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ARTICLE

The Machine Works: Why Turnout Buying is More Effective Than it Appears

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Abstract

Turnout buying is a mainstay of machine politics. Despite strong theory that selective incentives should spur turnout, meta-analyses of empirical studies show no effect, thus making machine politics seem irrational and unsustainable. I argue that the apparent failure of turnout buying is an artefact of common measurement decisions in experimental and observational research that lump together turnout buying, abstention buying, and vote-choice buying. Data generated using these compound measures include countervailing and null effects that drive estimates of the effects of each strategy toward zero. I show that machines have incentives to diversify their strategies enough to make compound measures substantially underestimate the impact of turnout buying. I propose simple alternative measurement approaches and show how they perform using new survey data and a constituency-level analysis of machine strategy in Mexico. Findings close the gap between theory and facts and reaffirm the rationality of machine politics.

Keywords: Elections; voting behavior; clientelism; vote buying

Introduction

Four weeks before the hard-fought 2017 gubernatorial elections in the State of Mexico, a strategy document for the incumbent Institutional Revolutionary Party (PRI) was leaked. The party was in danger of losing in Mexico's most populous state, long considered a bellwether for the presidential elections the following year. The consultancy that authored the document recommended diverse strategies, including mobilizing turnout in PRI strongholds, generating abstention in areas dominated by the opposition, and persuading wavering voters to choose the PRI's candidate.[1] To accomplish these goals, the PRI engaged in a massive vote-buying scheme that offered voters cash, food, household items, medical assistance, legal services, and other benefits (Beltrán and Castro Cornejo 2017; Ureste and Roldán 2022).

The party did precisely what theorists of electoral clientelism would prescribe. To maximize their vote share, political machines should buy turnout among their supporters, abstention among their strong opponents, and vote choices among their weak opponents (Díaz-Cayeros, Estévez, and Magaloni 2016; Dunning and Stokes 2008; Gans-Morse, Mazzuca, and Nichter 2014). Yet, recognition of this diversity is absent in empirical studies that use multi-barreled survey questions or compound experimental treatments. I refer to these together as 'compound measures' because

[1]https://es.slideshare.net/AMLO-MORENA/elecciones-en-edomex-2017.

they both lump together disimilar concepts.[2] Compound measures push causal estimates of the effect of any one form of vote buying toward zero. When examining the influence of turnout buying on turnout, including vote-choice buying adds null effects and including abstention buying adds countervailing effects to the data. The resulting gap between theory and findings is so stark that machine politics appears irrational and unsustainable.

Turnout buying is a mainstay of machine politics and the theory supporting its effectiveness is particularly clear.[3] Whether voting is rational (Riker and Ordeshook 1968) or expressive (Schuessler 2000), selective incentives can help overcome the time-costs of voting. Machines frequently direct electoral gifts to their own supporters (Aspinall et al. 2022; Cruz 2019; Stokes et al. 2013), plausibly because buying turnout is cheaper than buying abstention or votechoices among opponents (Zarazaga 2016). Targeting supporters is also less likely to spur legal sanctions and machines can easily monitor compliance at the polls (Cox and Kousser 1981, 656; Gans-Morse, Mazzuca, and Nichter 2014). Turnout buying is thought to endure over time because it is incentive compatible for the machine and its supporters (Nichter 2008).

It is thus surprising that existing studies find no impact of vote-buying attempts on turnout, on average. As I demonstrate below, meta-analyses of forty-one estimates from observational studies and eight estimates from experimental studies show no statistically significant effects. By contrast, door-to-door canvassing alone raises turnout by over 6.3 percentage points, on average, across multiple studies (Green and Gerber 2019, 182). Considering that buying turnout requires similar face-to-face contact *and* material payments, turnout buying seems like a bad bet compared to non-clientelist strategies.

I argue that current null findings are an artefact of measurement. If political machines diversify their strategies, as the PRI did in 2017, or competing machines target the same voters with different strategies, then compound measures underestimate the impact of turnout buying. Simple approaches that measure turnout buying cleanly instead show that it is highly effective at bringing clients to the polls, just as current theory predicts.

The first section of this paper shows that existing empirical research on turnout buying makes machine politics seem unworkable. The second section argues that compound measurement accounts for these lacklustre results. It then extends insights from a formal model due to Gans-Morse, Mazzuca, and Nichter (2014) to show why underestimates likely plague studies of machine politics around the world.

