Cash for Votes: Evidence from India[1]

By Anirban Mitra and Shabana Mitra

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Abstract

We examine consumption patterns around the time of elections to investigate vote-buying. Specifically, we combine data from state assembly elections in India with household-level consumer expenditure and employment surveys (conducted by NSSO) over the period 2004-12. Exploiting a difference-in-difference methodology, we estimate the positive effect elections have on reported consumption expenditures. We observe larger effects in swing areas but null effects for the poor. The total wage payments for the same period cannot justify the consumption spikes. Our results shed light on the mechanisms behind strategic vote-buying.

JEL codes: D12, D72, H40.

Keywords: vote-buying, clientelism, consumption patterns

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Anirban Mitra: University of Kent; Shabana Mitra: Shiv Nadar University.

1 Introduction

Elections are highly contested in vibrant democracies and candidates use many different ways to convince voters to vote for them. Some of these methods are integral to the spirit of campaigning, such as choosing a platform, discussing the policies offered and using reach tools like media, rallies and door-to-door canvassing. However, candidates sometimes also resort to methods that are unethical (if not illegal) like exerting physical force, engaging in clientelistic politics or outright vote-buying, to convince voters. These latter methods have come into the limelight recently and are being actively researched. Use of violence to alter voter perceptions has been studied widely (e.g., Varshney (2003), Wilkinson (2006)). The literature on clientelistic politics is also large (see Bardhan and Mookherjee (2016) for an overview). However, the third aspect of 'persuading' voters, namely, the phenomena of vote-buying, is comparatively less well-understood. This very phenomenon is the focus of our study.

While it is acknowledged that vote-buying is rampant in many democracies, measuring it reliably remains a difficult challenge. Given the illegality of the practice, neither political parties nor voters have any incentives for revealing any details regarding the cash/kind that changes hands. The practice creates incentives for opportunistic business agents to both influence election outcomes and enjoy perks by forming clientelistic relations with politicians and political parties that results in the reduction of politician accountability (Leight, Foarta, Pande and Ralston (2020)).[2] Given these serious obstacles, researchers have used experimental evidence to study clientelism and vote-buying (Wantchekon (2003) in Benin, Vicente

[2]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. 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 resources in this sector may be used for vote-buying.

(2014) in Sao Tome and Principe, Finan and Schechter (2012) in Paraguay, and Fergusson et al (2022) in Colombia among others).[3]

Our approach to this complex issue differs from the extant literature in several ways. On the strength of existing studies (e.g., Aidt et al. (2020), Nitcher (2011) and Labonne (2016)), we proceed with the premise that vote-buying and political business cycles *are* indeed common in democracies. In other words, we do not question the salience of this phenomenon. Our focus is on delving deeper into the mechanisms behind these processes. This is rendered possible owing to the advantages offered by our data and research design. We examine (potential) changes in consumption patterns for a wide variety of goods and households around the time of elections to gain a holistic understanding of vote-buying.

Our contributions can be viewed as the following. First, we present evidence in favour of a specific mechanism — namely, that funds are moved *across* space to strategically target swing voters around the time of elections. In this sense, we provide a test (and subsequent vindication) of the classic "swing voter" result from standard models of probabilistic voting in a different dimension of political competition. To be sure, the classic swing voter result applies to the targetting of public funds ('pork barrel') which bear fruit only *after* the winner of the election assumes office. Here, in contrast, the gains to the swing group is more immediate given the clandestine and one-off nature of the (pre-election) transfer.[4] Secondly, we document changes in consumption levels and patterns around elections *across* different socio-economic groups — in particular, to demonstrate if the poor are targetted (and thereby affected) more than the rich. By comparing how funds are thus targeted across income groups, we offer novel insights into strategies behind vote-buying. Thirdly, this study is — to the best of our knowledge — the first to record such patterns for sub-national level elections in the context of a fast-growing, developing democracy like India. Finally,

[3]Relatedly, the evidence of any persuasive effects of vote-buying is quite sparse too. A notable exception is Cantú (2019) who studies this in the context of the 2012 presidential election in Mexico.

[4]This implies that the swing group gain on two fronts — through illicit in-kind/cash transfers (not directly observed) and from targetted public spending (legitimate and observable).

as we utilise large, representative and well-known national level datasets for our analysis, concerns regarding external validity are considerably lower as compared to studies involving experiments/surveys in specific regions within countries.

Our three key sources of data are: (i) the National Sample Survey (NSS) rounds on household consumption expenditure; (ii) the NSS rounds on employment and unemployment where *both* set of rounds have been conducted during 2004–2012; and (iii) the state legislative assembly elections data pertaining to the same period. Each NSS consumption round contains detailed information on the surveyed households' monthly consumption expenditure on over 300 different commodities. Equally, each NSS employment-unemployment round contains detailed information on the individual's employment patterns (days worked, wages received, etc.). 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 these with the data on state assembly elections, we are able to ascertain whether (or not) a household is reporting on consumption (and employment) variables 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/after they voted and those that did so many days before/after voting. We construct 'time windows' around election dates to see how the consumption pattern changes in states with elections (hence, 'treated') and in states without (hence, 'control'). We essentially look for evidence of change in consumption in the'treated' sample relative to the 'control' sample in these different time windows.

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 confound the causal link from elections to changes in expenditure? The empirical strategy we employ takes cognisance of this matter. Our identification strategy relies on exploiting the timing of state-level elections and that of the NSS rounds to establish a causal relationship. The fact that a given NSS round takes a year to complete coupled with that elections can take place in *any* calendar month provides us with a spatial and temporal variation which can effectively account for seasonal patterns in spending.

Our first main observation is that the monthly per-capita consumption expenditure of a typical household is indeed affected by elections. Specifically, we observe that households in a state facing elections when surveyed 3 and 4 months before elections register a statistically significant spike in their consumption expenditure. This is of course relative to the 'control' group which is surveyed in the same time frame. The rise is about 100 INR when averaged across the two months which is nearly 8% of the average household's monthly per-capita expenditure (see Table 3). Interestingly, these effects are statistically significant only a few months ahead of the elections — in particular, the treated households surveyed a month or two before elections do not exhibit any observable change in their consumption expenditure. This is consistent with some anecdotal evidence reported in the media, especially concerning the timing of seizures by the police of cash and 'gifts' much ahead of election dates.[5]

We also check for the effect on six major categories of consumption derived from the NSS, which are a large and important share of the average household's monthly expenses. There are similar increases for several of them. Particularly, the spending on fish and meats shows a pronounced and persistent rise continuing till about a month after elections (see Figure 12). The patterns for spending on intoxicants (including various types of liquor), clothes and even health and education-related expenditures are similar to those observed for MPCE. These findings resonate with Aidt et al's (2020) results for Armenia although their proposed channel of vote-buying works through the manipulation of money supply.[6]

[5] For example, the following article in *The Economic Times* (a leading Indian English-language business-focused daily newspaper) cites the case of several states. See: https://economictimes.indiatimes.com/news/politics-and-nation/political-parties-wooing-%20electorates-with-liquor-cash-household-bene%0Cts-and-drugs/printarticle/51858290.cms

[6] Their main finding is that the monthly growth rate of the money supply (M1) around elections is higher than in other months in a sample of low- and middle-income countries.

By exploiting the richness of the NSS houshehold-level datasets on consumption expenditure and employment, we are able to isolate the differential effects across different socio-economic groups — particularly, as mentioned above, the asymmetry between (a) households living in politically "swing" and non-swing areas and (b) poor and the non-poor households. This line of investigation helps one obtain a more fuller understanding of the mechanisms behind vote-buying. One can explore whether vote-buying is more actively pursued among the incumbent loyalists thus complementing the post-election favoritism towards swing voters, or whether it simply supplements the perquisites to the swing groups. In a similar spirit, one can explore whether poorer voters – regardless of their political ideology – are targetted differently from others.

As regards (a), we examine whether these spikes are affected by whether the district is electorally swing or not as measured by the difference in the voteshares of the winner and the runner-up. The intuition from probabilistic voting models suggest that the spikes and more importantly the divergent patterns across socio-economic groups should be amplified in such swing areas. Indeed, we find corroboration of that idea in our analysis — higher political competitiveness re-enforces the baseline effect.

In relation to (b), we attempt to uncover which parts of the income distribution drive the main results. For that purpose, we look at some specific socio-economic groups separately. Given the data constraints, we focus on homeownership as a measure of household assets which is plausibly exogenous to any potential vote-buying related transfers. We find that it is actually the homeowning households that drive the baseline results. There is practically *no* effect on the treated households who do not own their homes. This does seem to suggest that the poor are actually not being targeted with pre-election transfers.

We also look at the effect in states where vote-buying is supposed to be more prevalent – specifically, the three populous states of Bihar, Tamil Nadu and Uttar Pradesh – and find that the effects on consumption expenditure are larger and more persistent than in the baseline (see Figure 6).[7]

To probe the underlying causes behind these consumption upticks, we explore the changes in employment patterns for the same sample of households during the same time period using the NSS employment-unemployment data rounds. We do *not* find any robust evidence that individuals from the treated households tend work more in the periods prior to elections. Furthermore, by exploring the pattern of employment pertaining to "Public Works" programmes (i.e., government-funded employment programmes), we find that there is no systematic effect on days worked on such programmes too.[8] So can any possible wage increases explain the uptick in the various consumption categories? To answer this, we explore the wage payments to these individuals and we find that the total wages paid is essentially *unchanged* in the period coinciding with higher consumption.[9] This finding confirms that these spikes in consumption simply cannot be explained by the employment patterns.

