***	-0.250***	-0.187***	-0.163***	-0.179***
	(0.025)	(0.019)	(0.008)	(0.005)	(0.005)	(0.005)
After	0.032***	0.017***	0.009*	0.007	0.003	-0.038***
	(0.007)	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)
Treated*After	-0.194***	-0.120***	-0.048**	-0.072***	-0.095***	-0.036***
	(0.035)	(0.028)	(0.020)	(0.015)	(0.009)	(0.007)
Hindu	-0.020***	-0.019***	-0.015***	-0.020***	-0.020***	-0.022***
	(0.007)	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)
General Caste	0.017***	0.021***	0.021***	0.015***	0.013***	0.013***
	(0.006)	(0.005)	(0.005)	(0.004)	(0.003)	(0.003)
Rural	-0.018***	-0.013***	-0.019***	-0.007*	-0.010***	-0.010***
	(0.006)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)
Household Size	-0.005***	-0.006***	-0.005***	-0.005***	-0.005***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Treated*Voteshare	0.380***	0.405***	0.513***	0.563***	0.545***	0.523***
	(0.062)	(0.045)	(0.022)	(0.015)	(0.015)	(0.014)
Treated* After*Voteshare	0.580***	0.398***	0.173***	0.004	0.034	-0.003
	(0.088)	(0.069)	(0.050)	(0.038)	(0.026)	(0.020)
Constant	0.277***	0.287***	0.283***	0.284***	0.281***	0.321***
	(0.012)	(0.010)	(0.009)	(0.007)	(0.007)	(0.006)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.010	0.009	0.009	0.012	0.014	0.016

Table 18: *D-D regressions (Political Competition): Clothes (Proportion of MPCE).* The dependent variable in every column is the proportion of the household's monthly per-capita expenditure which is spent on clothes. This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	-0.046***	0.007	-0.019**	-0.022***	-0.018***	-0.022***
	(0.015)	(0.022)	(0.009)	(0.005)	(0.004)	(0.004)
After	-0.018***	-0.010**	-0.001	-0.005	-0.004	-0.008**
	(0.007)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)
Treated*After	0.088***	0.013	0.025	0.039**	0.012	0.010
	(0.033)	(0.031)	(0.020)	(0.016)	(0.010)	(0.007)
Hindu	0.001	0.005	0.005	0.001	-0.001	-0.001
	(0.005)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
General Caste	0.009**	0.009**	0.009***	0.009***	0.007***	0.010***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Rural	0.006	0.004	0.002	0.002	0.000	0.000
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)
Household Size	0.000	0.000	0.000	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Treated*Voteshare	0.054	-0.064	0.009	0.028***	0.036***	0.030***
	(0.036)	(0.049)	(0.020)	(0.011)	(0.010)	(0.010)
Treated* After*Voteshare	-0.153**	0.001	-0.045	-0.093**	-0.042*	-0.023
	(0.077)	(0.072)	(0.046)	(0.038)	(0.022)	(0.016)
Constant	0.037***	0.034***	0.026***	0.033***	0.034***	0.039***
	(0.011)	(0.008)	(0.006)	(0.006)	(0.005)	(0.005)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.000	0.000	0.000	0.000	0.000	0.000