The third section discusses the heroic assumptions needed to separately measure turnout buying using existing data sets. It also describes two simple approaches that improve on current measures. Researchers can measure voters' beliefs about their expected behaviour in exchange for material benefits. This demand-side measure can be tapped through standard survey items. A more ambitious approach measures the machine's intended treatment of electoral constituencies. This supply-side measure requires knowledge gathered from party documents or informants, but recent research has been so successful at penetrating machines that even this approach may be feasible in some cases.

The fourth section compares the performance of the existing compound measures to the proposed new measures using individual-level survey data and information on machine strategy at the constituency level in Mexico's recent elections. The compound measures imply that turnout

[2]In math and the physical sciences, compound measures represent concepts that combine two or more unlike units. For instance, *speed* = (*distance*/*time*). Compound measures differ from composite measures such as indexes and scales that aggregate related simple measures into an overall score.

[3]See Cox and Kousser (1981) on the United States, Nichter (2008) and Szwarcberg (2015) on Argentina, Muhtadi (2019) on Indonesia, Aspinall et al. (2022) on Southeast Asia, Mares and Young (2019) on Eastern Europe, the chapters in Kitschelt and Wilkinson (2007) and Schaffer and Schedler (2007) on multiple countries, and the studies in the meta-analyses below.

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buying fails, whereas the new measures instead indicate overwhelming success. The conclusion discusses measurement issues in the study of machine politics.

Strong Theory, Weak Findings

To assess the average impact of electoral clientelism on electoral participation, I conduct meta-analyses using a census of existing studies. I include studies that report the causal effect (for experimental studies) or estimates from a regression analysis (for observational studies) of the influence of vote-buying attempts on turnout at the individual level or in electoral constituencies.[4] I include findings in published books and articles as well as publicly available working papers to mitigate the effects of publication bias (van Aert, Wicherts, and van Assen 2019). Analyses may be of elections for any public office and from any country-election-year, though nearly all studies are from national-level contests conducted over the past two decades. The search produced thirteen studies and forty-nine estimates of the effect of vote buying on turnout.

Making valid comparisons using meta-analysis procedures requires studies to use similar treatments, outcomes, and units of analysis. I thus analyze the estimated effects on individuals, all but one of which are from observational studies, separate from the effects on electoral constituencies, all of which come from experimental studies.[5] Appendix A reports details on the search procedure, criteria for inclusion, the included studies, and the meta-regression.

All included studies use compound measures of vote-buying attempts. Six of the seven studies that examine effects on individual behaviour rely on survey questions that ask respondents if they received a material offer. None of the questions allowed respondents to indicate whether they were asked to turn out, abstain, or choose the machine's candidate.[6] Carreras and Irepoğlu (2013) used America's Barometer 2010 survey that asked 'In recent years and thinking about election campaigns, has a candidate or someone from a political party offered you something, like a favor, food, or any other benefit or thing in return for your vote or support?'[7] In their analysis of seventeen sub-Sahara African countries, Guardado and Wantchekon (2018) employ the Afrobarometer question 'During the [year] elections, how often (if ever) did a candidate or someone from a political party offer you something, like food or a gift, in return for your vote?' (also see Bratton 2008).[8] Imai, Park, and Greene (2015) used a list experiment item asking respondents to indicate if they had 'Received a gift, favor, or access to a service in exchange for your vote'. Questions in the Comparative Study of Electoral Systems (CSES) 2000 used by Buendía and Somuano (2003) and the Mexico 2000 Panel Study employed by Cornelius (2004) ask only about the receipt of material incentives, implying that vote buying could have been for any purpose.

I include Hicken colleagues (2017) study of electoral clientelism in the Philippines (2013) in the meta-analysis of observational studies. These authors experimentally expose individual voters to

[4]Findings by Calvo and Murillo (2019), Cantú (2019) and Frye, Reuter, and Szakonyi (2019) could not be associated with individual or constituency-level effects on turnout. I also excluded observational studies without control variables or matching techniques, such as Muhtadi (2019, 212) and Conroy-Krutz and Logan (2012, 636–7).