It is widely believed that in-kind transfers are also made to buy votes in India. These often take the form of clothes (especially, saris) and liquor bottles although other goods – like food-grains – may be provided too.[10] It is not clear to what extent such in-kind recipient households 'revise' their consumption estimates when answering the NSS surveys. If, for example, they actually spend less on clothes because they received it in kind then they might actually report a lower expenditure on clothes. Thus, the expenditure increments that we capture are but a lower bound of the actual increase in consumption.

The rest of the paper is organised as follows. Section 2 describes the data, the identification strategy and the baseline results. Section 3 explores the heterogeneity in results

[7] We are grateful to an anonymous referee for this suggestion.

[8] More details are available in the Appendix.

[9] The estimated coefficient is positive and statistically significant only in the case of the treated households surveyed 5 months before elections. But that effect is too modest to explain the MPCE jumps in the other months. More on this later.

[10] A recent article in *The Hindu* newspaper contains such an account. See link: https://www.thehindu.com/news/cities/puducherry/silk-saris-worth-8-lakh-seized-from-bjp-activists-house-in-puducherry/article34072817.ece. Another recent incident of liquor being distributed to mobilise supporters – reported in the *Indian Express* newspaper – can be found here: https://indianexpress.com/elections/elections-2019-in-a-first-in-andhra-and-telangana-liquor-bottles-carry-party-stickers-5668053/.

along different dimensions. Section 4 describes the robustness check as regards the issue of selection-into-treatment, and Section 5 discusses the analysis with the employment data rounds, and Section 6 concludes. The appendix contains additional tables and figures.

2 Empirical Analysis

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

2.1 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 date of election 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 also use data on the outcomes of the elections from the same source (i.e., the Election Commission of India).

We match these data on the dates 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 61st and the 68th, a total of seven rounds.[11] Additionally we use data from the corresponding Employment and Unemployment rounds of the NSS to match the number of days worked and the wages received in this period. Since elections happen in several waves or phases within a state in India, the same state may have multiple

[11]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 61st round.

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 monthly time bins, both for the *treated* and for the *control* groups. In this way, we can track the consumption of households in each time bin. Specifically, we use the annual time frame and hence have time dummies for whether the household was surveyed one month, two months,.., six months before/after the election date.

Effectively, our data represent a repeated cross-sectional design with different households in the pre-exposure and post-exposure samples, where exposure is in the sense of any putative vote-buying (in-kind or cash transfers) and is related to the election timing. 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.

2.2 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. The key idea is to check for differences in consumption across the two groups for different time bins before/after elections. Our estimation equation therefore is as follows:

$$y_{ist} = \beta_0 + \beta_1 Treated_{ist} + \beta_{\mathbf{2}} Month^m_{ist} + \beta_{\mathbf{3}} Treated_{ist} * Month^m_{ist} + \gamma \mathbf{X_{ist}} + \epsilon_{ist} \tag{1}$$

where $Treated_{ist}$ is a binary indicator that indicates if household i resided in state s and had elections in year t. $Month^m_{ist}$ is a dummy variable that takes the value 1 for household i from state s in year t if the household was interviewed in month m where $m \in \{-6, .., 6\}$ such that $m = -6$ indicates 6 months prior to election and $m = 6$ indicates 6 months after the election.

As multiple states go to elections each year, how we use observations in states without elections is important. Suppose a household was interviewed in April 2012 in a state without elections but in 2012 we had states with elections in March 2012 and June 2012. This is how we code the month variable for that household: the idea is that such a household would show up twice in our sample, once for each election state. So, in one observation the month variable would be 1 (April-March) and -2 (April-June) for the other.

The outcome variable y_{ist} is some measure of consumption for household i in state s surveyed in year t. 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 expenditures on health and education. Additionally, in the analysis with the employment data the outcome is some employment-related variable reported by the household. $\mathbf{X_{ist}}$ is set of control variables at the household level. We cluster the standard errors at the district level in all our models.[12]

The main parameters of interest are the coefficients on $Treated_{ist} * Month^{m}_{ist}$, i.e, $\beta_{\mathbf{3}}$. These capture the average increase in y_{ist} for households in election states relative to non-election ones that were surveyed $6 - m$ months before (for $m \in \{1, 6\}$) and $m - 6$ months after (for $m \in \{7, 12\}$) the elections when accounting for the control variables.

The key identification assumption for our estimation strategy is that the assignment to treatment is random. Therefore, the differences in the baseline characteristics across the treated and control groups should be negligible in a statistical sense.

2.2.1 Descriptive statistics and balance checks:

Table 1 contains some descriptive statistics for the key variables in our analysis. We present the details of the variables by treatment category. Here, the *treated* sample is all households

[12]The state assembly constituencies are smaller than the districts and they fit neatly inside the district boundaries. We have 445 such districts in our sample. An alternative would be to cluster by state, but this is problemmatic as the number of states in our sample is much smaller than 30.

with state elections surveyed upto 6 months ahead of election day. The *control* sample is all households surveyed during the same time period but from states without elections. In this sense, the samples are from the pre-treatment period. To be sure, any potential vote-buying actually takes place *before* the elections and hence the various expenditure related items for the *treated* sample may already exhibit a rise compared to the *control* ones. Indeed, that is what we find.

Given that our sample is based on observational data rather than a randomised control trial, there is a need to check for balancedness across the treated and control households for the relevant variables at the baseline/pre-treatment stage. The concern that the households surveyed close to elections (hence, the treated ones) may be selected is a valid one — hence, the need to conduct such checks. Recall that the set of treated households in the pre-treatment stage do *not* coincide with the set of households in the post-treatment stage owing to the sampling technique of the NSSO. Hence, such balance checks need to be interpreted keeping this issue in mind.

We report comparisons of the relevant variables at baseline for the different variables in Table 2.

As can be noted from Table 2, the households in the treated and control groups do look dissimilar on certain characteristics (religion, caste, size, etc.) at baseline. This however is accounted for in the regression analysis as we control for all such household characteristics. What is perhaps of concern is that the treated households seem to be spending more overall (MPCE) and also on fish/meats and health and education in comparison to the control ones. In terms of spending on pulses, the treated households exhibit lower spending at baseline. While this is not ideal, two things are worth noting. (1) The treated households in the "pre-treatment" stage are *not* the same as those households in the "post-treatment" stage owing to the sampling technique of the NSSO. (2) The consumption expenditure spending in the treated group may already reflect the cash transfers due to vote-buying.

	Mean	Standard Deviation	Observations
Control			
Household MPCE	1,233.537	1,361.056	146,764
Spending categories:			
Pulses	477.961	439.879	146,764
Fish+Meats	100.640	221.450	146,764
Intoxicants	23.015	194.956	146,764
Clothes	974.986	2,116.201	146,764
Health	261.725	4,358.292	146,764
Education	732.277	4,083.032	146,764
Household characteristics:			
Hindu	0.830	0.376	146,764
General caste	0.344	0.475	146,764
Rural	0.613	0.487	146,764
Household size	5.214	3.194	146,764
Homeowner (Y=1, N=0)	0.839	0.367	146,764
Treated			
Household MPCE	1,387.294	1,565.663	47,072
Spending categories:			
Pulses	427.844	420.838	47,072
Fish+Meats	104.171	202.740	47,072
Intoxicants	23.476	128.981	47,072
Clothes	984.658	2,107.805	47,072
Health	359.695	6,335.203	47,072
Education	860.490	4,675.781	47,072
Household characteristics:			
Hindu	0.814	0.389	47,072
General caste	0.366	0.482	47,072
Rural	0.594	0.491	47,072
Household size	5.045	3.223	47,072
Homeowner (Y=1, N=0)	0.821	0.383	47,072

Table 1: *Descriptive Statistics.*
The *Treated* sample is all households with state elections surveyed upto 6 months ahead of election day. The *Control* sample is all households surveyed during the same time period but from states without elections. In this sense, the samples are from the pre-treatment period. All expenditures are stated in INR.

	Control			Treated			Diff. (C-T)
	Mean	Std Dev.	Obs.	Mean	Std Dev.	Obs.	z-stat
Household MPCE	1,233.537	1,361.056	146,764	1,387.294	1,565.663	47,072	-20.5364***
Spending categories:							
Pulses	477.961	439.879	146,764	427.844	420.838	47,072	21.7343***
Fish+Meats	100.640	221.450	146,764	104.171	202.740	47,072	-3.0715***
Intoxicants	23.015	194.956	146,764	23.476	128.981	47,072	-0.4799
Clothes	974.986	2,116.201	146,764	984.658	2,107.805	47,072	-0.8637
Health	261.725	4,358.292	146,764	359.695	6,335.203	47,072	-3.7653***
Education	732.277	4,083.032	146,764	860.490	4,675.781	47,072	-5.716**
Household characteristics:							
Hindu	0.830	0.376	146,764	0.814	0.389	47,072	8.0818***
General caste	0.344	0.475	146,764	0.366	0.482	47,072	-8.6676***
Rural	0.613	0.487	146,764	0.594	0.491	47,072	7.4537***
Household size	5.214	3.194	146,764	5.045	3.223	47,072	9.9424***
Homeowner (Y=1, N=0)	0.839	0.367	146,764	0.821	0.383	47,072	9.2431***

Table 2: *Balance checks.*
We compare the treated households (i.e., households in states with elections) with the control ones (no election states). Here we report the balance checks for the relevant variables measures at baseline (pre-treatment), i.e., households surveyed up to 6 months prior to election days. All expenditures are stated in INR.