Table 19: *D-D regressions (Political Competition): Health (Proportion of MPCE).* The dependent variable in every column is the proportion of the household's monthly per-capita expenditure which is spent on health-related items. This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1]	[2]	[3]	[4]	[5]	[6]
	(10)	**(15)**	**(20)**	**(30)**	**(40)**	**(50)**
Treated	-0.049*	-0.029	-0.083***	-0.068***	-0.053***	-0.052***
	(0.026)	(0.022)	(0.010)	(0.007)	(0.008)	(0.007)
After	-0.008	0.000	0.004	-0.009	-0.024***	-0.039***
	(0.008)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)
Treated*After	0.156***	0.109***	0.128***	0.101***	0.024**	0.010
	(0.038)	(0.032)	(0.022)	(0.018)	(0.012)	(0.011)
Hindu	0.004	0.014*	0.018***	0.023***	0.025***	0.022***
	(0.009)	(0.008)	(0.007)	(0.006)	(0.005)	(0.005)
General Caste	0.057***	0.062***	0.063***	0.060***	0.059***	0.064***
	(0.007)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)
Rural	-0.093***	-0.090***	-0.091***	-0.088***	-0.084***	-0.087***
	(0.007)	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)
Household Size	-0.002*	-0.002**	-0.002*	-0.002***	-0.003***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treated*Voteshare	0.012	-0.011	0.101***	0.062***	0.031*	-0.006
	(0.056)	(0.049)	(0.022)	(0.015)	(0.017)	(0.017)
Treated* After*Voteshare	-0.311***	-0.248***	-0.278***	-0.200***	-0.024	0.044*
	(0.085)	(0.072)	(0.049)	(0.039)	(0.026)	(0.024)
Constant	0.162***	0.149***	0.138***	0.148***	0.158***	0.179***
	(0.016)	(0.012)	(0.011)	(0.010)	(0.009)	(0.009)
Observations	28,037	37,093	44,392	60,398	76,267	88,941
Adjusted R^2	0.014	0.013	0.014	0.012	0.011	0.012

Table 20: *D-D regressions (Political Competition): Education (Proportion of MPCE).* The dependent variable in every column is the proportion of the household's monthly per-capita expenditure which is spent on education-related items. This has a recall period of 30 days from the date of survey. The window size utilises the distance of the date of survey from the date of election for the households and determines the four groups: *treated* and *control* times *before* and *after*. It is denoted in *days* and provided under each column heading. *Hindu, General Caste, Rural* are dummy variables at the household level. *Voteshare* is the percentage of votes obtained by the winner in the state assembly election. All regressions have the robust standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%

Proofs

Proof. [Observation 1.] Suppose not. Say h^* stays unchanged as π goes up. Then the LHS of equation (2) increases as $\lambda(y) > 1$ while the RHS stays unchanged. Hence, equation (2) is no longer satisfied. This contradicts the definition of h^* and rules out h^* not changing with π.
Now suppose h^* falls as π increases. Then the LHS of equation (2) increases while the RHS falls given the strict concavity of u and v. Again, equation (2) is no longer satisfied leading to a contradiction. This completes the proof. ∎

Proof. [Observation 2.] Suppose not. Say c^* stays unchanged as y goes up. This implies h^* increases with y. Then the LHS of equation (2) falls as h^* has increased and $\lambda'(y) \leq 0$ while the RHS stays unchanged. Hence, equation (2) is no longer satisfied.
Now suppose c^* falls as y goes up. Then the LHS of equation (2) falls while the RHS increases owing to the strict concavity of u. This implies that equation (2) is no longer satisfied. This contradiction establishes that c^* increases with y for any given level of income $y \geq \underline{y}$.
Similar arguments apply for the case $\lambda(y) = \overline{\lambda}$. ∎

Proof. [Observation 3.] Consider $\pi = 0$. Here, by equation (2) we have that $h^* > 0$ since $v'(0) = \infty$. Consider $y_2 > y_1 \geq \underline{y}$. Here, for $i = 1, 2$, we have

$$v'(h_i) = \frac{pu'(y_i - ph_i)}{\lambda(y_i)\pi + (1 - \pi)}.$$

Since $u'(y_2 - ph_2) < u'(y_1 - ph_1)$ by Observation 2 and the strict concavity of u, we have $v'(h_2) < v'(h_1)$ for $\pi = 0$. This, in turn, implies $h_2 > h_1$ since v is strictly concave. By continuity there is some $\underline{\pi} > 0$ such that $h_2 > h_1$ for all $\pi < \underline{\pi}$ even with $\lambda(y_2) \leq \lambda(y_1)$. ∎

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