[5]If authors report multiple models, I select the one using the most controls. I prioritize models using electoral returns over self-reports of turnout (e.g., in Vicente 2014 and Cruz, Keefer and Labonne 2016). To avoid violating the assumption of independent observations (López-López et al. 2018), I include Guardado and Wantchekon's (2018) estimate for Kenya 2002 using Afrobarometer data but not Kramon's (2013) estimate using the same data and measurement strategy.

[6]Nichter (2008) and Kramon (2016) study turnout buying but do not estimate efficacy.

[7]Carreras and Irepoğlu (2013, 615, Model 1) report one estimate for 18 Latin American countries using an unweighted pooling of all respondents. I generated country estimates using the authors' data and coding approach, which they generously provided (Appendix A). I use these estimates because they come from samples of the constituencies where turnout occurs. Excluding all 18 cases yields an estimate of 1.7pp [95% confidence interval: −1.2, 4.6] for the remaining observational studies, which supports my argument.

[8]I use Guardado and Wantchekon's (2018) individual country estimates. Excluding all these cases leads to the estimate of −0.9pp [95% confidence interval: −4.0, 2.2] for the remaining observational studies, which favours my argument more than the reported estimate.

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treatments that subjects could decline. Because it is unethical for researchers to distribute electoral gifts to citizens that may shift election outcomes, this and other experimental studies discussed below treat constituencies with anti-vote-selling messages. Hicken colleagues (2017) randomly assigned individual voters to a control group or to receive experimenters' requests that they make one of two anti-clientelism promises: (1) 'I won't take the money', or (2) 'I'll take the money, but will vote my conscience'. Uptake was around 50 per cent. The opportunity to decline treatment makes this study similar enough to observing machines' vote-buying offers to include the two types of findings in the same category for meta-analysis.[9]

Figure 1 shows a forest plot to summarize the results of the meta-analysis for the studies using individual-level data. In the figure, each study's effect size is shown along with the confidence interval.[10] I employ a random-effects meta-analysis model to account for the inclusion of studies of different country-election years where the strategic conditions yield incentives for different levels of turnout buying. See Appendix A.

Together, these studies estimate that vote-buying attempts *may* increase individual-level turnout propensity by 1.5 percentage points (pp), but this quantity falls short of statistical significance [95% confidence interval: −0.1, 3.2]. Just seven of the forty-one estimates yield evidence consistent with the hypothesis that turnout buying is effective. The other thirty-four estimates lead to the conclusion that turnout buying does not work as intended, possibly even causing a *decrease* in electoral participation.

Six studies examine the effects of vote-buying offers on aggregate turnout in electoral constituencies. Their experimental designs use compound treatments intended to suppress the effects of all forms of vote buying by deploying anti-vote-buying messages and/or encouraging voters to choose based on non-clientelist grounds. Vicente (2014) treated constituencies in Sao Tomé e Principe with leaflets stating 'Do not let your conscience take a "banho". Vote your conscience.' Schechter and Vasudevan (2021) disseminated a sixty-second radio spot including anti-clientelist and anti-corruption messages. Blattman et al. (2019) distributed anti-clientelist leaflets and posters, made resolutions against electoral clientelism in village meetings, and made turnout phone calls the night before the election. Fujiwara and Wantchekon (2013) enlisted candidates to campaign on programmes rather than clientelism. Cruz, Keefer, and Labonne (2016) distributed flyers featuring the candidates' intended allocation of development funds.[11]

These studies compare treated units to untreated units in contexts that are rife with clientelism. The implied counterfactual is that absent the treatment, turnout would have been at the level observed in the untreated units. Vicente writes, "I followed a voter education campaign against vote-buying practices ... In the event that the campaign reduced the effectiveness of vote buying, and this constituted the campaign's main mechanism of change, I am able to infer the likely effect of vote buying on the electoral outcomes" (2014, 357, also see 361, 377). Consistent with this logic, I reverse the sign of the estimated effect of treatments designed to suppress the effects of vote buying on turnout.

Figure 2 shows a forest plot to summarize the results of the meta-analysis for the experimental studies using aggregate data.[12] As a group, these studies show no effect of vote-buying attempts on

[9]Excluding Hicken et al. (2017) yields an estimate of 1.8pp [95% confidence interval: −0.1, 3.7] for the remaining observational studies, which supports my argument.

[10]Most studies report effect sizes of vote buying on turnout and their confidence intervals. I replicated Carreras and İrepoğlu (2013) and Cornelius (2004) using the original specifications and data. Appendix A.