Nonetheless, we take this issue seriously and attempt to address this using propensity score matching. This is discussed in greater detail in Section 5 below.

2.3 Baseline Results

Table 3 contains our baseline results. In column 1, the dependent variable is the household's monthly per-capita expenditure. The observations in any given year span a period of 12 months — six months before and six months after the elections. The main coefficients of interest are those on *Treated*Month m* for $m \in \{-6, .., 6\}$. Note, $m = 0$ marks the election occurence. Note, we control for several household characteristics like religion (Hindu or not), caste group (general or not), rural/urban, size and homeownership status of the household

	[1] MPCE	[2] Pulses	[3] Fish+Meat	[4] Intoxicants	[5] Clothes	[6] Health	[7] Education
Treated	45.5	-73.7***	-32.6***	-3.6**	38.2	-4.1	81.0
	(29.9)	(15.9)	(6.7)	(1.7)	(84.1)	(58.5)	(141.1)
Treated* m-6	-35.7	88.2***	49.5***	1.7	-54.1	62.4	123.8
	(51.5)	(20.9)	(8.8)	(3.1)	(99.5)	(97.4)	(166.2)
Treated*m-5	34.1	23.1	32.9***	7.5**	39.2	-14.5	-23.5
	(52.9)	(19.5)	(6.7)	(3.8)	(88.3)	(91.1)	(168.9)
Treated*m-4	124.9***	39.2**	37.6***	7.8***	235.5***	222.4**	321.5**
	(42.7)	(16.3)	(6.1)	(2.6)	(69.7)	(87.7)	(157.9)
Treated*m-3	75.9**	17.5	28.1***	3.9*	79.7	139.3*	158.2
	(37.3)	(12.7)	(6.0)	(2.2)	(72.5)	(71.9)	(154.1)
Treated*m-2	31.1	-7.4	13.2**	-3.7	-85.8	-44.2	-185.2
	(35.0)	(14.2)	(6.4)	(4.3)	(86.7)	(84.7)	(164.5)
Treated*m-1	54.2	-11.5	12.5**	-1.3	40.5	243.3	-129.9
	(35.2)	(14.5)	(5.8)	(3.2)	(71.1)	(218.8)	(153.6)
Treated*m+1	5.8	10.9	9.3*	-3.7	-51.9	-63.1	-120.2
	(41.6)	(14.3)	(4.9)	(2.3)	(68.9)	(64.5)	(109.2)
Treated*m+2	17.7	4.5	9.2	-0.9	-39.1	79.4	-182.9
	(26.9)	(10.9)	(6.2)	(2.2)	(59.0)	(98.2)	(127.6)
Treated*m+3	35.4	0.4	-0.4	0.1	-18.5	-1.9	-146.9
	(28.1)	(11.3)	(5.4)	(2.5)	(63.2)	(82.9)	(139.3)
Treated*m+4	-28.3	3.7	-19.1***	-2.4	277.2***	-189.5**	-11.4
	(33.3)	(17.3)	(6.7)	(2.3)	(81.9)	(80.9)	(134.1)
Treated*m+5	-30.5	51.9***	16.4***	1.4	-11.5	46.8	-311.5**
	(40.4)	(18.5)	(6.3)	(2.6)	(96.2)	(134.8)	(152.1)
Treated*m+6	-111.7*	75.8***	17.7**	1.8	-67.5	17.6	-346.8*
	(63.4)	(18.3)	(7.4)	(3.1)	(107.8)	(119.9)	(199.9)
Constant term	1,578.9***	113.2***	116.6***	14.9***	157.1***	-178.0**	-38.3
	(41.1)	(9.2)	(6.4)	(1.9)	(58.7)	(71.8)	(70.8)
Month dummies (12)	Y	Y	Y	Y	Y	Y	Y
Household controls	Y	Y	Y	Y	Y	Y	Y
Observations	320,806	320,806	320,806	320,806	320,806	320,806	320,806
Adjusted R^2	0.108	0.215	0.039	0.001	0.139	0.003	0.036

Table 3: *Baseline regressions: MPCE and six categories.* The dependent variable in column 1 is the household's monthly per-capita expenditure. The other columns have different spending items as the dependent variables. Household controls, i.e., *Hindu, General Caste, Rural*, are household level dummy variables. Robust standard errors clustered by district in parentheses. *significant at 10% **significant at 5% ***significant at 1%

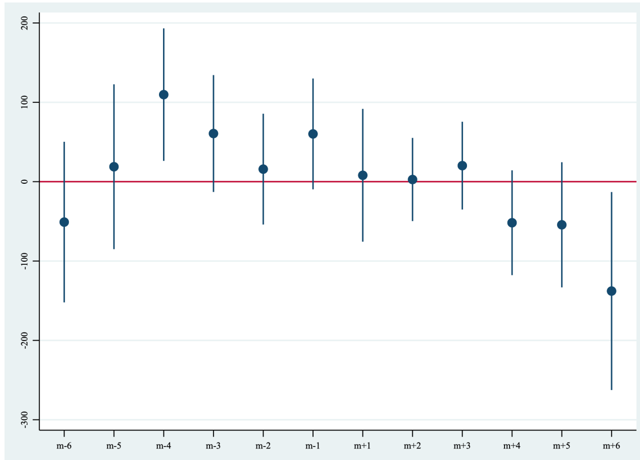


Figure 1: *Consumption shifts (absolute terms, units: INR) for MPCE:* The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month m* for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

(an indicator of household's assets) in our specification. All observations in states with elections outside of the $+/-6$ months window are dropped. The base "treated" category contains the 'residual' households in a state with elections within the actual 12-months range as we use 30 for the month size (hence, 360 days rather than the actual year-length).

We observe that the coefficients for *Treated*m-3* and *Treated*m- 4* are positive and statistically significant. This suggests that on average a household's consumption 3 and 4 months prior to elections registers an increase in states facing elections. Of particular interest is the finding that the coefficients on post-election interaction terms are not statistically significant and the coefficient even turns negative 4 months after the elections. The main coefficients of interest are presented in Figure1.

Next, we unpack the monthly per capita expenditure basket to see first if certain commodities that are highlighted as election-related gifts – in anecdotal accounts or otherwise – are indeed affected by the election cycle. We study six major categories of consumption derived from the NSS, which are typically a large and important share of the households monthly expenses. Moreover, some of these are commodities which have pinpointed by the literature as being used for voter coercion. They are: (i) pulses (various types of lentils.) (ii) fish, meat and other animal products, (iii) intoxicants (alcohol, narcotic drugs, etc.), (iv) clothes, (v) health-related expenditures and (vi) education-related expenditures. The grouping of items into a few large categories is important as NSSO reports spending by a household on a plethora of items and it is quite possible to find correlations between spending on a few of those by mere chance. Hence, we club similar items together to form sizeable spending categories to mitigate such concerns. We pay close attention to the items frequently mentioned in the anecdotes (like clothes, foodgrains, liquor etc.) among others.

Using the same identification strategy as for household monthly per-capita expenditure, we observe the effect of elections on expenditure on each of the six categories listed above — see columns 2–7 in Table 3.

Figures 11 – 16 in the Appendix contain visual depictions of the coefficients of interest, i.e., those on *Treated*Month m*. As can be seen for these six categories, the coefficients of interest are positive and statistically significant for some month/months prior to elections. The result is strongest for fish-cum-meats and intoxicants. Given the above findings, 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 most striking is the very fact that *any* kind of changes in consumption actually exist around the timing of elections. To be sure, this is not sufficient grounds to suspect foul play by political parties. But the very existence of these spikes and their patterns clearly need a thorough explanation.

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 after the conduct of elections. This is essentially what we find in most cases. Our results taken together suggest that the spikes in consumption are basically a pre-election phenomena – peaking at around 3 months before elections – which eventually disappear as we move along the months.

3 Heterogeneity analysis: Exploring mechanisms

Our objective of investigating the mechanisms behind vote-buying involves the consideration of the following:

(a) The effects may differ *ceteris paribus* by the economic status of households. In particular, poorer households may be more likely to receive these cash transfers leading to an uptick on basic consumption items by them after controlling for their political ideologies.

(b) Given that politically swing areas will receive more funds, the magnitude of the spike in consumption items should be greater in such places.

(c) Regions where vote-buying is believed to be more prevalent (based on anecdotal evidence) may exhibit a more pronounced reaction in terms of consumption changes.

We now turn to the examination of points (a) – (c).

3.1 Economic status

There are a number of reasons why vote buying might be most effective in conditions where voters are living in poverty. First, the marginal benefit of a small material transfer is larger for the poor than it is for the wealthy. That is, the types of goods that tend to be distributed during elections are simply more valuable to the poor, and so they may be more responsive to them. Second, the poor may be more risk averse (Stokes et al., 2013). Jensen and Justesen (2014) show that political parties' vote buying campaigns disproportionately target the poor. Canare et al (2018) also focus on the poor, as extensive anecdotal evidence in the Philippines suggests that this could be the group most prone to sell their votes, in large part because of their needs. This is supported by Schaffer (2005), who found evidence that vote buying is particularly effective among low-income voters. There are studies with similar results using data from Argentina (Brusco et al., 2004) and Turkey (Carkoglu and Aytac, 2015).

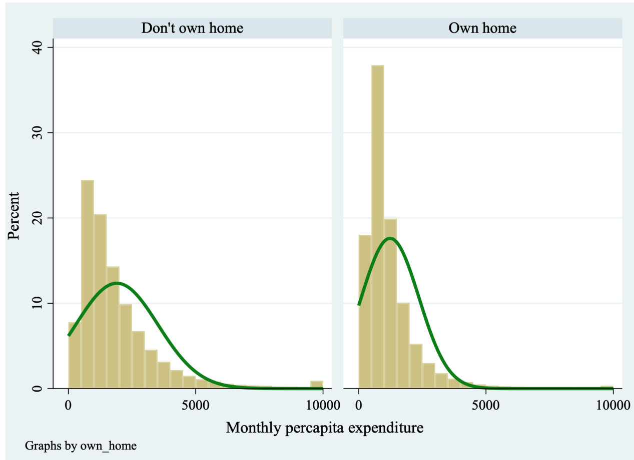


Figure 2: *Consumption shifts: Economic status – MPCE histogram:* The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month* m for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

We re-do our analysis for two distinct subsets of households: one is the set households that own their homes and the other group is the set of households that do not. We chose this particular measure of economic status, i.e., homeownership, owing to the data constraints and also because it is plausibly exogenous to any potential election-related transfers. We present a plot of the distribution of MPCE for home owners vs. non-home owners in Fgure 2. The figure supports our assumption of economic differences across the two broad groupings.

The key findings for the households who do not own homes is deipcted in Figure 3. Essentially, there appears to be no discernible effect on the treated households' consumption around the time of elections. This is in clear contrast to our baseline findings. In fact, apart from the expenditure on fish and meats (which looks similar to the baseline case) there seems to be no change at all in the expenditure categories for these households.

For the other set, i.e., households who own their homes, the effect is very similar to the baseline ones. If anything, the effects are more marked. Figure 4 validates this.

What lies beind this asymmetry? One potential explanation could be that the poorer households are politically more ideological (perhaps, more left-leaning) and thus less likely to

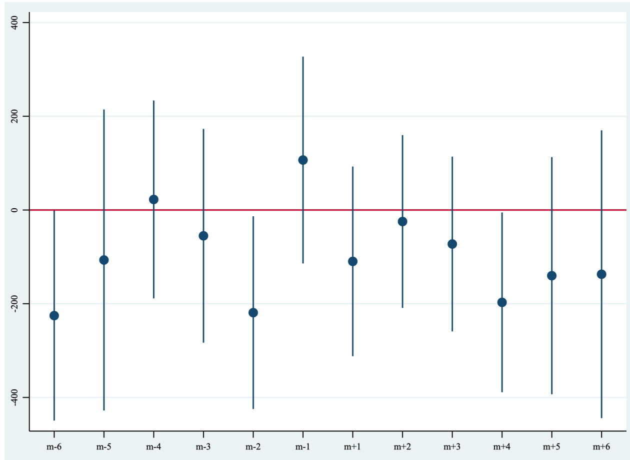


Figure 3: *Consumption shifts: Economic status (not homeowners) – MPCE:* The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month m* for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

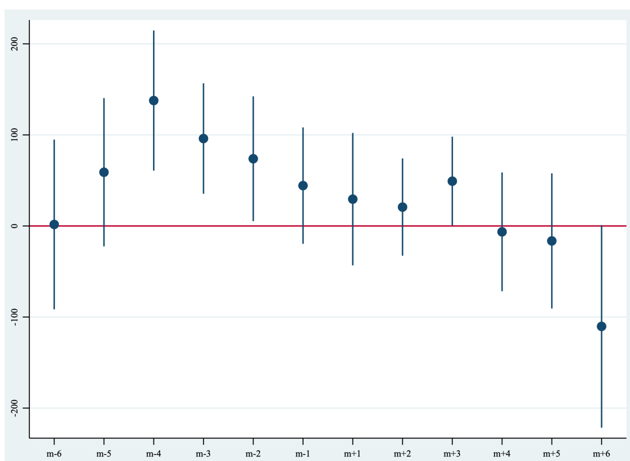


Figure 4: *Consumption shifts: Economic status (homeowners) – MPCE:* The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month m* for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

respond to cash transfers. Note, we are unable to account for the political ideology of the households in our analysis owing to the lack of granular data on voting patterns by income. Another possible reason could be that the poorer households are less likely to vote in the first place (this phenomenon has been observed in many countries) and hence they are ignored in this vote-buying exercise by all politicians. Once again, this is difficult to empirically verify in this context.

3.2 Politics

The 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) 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, we argue that swing districts might actually also witness higher levels of vote-buying.

We next 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.

Figure 5 presents the coefficients of interest when the outcome variable is the household's monthly per-capita expenditure. What is presented are the coefficients on the interaction of *Treated*Month m* with the 1-*Winning Margin* variable. The idea is that the coefficients on *Treated*Month m*1-Winning Margin* will indicate the effect higher political competition — as captured by a higher value of 1- *Winning Margin* — has on the treated household's consumption for month m. To interpret this coefficient, think of the additional transfer in

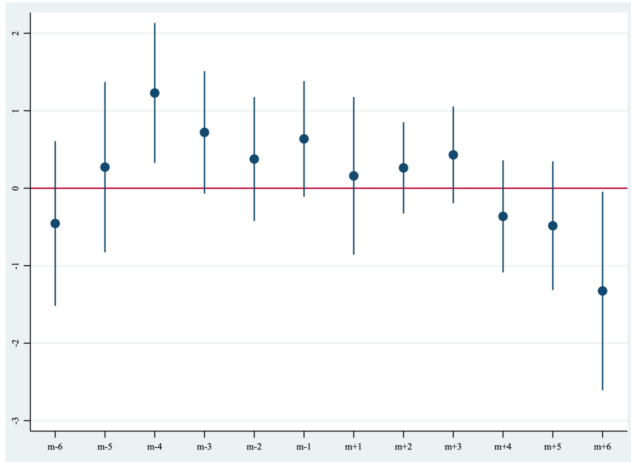


Figure 5: *Consumption shifts: Political competition – MPCE:* The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month m*1-Winning Margin* for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

a constituency where *(1-winning margin)* is 1% higher — i.e., political competition is 1% higher. The coefficient gives the average additional transfer that a household gets in the treated group over and above the transfer a household in a less competitive constituency would get.

From Figure 5 it appears that more resources are actually devoted to more competitive districts. There is a clear spike in some of the months preceding the elections.[13] The patterns are largely similar for the six categories of commodities (see Appendix). In sum, political competition tends to increase the magnitude of these consumption spikes.

3.3 Select states

While there is a paucity of hard evidence on the extent and patterns of vote-buying in India, there exists a large body of anecdotal evidence on the prevalence of this phenomenon.[14]

[13] We spilt the sample by above/below median levels of political competition. The effects are actually present for both samples, although the triple interaction regressions/plots make it clear that it is stronger for the more competitive elections.

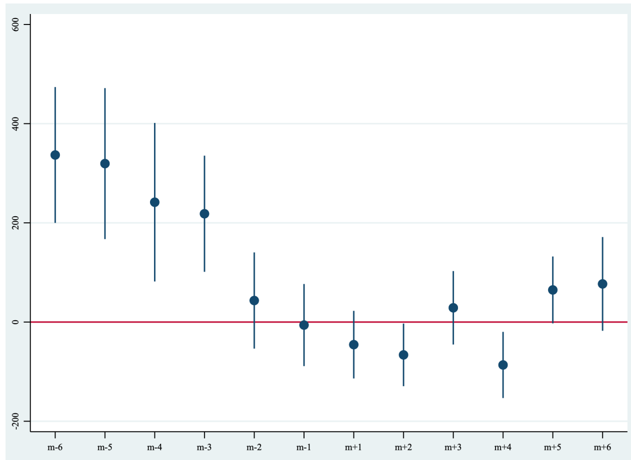
[14] See e.g., https://scroll.in/article/926120/whats-the-price-of-a-vote-in-india-a-new-report-comes-up-with-a-startling-number.

In particular, the states of Uttar Pradesh, Bihar and Tamil Nadu have historically been associated with such forms of corrupt practices.

We focus attention on just these three states each which are of more populous than the average Indian state and re-run our main analysis. The aim is to check whether the baseline patterns are present and specifically are more marked in this sample. The main results for MPCE and spending on the six categories are collected in Table 4.

As can be seen for MPCE in column 1 of this table, the coefficients on *Treated*Month m* for $m \in \{-6, .., -3\}$ is positive and statistically significant. This means that there is a rise in consumption expenditure till about 2 months to elections after which it essentially flattens out. The coefficients are also larger than in the sample *without* these states — this strongly suggests the possibility of higher vote-buying in the former sample. This is depicted clearly in the two panels in Figure 6.

Columns 2–7 in Table 4 show the effect on spending on the six categories. Here, the evidence is somewhat different from those in the baseline (Table 3). Specifically, for pulses (column 2) the effect is actually negative for months preceding the elections. For fish and meats (column 3), the effect is basically absent except for 4 months preceding the elections where it is actually negative. There is a similar pattern for intoxicants (column 4). The results for clothes (column 5) is somewhat similar to the baseline although there is a negative effect for a couple of months close to elections. The pattern for health expenditures (column 6) is essentially flat and for education expenditures (column 7) it is similar to the baseline. Taken together, this suggests that possibly other components of the household's consumption basket are responsible for the uptick in MPCE (as noted in column 1).