[11]I also include Banerjee et al. (2011, Study 1) who treated voters with messages designed to suppress ethnic voting, which Chandra (2005) argues is the marker of clientelist voting in India. Excluding Banerjee et al. yields null effects at 0.5pp [95% confidence interval: −0.9, 2.0] for the remaining experimental studies.

[12]Two studies could not be included because their treatments and outcomes are incommensurate with the included studies. Larreguy, Marshall, and Querubín's (2016) natural experiment uses a regression discontinuity of 2012 election returns in Mexico, finding that adding one ballot box per precinct (which typically reduces the number of voters per box) increases turnout by 0.85pp [95% confidence interval: 0.6, 1.1]. Bowles, Larreguy, and Liu (2020) use a similar approach but examine vote share rather than turnout.

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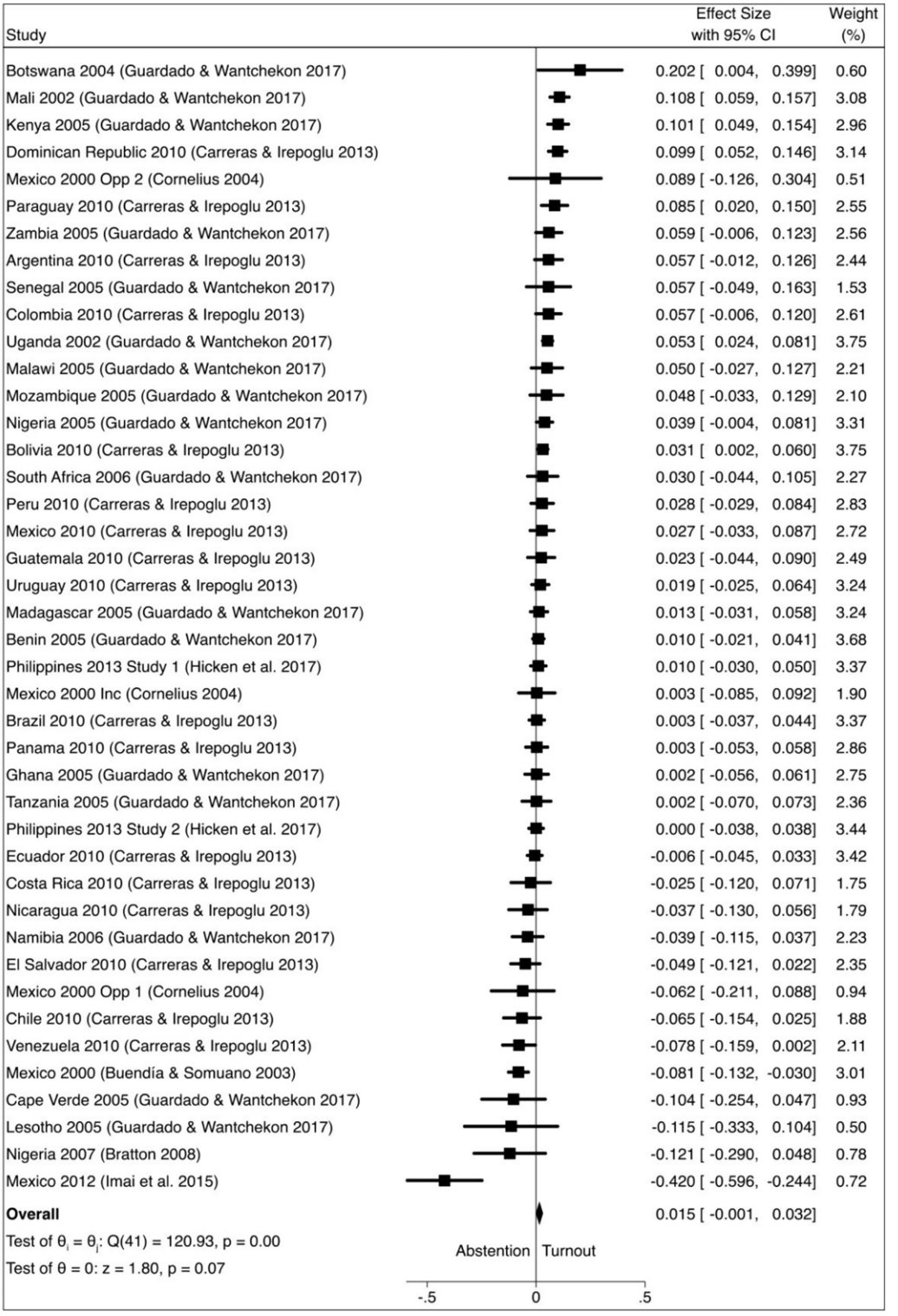

Figure 1. Meta-analysis of observational studies on vote buying.
Note: See Appendix A for details.