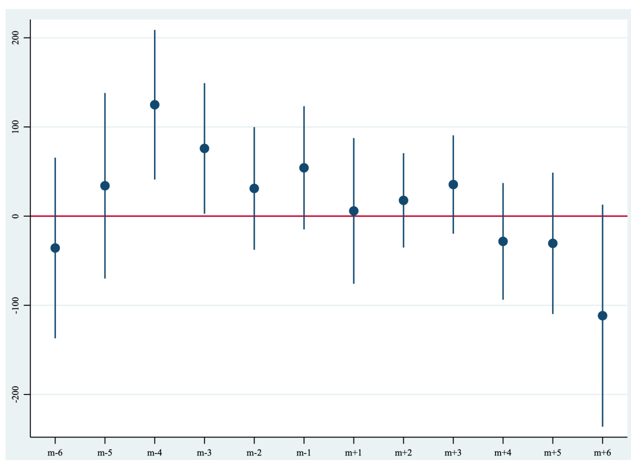


Figure 6: *Consumption shifts: Select states and all others– MPCE:* The top panel pertains to the states of Bihar, Tamil Nadu and Uttar Pradesh. The bottom panel comprises all others states in the overall sample. The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month* m for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

	[1] MPCE	[2] Pulses	[3] Fish+Meat	[4] Intoxicants	[5] Clothes	[6] Health	[7] Education
Treated	66.9**	-134.9***	-30.5***	-0.2	445.9***	218.3**	767.7***
	(31.6)	(13.1)	(3.9)	(1.0)	(67.7)	(103.5)	(214.5)
Treated*m-6	336.9***	-77.6**	-13.4	-2.5	5.1	50.5	345.2
	(69.2)	(34.5)	(11.8)	(4.7)	(159.3)	(400.2)	(330.6)
Treated*m-5	319.5***	-135.0***	-17.8**	-10.6***	154.2	11.9	32.6
	(76.9)	(30.2)	(8.1)	(2.9)	(124.6)	(193.4)	(349.9)
Treated*m-4	241.6***	11.3	14.4	-1.2	534.9***	181.3	767.4**
	(80.8)	(27.2)	(8.7)	(2.9)	(110.5)	(156.5)	(375.7)
Treated*m-3	218.3***	-76.8***	9.7	-3.5*	-268.5***	147.8	-393.3
	(59.2)	(15.3)	(6.5)	(1.9)	(101.8)	(157.9)	(267.0)
Treated*m-2	43.4	-69.0***	12.5*	-3.3	-363.3**	-276.8	-771.0***
	(49.0)	(24.3)	(6.9)	(2.6)	(141.9)	(183.2)	(286.9)
Treated*m-1	-6.1	-19.2	8.7	-4.1**	-97.4	662.9	-455.4*
	(41.9)	(25.7)	(6.7)	(1.9)	(105.7)	(541.1)	(267.4)
Treated*m+1	-45.6	10.1	2.4	-4.7**	-80.9	-261.3***	-250.4
	(34.5)	(24.8)	(5.2)	(2.2)	(104.4)	(94.1)	(183.3)
Treated*m+2	-66.2**	21.5	6.9	-6.2***	-193.3**	56.0	-540.7***
	(31.9)	(17.3)	(5.5)	(1.9)	(78.4)	(167.4)	(197.1)
Treated*m+3	28.7	26.7	9.9*	-3.008	-239.3**	-129.4	-420.5*
	(37.4)	(17.5)	(5.3)	(1.9)	(94.1)	(147.7)	(249.5)
Treated*m+4	-86.5**	117.2***	19.5***	-0.2	-132.6	-192.1*	-600.7***
	(33.7)	(17.8)	(4.7)	(1.9)	(85.5)	(111.1)	(207.2)
Treated*m+5	64.8*	112.3***	28.9***	-0.0	-496.2***	-170.1	-845.9***
	(34.1)	(23.1)	(5.8)	(2.1)	(93.3)	(231.3)	(196.9)
Treated*m+6	76.9	102.3***	22.2***	-1.5	-555.6***	-106.6	-1,108.6***
	(47.8)	(24.5)	(6.7)	(2.0)	(87.9)	(183.3)	(243.8)
Constant term	1,287.1***	140.1***	114.6***	2.7**	-122.5**	-386.2**	-298.4***
	(32.2)	(11.4)	(5.2)	(1.3)	(57.7)	(151.7)	(84.0)
Month dummies (12)	Y	Y	Y	Y	Y	Y	Y
Household controls	Y	Y	Y	Y	Y	Y	Y
Observations	100,960	100,960	100,960	100,960	100,960	100,960	100,960
Adjusted R^2	0.120	0.252	0.071	0.004	0.178	0.003	0.046

Table 4: *Select states (Bihar, Tamil Nadu and Uttar Pradesh): MPCE and six categories.* The dependent variable in column 1 is the household's monthly per-capita expenditure. The other columns have different spending items as the dependent variables. Household controls, i.e., *Hindu, General Caste, Rural*, are household level dummy variables. Robust standard errors clustered by district in parentheses. *significant at 10% **significant at 5% ***significant at 1%

4 Robustness: Propensity Score Matching

Here we deal with issues regarding selection into treatment. While there is little reason to believe that the NSS survey timings have any specific correlation with the election dates set by the State Election commissions, one should regardless try to ensure that the treated sample is not systematically different from the control group. The first step in this regard is to control for all household characteristics at baseline, which is something we do in all our regression models.

The concern for 'balance' between the treated and control groups is nonetheless a valid one and we therefore try to address it head on. We employ a propensity score matching (i.e., PSM) technique to make sure that the control group is the appropriate one for our treated sample. The propensity score match is based on observable household characteristics. We match on household characteristics, such as religion, caste, household size and whether the household is a rural household or not. Weights are assigned to the closest 5 matches according to the closeness between the treated and the control observations.[15] These weights are subsequently used as the weights in the regression analysis.

The main results using PSM are collected in Table 5. This is a direct analogue of our baseline table (i.e.. Table 3). The results are largely similar when comparing column by column of these two tables. A graphical depiction of the coefficients of interest for MPCE is presented in Figure 7. This is the exact counterpart of Figure 1. The similarity in the two suggests that selection into treatment is probably not an actual concern in our analysis.

We also conduct our analysis using PSM as regards the states of Uttar Pradesh, Bihar and Tamil Nadu as discussed in Section 3.3. The main results are collected in Table 8 in the Appendix. The findings are essentially the same as in Table 4.

[15] This is implemented using the code *psmatch2* in STATA.

	[1] MPCE	[2] Pulses	[3] Fish+Meat	[4] Intoxicants	[5] Clothes	[6] Health	[7] Education
Treated	60.7**	-60.9***	-30.6***	-3.1*	-7.4	5.1	110.5
	(29.7)	(16.4)	(6.9)	(1.8)	(86.6)	(60.1)	(144.2)
Treated*m-6	-50.9	75.5***	47.6***	1.2	-8.5	53.3	94.4
	(51.5)	(21.6)	(8.9)	(3.1)	(101.6)	(98.3)	(168.9)
Treated*m-5	18.8	10.3	31.0***	6.9*	84.8	-23.6	-53.0
	(52.9)	(20.2)	(6.8)	(3.8)	(89.8)	(92.3)	(172.3)
Treated*m-4	109.7**	26.5	35.6***	7.2***	281.1***	213.2**	292.1*
	(42.5)	(16.8)	(6.1)	(2.6)	(71.4)	(88.9)	(160.7)
Treated*m-3	60.7	4.8	26.1***	3.3	125.2*	130.2*	128.7
	(37.5)	(13.0)	(6.1)	(2.2)	(75.1)	(73.3)	(157.1)
Treated*m-2	15.8	-20.1	11.3*	-4.3	-40.3	-53.3	-214.6
	(35.5)	(14.5)	(6.4)	(4.3)	(90.4)	(86.0)	(166.5)
Treated*m-1	60.2*	-11.1	12.7**	-1.2	55.3	252.4	-128.8
	(35.5)	(14.5)	(5.9)	(3.2)	(72.5)	(225.4)	(157.3)
Treated*m+1	8.1	13.1	9.3*	-3.5	-64.2	-63.5	-129.6
	(42.6)	(14.2)	(5.0)	(2.3)	(69.2)	(65.8)	(111.7)
Treated*m+2	2.7	-7.9	7.2	-1.5	5.9	70.5	-212.1*
	(26.7)	(11.6)	(6.2)	(2.2)	(61.1)	(99.1)	(128.7)
Treated*m+3	20.2	-12.3	-2.4	-0.4	27.0	-11.0	-176.5
	(28.2)	(11.7)	(5.4)	(2.5)	(65.1)	(83.9)	(141.8)
Treated*m+4	-51.7	-15.1	-22.4***	-3.3	341.9***	-204.5**	-55.4
	(33.6)	(16.3)	(6.7)	(2.3)	(78.7)	(82.3)	(137.6)
Treated*m+5	-54.3	33.3*	13.5**	0.6	51.7	30.9	-359.8**
	(40.1)	(17.2)	(6.3)	(2.6)	(93.2)	(136.3)	(155.1)
Treated*m+6	-137.8**	57.8***	14.8**	0.9	-3.2	2.6	-402.0**
	(63.5)	(17.4)	(7.4)	(3.2)	(107.6)	(121.2)	(202.3)
Constant term	1,579.6***	112.2***	116.8***	14.9***	159.3***	-178.5**	-40.5
	(41.1)	(9.3)	(6.4)	(1.9)	(58.8)	(72.0)	(70.9)
Month dummies (12)	Y	Y	Y	Y	Y	Y	Y
Household controls	Y	Y	Y	Y	Y	Y	Y
Observations	319,612	319,612	319,612	319,612	319,612	319,612	319,612
Adjusted R^2	0.108	0.216	0.038	0.001	0.139	0.003	0.036