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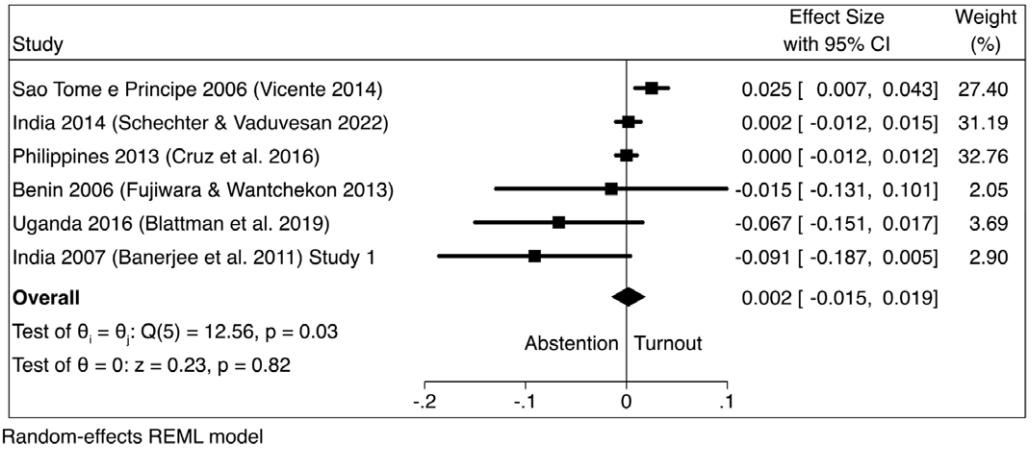

Figure 2. Meta-analysis of experimental studies on vote buying.
Note: See Appendix A for details.

turnout (0.2pp [−1.5, 1.9]). Strikingly, just one of six individual estimates surpasses statistical significance.

These findings should give vote buyers pause. Strong theory indicates that turnout buying is central to machine politics, but empirical findings indicate that it is ineffective, on average. As noted in the introduction, the return on investment appears *worse* than door-to-door canvassing alone, which is a central component of most turnout-buying efforts (Green and Gerber 2019). Despite strong statements in the literature regarding the use of selective benefits to buy electoral participation and a straightforward compliance mechanism, existing studies imply that political machines should not exist.

Why Standard Measures Underestimate the Effects of Turnout Buying

I argue that the mismatch between strong theory and null empirical findings is an artefact of measurement. Current empirical work uses measures that lump together all forms of votebuying. If machines diversify their vote-buying strategies, as current theory suggests, then these compound measures include affirmative responses to contrasting behaviours. Analyses of such data underestimate the effectiveness of turnoutbuying and can generate false null or even negative estimates.

To appreciate the effects of lumping, imagine that all vote-buying strategies are successful. Each case of turnout buying would cause electoral participation, resulting in scores on the compound measure of vote buying and turnout of [1, 1]. Each case of abstention buying would result in abstention, represented by [1, 0] and each case of vote-choice buying would have no effect on turnout, leading to either [1, 1] or [1, 0]. A dataset composed of these scores would underestimate the influence of turnout buying because abstention buying exerts countervailing effects and vote choice buying adds null effects. Appendix C shows a simulation.

The degree of underestimation depends on the level of machines' strategy diversification. I demonstrate that machines have incentives to engage in enough vote-choice and abstention buying to severely bias estimates of the impact of turnout buying on electoral participation. To generate predictions about the magnitude of each type of clientelism, I use a model due to Gans-Morse, Nichter, and Mazzuca (2014), hereafter GNM.[13]

[13]Other studies argue that machines should diversify but do not provide straightforward ways of deriving the relative use of each strategy (Díaz-Cayeros, Estévez and Magaloni 2016; Dunning and Stokes 2008).