Table 5: *Propensity Score matching: MPCE and six categories.* The dependent variable in column 1 is the household's monthly per-capita expenditure. The other columns have different spending items as the dependent variables. Household controls, i.e., *Hindu, General Caste, Rural*, are household level dummy variables. Robust standard errors clustered by district in parentheses. *significant at 10% **significant at 5% ***significant at 1%

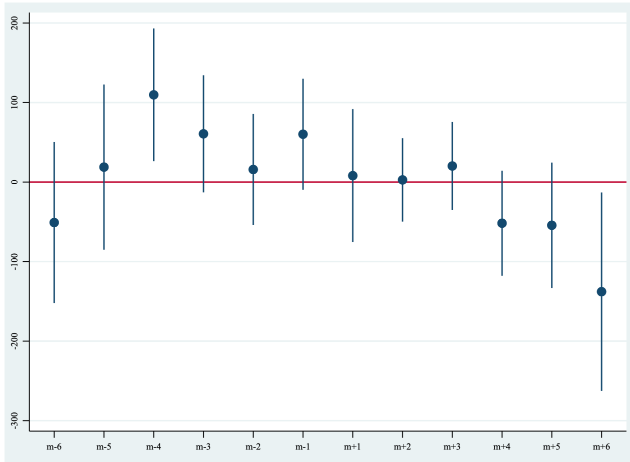


Figure 7: *Propensity Score Matching – MPCE:* The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month m* for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

5 Economic activity due to elections

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 expenditure. Some of the crucial questions which arise here are the following: *what is the reason behind these spikes? What drives them?* Our preferred explanation is the following: 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 in kind; so – in effect – some form of "vote-buying".

One alternative and admittedly more benign explanation for the spikes in consumption could be economic activity around elections. Elections are times of heightened economic activity with the political parties spending a lot of resources on campaigning and mobilisation of supporters. A natural expectation would be that states going for election should register increased days of work and relatedly greater wage bills paid out. We investigate this question carefully using the data from the NSS employment rounds corresponding to the *exact* consumption expenditure rounds used — i.e., the NSS rounds 61st through 68th.

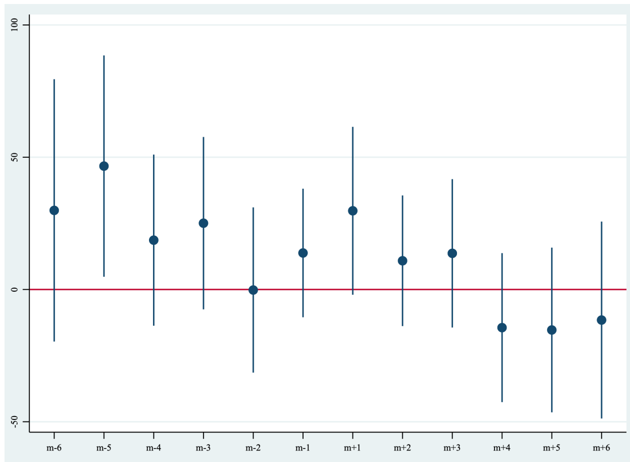


Figure 8: *Employment – Total wages paid:*The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month* m for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

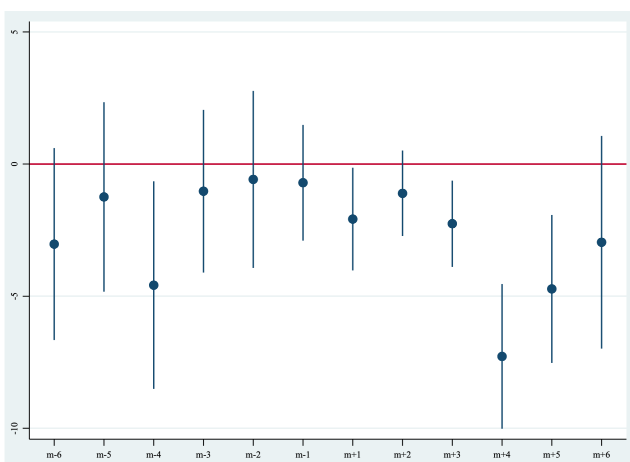


Figure 9: *Employment – Days worked:* The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month* m for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

The nature of the analysis is analogous to our main exercise. We reproduce the *treated* and *control* groups exactly as in our analysis so far, and follow them over the 12 months (6 months before and 6 months after the elections). What is different here is the outcome variables which are now constructed from the NSSO Employment rounds. We first look at the total wage payments to households around the time of elections for the same months. As can be seen from Figure 8, the coefficient on *Treated*Month m* is statistically not signficant at the 5% level for all the months except 5 months prior to elections. Even here, the rise is modest (less than 50 INR) which is well below the average rise in MPCE happening in 3-4 months before elections. Thus, the total wages received by individuals cannot finance the consumption spikes documented earlier.

We look at other measures of employment too. Figure 9 reports some results where the dependent variable is the total number of days worked in a month. There is actually a drop in the days worked 3 months prior to the elections, exactly where there is a jump in MPCE.

The logic of clientelistic politics — by which the incumbent increases the spending on various employment-related public programmes (titled "Public Works" in India) when elections approach, thus utilising government resources to provide immediate and tangible benefits to the constituents — also suggests an uptick in employment. We explore this channel too. Figure 17 in the appendix highlights the coefficients of interest where the dependent variable is the number of days worked on the public programmes ("Public Works") in a month. Here too the effect is hardly distinguishable from nought in the months prior to elections.

The above should not be viewed as a contradiction to the idea that the local economy may be stimulated by political activities circa elections. As an illustration, we include an excerpt from the expenses list for election-related use of the helipad at Balaghat in Madhya Pradesh. See Figure 10. This is an official document in the public domain accessed from the Government of Madhya Pradesh website.[16] The reason why such expenditures may not end

[16]The link is https://balaghat.nic.in/en/document/lok-sabha-election-2024-election-expenditure-rate-list/.

लोकसभा आम निर्वाचन 2024

RATE LIST

(HELIPAD AND AIRSTRIP BALAGHAT RELATED CHARGES)

हवाई पटटी बिरवा का विमान/ हेलीकॉप्टर का प्रतिदिन किराया निर्धारण			
S.NO.	**Description**	**Unit**	**Rate**
I	Airstrip halting Charges		
1	हेलीकोफ्टर/प्लेन लैडिंग शुल्क म.प्र. शासन विमानन विभाग मंत्रालय का पत्र क्र. एफ 9-3/2009 पैंतालीस भोपाल, दिनांक 05 मई 2019	Per landing	5000
2	हेलीकोफ्टर/प्लेन हालटिंग शुल्क	12 Hour	2000
		24 Hour	5000
II	In Case of levelled & Compacted Ground		
1	Cleaning of Helipad and "H" marking for temporary Helipad	Each	1297
2.	Cleaning of Helipad and application of 2-3 coats of cow dung mix with water and "H" Marking with Lime	Each	3650
III	In Case of rough & unfinished ground		
	Higher charges of machineries i/c fuel 7 driver on hourly basis as per Collectorate /Nagar Palika rate.	As per Nagar Palika rate	
IV	Soft Barricades for temporary Helipad		
1	Balli-Bamboo Barricade	Per Helipad	2287
2	Balli-Rassi Barricade	Per Helipad	1345.20
V	Barricading		
1	Steel	Per Meter	22
2	Welded wire mesh	Per Meter	18
3	Bamboo/Pipe	Per Meter	8
4	Vertical post of hollow pipe/Ballies	Each	12
VI	Drop Gate	Each	308
VII	FIRE BRIGADE VEHICLE CHARGE PER EVENT	2000	
VIII	AMBULANCE CHARGE	1000	

नोडल अधिकारी व्यय लेखा अधिकारी	**उप जिला निर्वाचन अधिकारी जिला-बालाघाट**	**कलेक्टर एवं जिला निर्वाचन अधिकारी जिला-बालाघाट**

Figure 10: *Example of political activity around elections.*

up boosting the reported consumption of households possibly rests on the idea of Kapur and Vaishnav (2013). They show that resources (in the construction sector and possibly others) are re-purposed for election spending and not used to support economic activity. Therefore, it is feasible that election periods offer greater work opportunities but lower wage payments in the aggregate.

Our findings here basically negate the hypothesis that standard economic and business-related activities surrounding elections can fully explain the consistent increase in consumption expenditures.

6 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 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 there *is* a clear spike in the consumption expenditure for a vast majority of households. Our findings are strongly consistent the following thesis: funds are transferred across constituencies during the time of elections. This movement of resources follows a pattern of vote-maximisation in the spirit of the classic probabilistic-voting models. Our exercise allows us to engage with the issue of the price of a vote.[17] Anecdotal evidence in the media suggests that it was about 700 INR in the 2024 general election in India.[18] However,

[17] We are thankful to an anonymous referee for highlighting this.

[18] See https://www.newindianexpress.com/web-only/2024/Apr/18/more-than-rs-700-per-vote-inside-indias-record-breaking-election.

it has risen over the decades and our data ends in 2012. The estimate based on our baseline regressions for MPCE is between 75 and 125 INR when looking at the statistically significant coefficients (Table 3, column 1).

Somewhat suprisingly, we find that periods of increased consumption actually correspond to largely constant wage payments. Thus, the employment patterns cannot explain the increased spending by the households prior to elections. Our results certainly point towards the use of cash to buy votes and not merely increase economic activity due to the business of elections. This bears implications for election financing in general and highlights the importance of the question of whether public funding of elections is a necessity in developing countries.

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Appendix

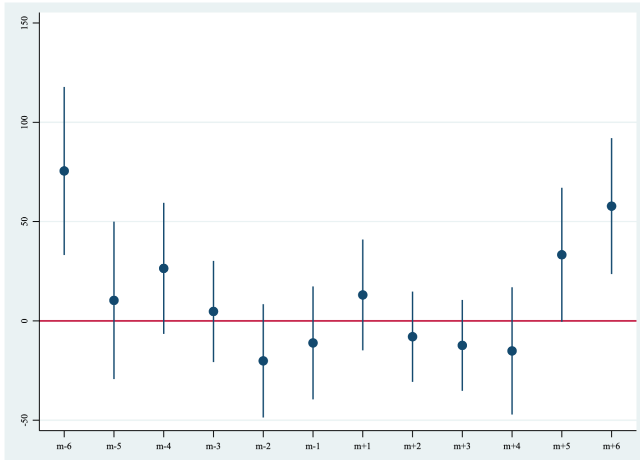

Figure 11: *Consumption shifts (absolute terms, units: INR) for Pulses:* The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month m* for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

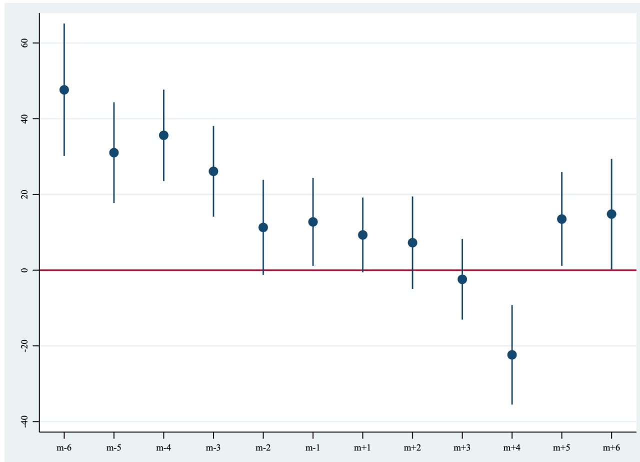

Figure 12: *Consumption shifts (absolute terms, units: INR) for Fish/Meat:* The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month m* for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

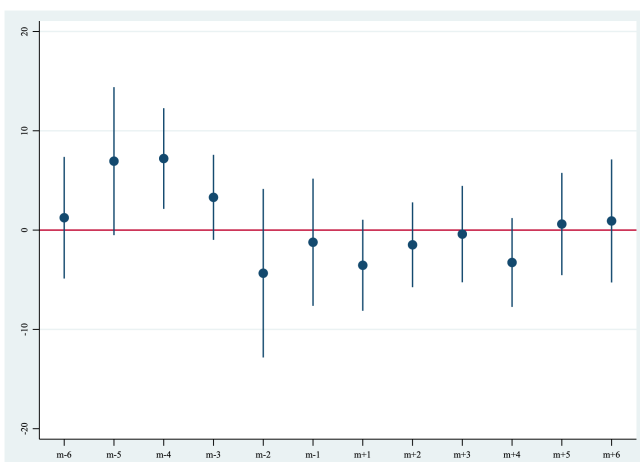

Figure 13: *Consumption shifts (absolute terms, units: INR) for Intoxicants:* The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month m* for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

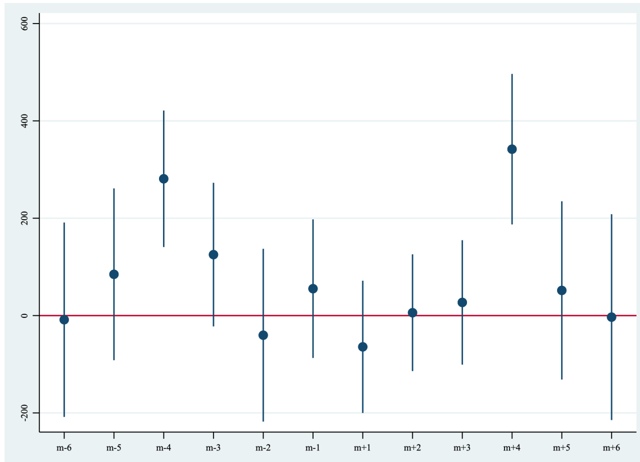

Figure 14: *Consumption shifts (absolute terms, units: INR) for Clothes:* The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month m* for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

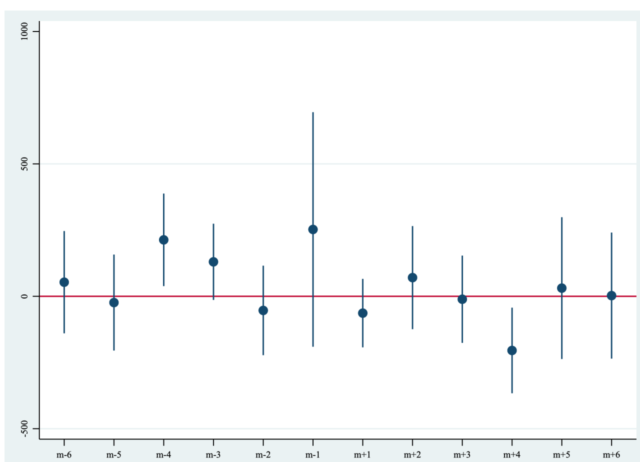

Figure 15: *Consumption shifts (absolute terms, units: INR) for Health:* The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month m* for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

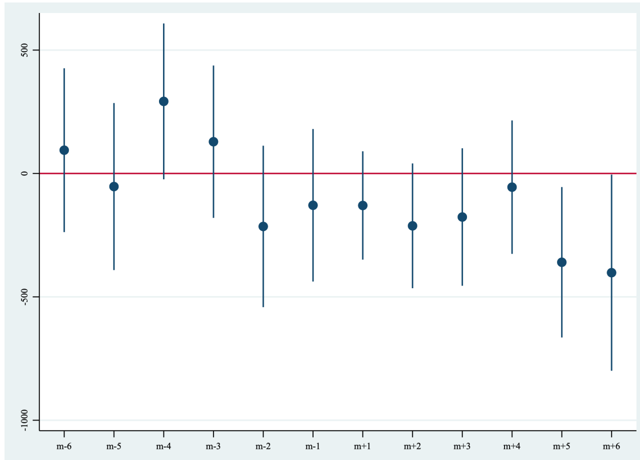

Figure 16: *Consumption shifts (absolute terms, units: INR) for Education:* The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month m* for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

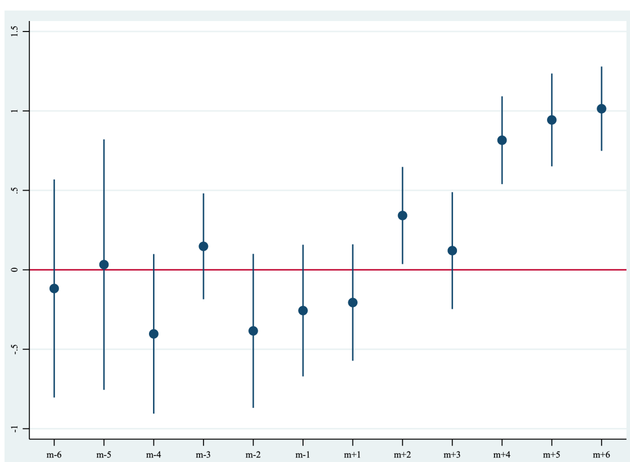

Figure 17: *Employment – Total days in Public Works:* The dots denote the coefficients of interest which captures the effect of elections, namely, *Treated*Month m* for $m \in \{-6, .., +6\}$, where 0 coincides with elections. The 95% confidence interval is also shown.

	[1] MPCE	[2] Pulses	[3] Fish+Meat	[4] Intoxicants	[5] Clothes	[6] Health	[7] Education
Treated	89.5	-53.3**	-24.1**	-3.2	19.9	12.4	67.2
	(75.4)	(23.3)	(9.9)	(2.9)	(140.4)	(92.1)	(269.1)
Treated*m-6	-225.4**	50.5*	23.8*	-1.2	-32.9	105.9	74.9
	(114.0)	(27.9)	(12.3)	(6.5)	(194.6)	(116.7)	(339.9)
Treated*m-5	-106.7	43.6	38.9***	14.5*	-51.9	-35.3	-221.2
	(163.5)	(29.4)	(10.1)	(7.8)	(156.7)	(128.9)	(338.0)
Treated*m-4	22.5	30.8	22.6**	2.9	101.2	81.4	436.0
	(107.5)	(24.3)	(10.2)	(4.9)	(131.5)	(112.7)	(384.7)
Treated*m-3	-55.1	-5.0	10.8	-6.5*	-33.0	91.1	-193.1
	(116.1)	(24.0)	(9.9)	(3.9)	(124.5)	(99.1)	(315.3)
Treated*m-2	-219.0**	-5.6	1.9	2.2	-239.3*	-121.4	-229.3
	(104.6)	(28.1)	(10.0)	(5.0)	(144.1)	(118.9)	(352.3)
Treated*m-1	106.6	-5.6	17.5	-5.1	-136.6	-82.1	-531.8**
	(112.3)	(27.4)	(11.5)	(6.8)	(120.7)	(116.9)	(241.7)
Treated*m+1	-109.6	13.8	5.6	-4.7	-59.6	-149.6	-352.9
	(103.0)	(22.5)	(9.2)	(4.7)	(119.3)	(118.3)	(247.3)
Treated*m+2	-24.6	3.7	2.8	-0.4	-43.2	104.2	-396.5
	(93.8)	(20.4)	(7.4)	(4.5)	(113.5)	(198.7)	(349.7)
Treated*m+3	-72.6	9.3	6.4	0.6	106.3	-15.6	-41.1
	(94.9)	(21.4)	(9.5)	(5.4)	(139.3)	(140.9)	(333.1)
Treated*m+4	-197.1**	-20.0	-17.3*	0.7	156.5	-85.4	-303.9
	(97.6)	(24.2)	(9.3)	(4.8)	(146.8)	(142.4)	(372.8)
Treated*m+5	-140.1	1.7	0.2	-2.3	134.2	118.5	-617.0
	(128.8)	(32.7)	(13.2)	(5.1)	(171.5)	(168.0)	(410.260)
Treated*m+6	-137.1	56.0**	11.7	5.0	34.2	115.7	-269.1
	(156.3)	(25.6)	(13.9)	(5.0)	(197.5)	(168.3)	(416.8)
Constant term	1,563.1***	185.9***	98.0***	13.1***	479.2***	-13.9	-379.4***
	(64.3)	(14.5)	(5.4)	(2.9)	(76.9)	(39.4)	(139.5)
Month dummies (12)	Y	Y	Y	Y	Y	Y	Y
Household controls	Y	Y	Y	Y	Y	Y	Y
Observations	51,369	51,369	51,369	51,369	51,369	51,369	51,369
Adjusted R^2	0.061	0.119	0.046	0.004	0.079	0.003	0.030