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GNM models the investment that rational machines should make in each vote-buying strategy. The machine (m) can choose between buying turnout (strategy T), abstention (A), vote choices (V), or doubly persuading voters (D) to turn out and vote for the machine. The opposition party (o) cannot distribute benefits. The machine knows that voters' utility for party $P \in (m, o)$ is $U^P(x_i, c_i) = -k|x^P - x_i| + b_i - c_i$ where the distance metric represents the expressive utility of voting for a party that is closer to the voter's ideal point x_i on an ideological dimension X, c_i represents the cost of voting, and b_i represents the value of gifts that the machine may make. The k −term represents the value of expressing support for one's preferred party relative to the gift and the cost of voting.

The machine also knows that the benefit of buying one vote choice is equivalent to turning out two of its supporters or stoking abstention among two of its opponents. Hence, it is willing to pay the most expensive voter targeted for vote-choice buying (V) twice as much as the most expensive voter targeted for turnout (T) or abstention (A). Finally, the two parties' ideological positions are fixed, symmetric, and located at the extremes of the voter distribution, the cost of voting is positive, and voters are uniformly distributed over the ideological domain.[14]

With these assumptions, GNM shows which voters are subject to each strategy in Figure 3 below. The horizontal axis represents ideology, and the vertical axis represents the cost of voting. The bold line shows the 'Turnout Indifference Vertex'. Below this line, citizens vote, unless they are given electoral gifts to abstain; above it, they abstain unless they are given gifts to participate. The bold line climbs as it nears each party's ideological location because citizens with more intense preferences for the parties are willing to sustain higher costs for voting. I add the term $\tilde{x}$ to describe the cut point between machine supporters to the right and opponents to the left.

To compare the relative magnitude of areas A, T, D, and V, I first derive the equations for the lines labelled in Figure 3.[15] I take advantage of the symmetry between x^M and x^O to drop the superscript. Supporting non-voters have a reservation value of 0 and ideal points between $\tilde{x}$ and x. Buying turnout requires paying $U^M + b_i \geq 0$. The maximum cost that the machine is willing to pay for one vote (and therefore the height of the areas designated A, D, and T) is $b^\star$. Substituting for U^M and solving for c, yields the equation for the turnout-buying line l_4 as $c_i = -k(\tilde{x} - x_i + x) + b^*$. and for l_1 as $c_i = -k(\tilde{x} - x_i + x)$.[16] Opposition voters have a reservation value of U^O and lie between $\tilde{x}$ and $-x$. Inducing abstention requires the machine to pay $b_i > U^O$. Following the same procedure as above yields the equation for l_7 as $c_i = -k(x_i - \tilde{x} + x) - b^*$ and l_2 as $c_i = -k(x_i - \tilde{x} + x)$. Finally, because buying a vote choice benefits the machine twice as much as buying turnout or abstention, it is willing to pay $2b^\star$ (the expression for line l_3) to those whose cost of voting is less than $-kx$ (the expression for line l_6).[17] With this information, I generate the Cartesian coordinates for the vertices of each labelled area[18] and use Gauss' Area Formula (AKA the Shoelace Algorithm) to determine their areas. The calculations appear in Appendix B. The areas are as follows:

[14]GNM (2014, 420, footnote 12) guarantee an interior solution and ensure that area V exists by assuming that '(1) some indifferent citizens [on the ideological dimension x] vote (formally, this requires $\underline{C} < x_M$); and (2) even with electoral clientelism, there exist strong supporters who do not vote (formally, this requires $\overline{C} > b^*$, where b^* is defined below as the most expensive payment to nonvoters).'

[15]I assume that ballot secrecy exists and thus does not consider the case where area V is non-rectangular, as in GNM (2014, 426, Fig. 4b).

[16]These equations differ from GNM 2014, footnotes 23, 24, and 28 because I incorporate the k-term the authors discuss on p. 427 and $\tilde{x}$ that appears on p. 428.

[17]When $c > x$, it is more efficient to pay opposing voters to abstain (GNM 2014, 423).