Table 6: *Economic status – Non-homeowners: MPCE and six categories.* The dependent variable in column 1 is the household's monthly per-capita expenditure. The other columns have different spending items as the dependent variables. Household controls, i.e., *Hindu, General Caste, Rural*, are household level dummy variables. Robust standard errors clustered by district in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1] MPCE	[2] Pulses	[3] Fish+Meat	[4] Intoxicants	[5] Clothes	[6] Health	[7] Education
Treated	40.9	-78.5***	-34.3***	-3.7*	38.8	-8.8	83.8
	(26.7)	(16.1)	(6.8)	(1.9)	(81.4)	(66.3)	(137.1)
Treated*m-6	1.6	98.7***	55.7***	2.4	-48.8	56.1	135.9
	(47.4)	(22.6)	(9.3)	(3.4)	(98.2)	(115.2)	(162.3)
Treated*m-5	59.0	19.7	31.6***	5.7	68.9	-7.6	26.3
	(41.4)	(19.9)	(7.0)	(4.0)	(85.3)	(105.6)	(164.0)
Treated*m-4	137.7***	41.3**	40.8***	8.7***	267.1***	254.9**	298.4**
	(39.2)	(17.2)	(6.4)	(2.6)	(68.8)	(101.8)	(150.0)
Treated*m-3	96.0***	21.9*	31.3***	5.8**	101.3	148.4*	229.5
	(30.9)	(12.7)	(6.1)	(2.3)	(72.0)	(83.1)	(147.3)
Treated*m-2	73.8**	-6.4	15.3**	-4.9	-53.4	-27.8	-177.1
	(34.9)	(15.5)	(6.8)	(4.9)	(90.1)	(97.2)	(163.9)
Treated*m-1	44.3	-12.6	11.7**	-0.7	67.6	296.3	-61.7
	(32.5)	(15.3)	(5.6)	(3.5)	(75.9)	(253.5)	(171.2)
Treated*m+1	29.4	9.7	9.9*	-3.5	-52.6	-48.4	-75.9
	(37.0)	(15.4)	(5.6)	(2.5)	(72.6)	(73.8)	(119.1)
Treated*m+2	20.7	5.9	10.5	-1.1	-33.7	76.9	-144.2
	(27.2)	(11.8)	(6.7)	(2.3)	(61.5)	(108.5)	(140.2)
Treated*m+3	49.2**	-0.3	-1.7	-0.1	-40.3	2.5	-166.4
	(24.9)	(12.6)	(5.6)	(2.7)	(64.2)	(93.5)	(142.4)
Treated*m+4	-6.5	7.8	-19.1***	-3.0	293.4***	-209.0**	39.3
	(33.2)	(18.4)	(7.1)	(2.4)	(83.7)	(93.5)	(147.4)
Treated*m+5	-16.4	62.4***	19.4***	1.9	-31.6	40.3	-267.3*
	(37.8)	(18.9)	(6.2)	(2.9)	(99.7)	(153.0)	(157.6)
Treated*m+6	-110.3*	81.3***	18.9**	1.0	-80.7	1.5	-362.2*
	(56.6)	(20.1)	(7.5)	(3.6)	(110.3)	(139.7)	(204.1)
Constant term	1,506.0***	110.9***	122.5***	14.3***	119.3*	-189.9**	43.2
	(44.2)	(9.6)	(7.7)	(2.1)	(62.0)	(86.2)	(76.9)
Month dummies (12)	Y	Y	Y	Y	Y	Y	Y
Household controls	Y	Y	Y	Y	Y	Y	Y
Observations	269,437	269,437	269,437	269,437	269,437	269,437	269,437
Adjusted R^2	0.095	0.223	0.038	0.001	0.152	0.003	0.038

Table 7: *Economic status – Homeowners: MPCE and six categories.* The dependent variable in column 1 is the household's monthly per-capita expenditure. The other columns have different spending items as the dependent variables. Household controls, i.e., *Hindu, General Caste, Rural*, are household level dummy variables. Robust standard errors clustered by district in parentheses. *significant at 10% **significant at 5% ***significant at 1%

	[1] MPCE	[2] Pulses	[3] Fish+Meat	[4] Intoxicants	[5] Clothes	[6] Health	[7] Education
Treated	66.9**	-134.9***	-30.5***	-0.2	445.9***	218.3**	767.7***
	(31.6)	(13.1)	(4.0)	(1.0)	(67.7)	(103.5)	(214.5)
Treated*m-6	336.9***	-77.6**	-13.4	-2.5	5.1	50.5	345.2
	(69.2)	(34.5)	(11.8)	(4.7)	(159.3)	(400.2)	(330.6)
Treated*m-5	319.5***	-135.0***	-17.8**	-10.6***	154.2	12.0	32.6
	(77.0)	(30.2)	(8.1)	(2.9)	(124.6)	(193.4)	(350.0)
Treated*m-4	241.6***	11.3	14.4	-1.2	534.9***	181.3	767.4**
	(80.8)	(27.2)	(8.7)	(3.0)	(110.5)	(156.5)	(375.7)
Treated*m-3	218.3***	-76.8***	9.7	-3.5*	-268.5***	147.8	-393.3
	(59.2)	(15.3)	(6.5)	(1.9)	(101.8)	(158.0)	(267.0)
Treated*m-2	43.4	-69.0***	12.5*	-3.4	-363.3**	-276.8	-771.0***
	(49.0)	(24.3)	(6.9)	(2.6)	(142.0)	(183.2)	(286.9)
Treated*m-1	-6.1	-19.2	8.7	-4.1**	-97.4	663.0	-455.4*
	(41.9)	(25.7)	(6.7)	(1.9)	(105.7)	(541.1)	(267.4)
Treated*m+1	-45.6	10.1	2.4	-4.7**	-80.9	-261.3***	-250.4
	(34.5)	(24.8)	(5.2)	(2.2)	(104.4)	(94.1)	(183.3)
Treated*m+2	-66.2**	21.5	6.9	-6.2***	-193.3**	56.0	-540.7***
	(31.9)	(17.3)	(5.5)	(1.9)	(78.4)	(167.4)	(197.1)
Treated*m+3	28.8	26.7	10.0*	-3.0	-239.3**	-129.4	-420.5*
	(37.4)	(17.5)	(5.3)	(1.9)	(94.1)	(147.7)	(249.5)
Treated*m+4	-86.5**	117.2***	19.5***	-0.2	-132.6	-192.0*	-600.7***
	(33.7)	(17.8)	(4.7)	(2.0)	(85.5)	(111.1)	(207.2)
Treated*m+5	64.8*	112.3***	29.0***	-0.0	-496.2**	-170.1	-846.0***
	(34.1)	(23.1)	(5.8)	(2.1)	(93.3)	(231.3)	(196.9)
Treated*m+6	76.9	102.3***	22.2***	-1.5	-555.6***	-106.6	-1,108.6***
	(47.8)	(24.5)	(6.7)	(2.0)	(87.9)	(183.3)	(243.8)
Constant term	1,287.1***	140.1***	114.6***	2.7**	-122.5**	-386.2**	-298.4***
	(32.2)	(11.4)	(5.2)	(1.3)	(57.8)	(151.7)	(84.0)
Month dummies (12)	Y	Y	Y	Y	Y	Y	Y
Household controls	Y	Y	Y	Y	Y	Y	Y
Observations	100,960	100,960	100,960	100,960	100,960	100,960	100,960
Adjusted R^2	0.120	0.252	0.071	0.004	0.178	0.003	0.046

Table 8: *Select states (Bihar, Tamil Nadu, Uttar Pradesh) – PSM: MPCE and six categories.* The dependent variable in column 1 is the household's monthly per-capita expenditure. The other columns have different spending items as the dependent variables. Household controls, i.e., *Hindu, General Caste, Rural*, are household level dummy variables. Robust standard errors clustered by district in parentheses. *significant at 10% **significant at 5% ***significant at 1%