[18]Starting at the highest point and moving counterclockwise, the vertices for each area are as follows: T $(x, -k\tilde{x} + b)$, $(\tilde{x}, -kx + b)$, $(\tilde{x}, -kx)$, $(x, -k\tilde{x})$; A $(-x, k\tilde{x})$, $(-x, k\tilde{x} - b)$, $(\tilde{x} - 2b, -kx)$, $(\tilde{x}, -kx)$; and D $(\tilde{x}, -kx + b)$, $\left(\tilde{x} - (b/2k), (b/2) - kx\right)$, $(\tilde{x}, -kx)$. Starting at the upper-left and moving counterclockwise, the vertices for area V are $(\tilde{x} - 2b, -kx)$, $(\tilde{x} - 2b, -kx - x)$, $(\tilde{x}, -kx - x)$, $(\tilde{x}, -kx)$.

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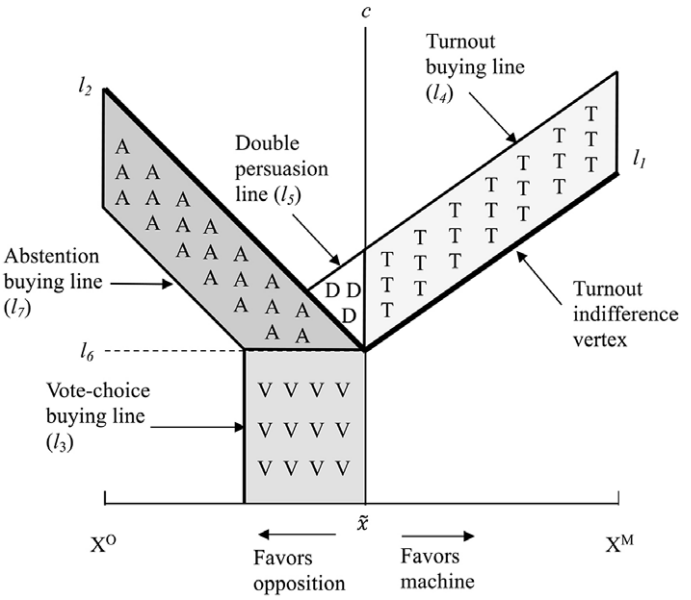

Figure 3. Varieties of electoral clientelism.
Note: The figure replicates GNM (2014) Figures 2 and 3 with added notation. GNM notes 'Citizens labelled T are nonvoting machine supporters who are mobilized due to turnout buying. Citizens labelled V are opposing voters who switch their votes due to vote-choice buying. Citizens labelled A are opposing voters who stay home due to abstention buying. Citizens labelled D are opposing nonvoters who turn out and vote for the machine due to double persuasion.'

- Area V (vote choice buying): $2b^*x$
- Area T (turnout buying): $b^*(x-\tilde{x})$.
- Area D (double persuasion): $\frac{b^{*2}}{4k}$.
- Area A (abstention buying): $b^*(\tilde{x}-x-b^*)$.

The proportion of total voters that are offered an electoral gift for turnout is: $\frac{(T+D)}{(T+D+A+V)} = \frac{b^*-4k\tilde{x}+4kx}{b^*-4kb^*+16kx}$. I sum turnout buying and double persuasion because both strategies seek to increase turnout. The proportion of voters given a gift to abstain is $\frac{A}{(T+D+A+V)} = \frac{4k(\tilde{x}-b^*+x)}{b^*-4kb^*+16kx}$.; and the proportion of voters given a gift for their choice only is $\frac{V}{(T+D+A+V)} = \frac{8kx}{b^*-4kb^*+16kx}$.

Compound measures yield more severe underestimates of the effectiveness of turnout buying as the proportion of turnout buying falls relative to abstention buying. GNM argue that machines should employ the most turnout buying when their support based on ideology is high and the least when support is low. Without loss of generality, I set $x = 1$ and then vary $\tilde{x}$ to represent shifts in machine support.[19] The left panel of Figure 4 shows low machine support ($\tilde{x} = 0.2$). such that the machine wins 40 per cent of the vote and the opposition wins 60 per cent. In this case, turnout buying and double persuasion reach their minimum at 20 per cent of electoral clientelism, with abstention buying at 30 per cent and vote choice buying at 50 per cent. The right panel shows high machine support ($\tilde{x} = -0.2$). with the machine winning 60 per cent of the vote and the opposition winning 40 per cent. Here, turnout buying and double persuasion reach their

[19]I show up to $b = (1/2)$ where the machine just buys the vote choices of all opposition voters. This is the maximum permissible size of b that ensures an interior solution, given that $k = (1/2)$.

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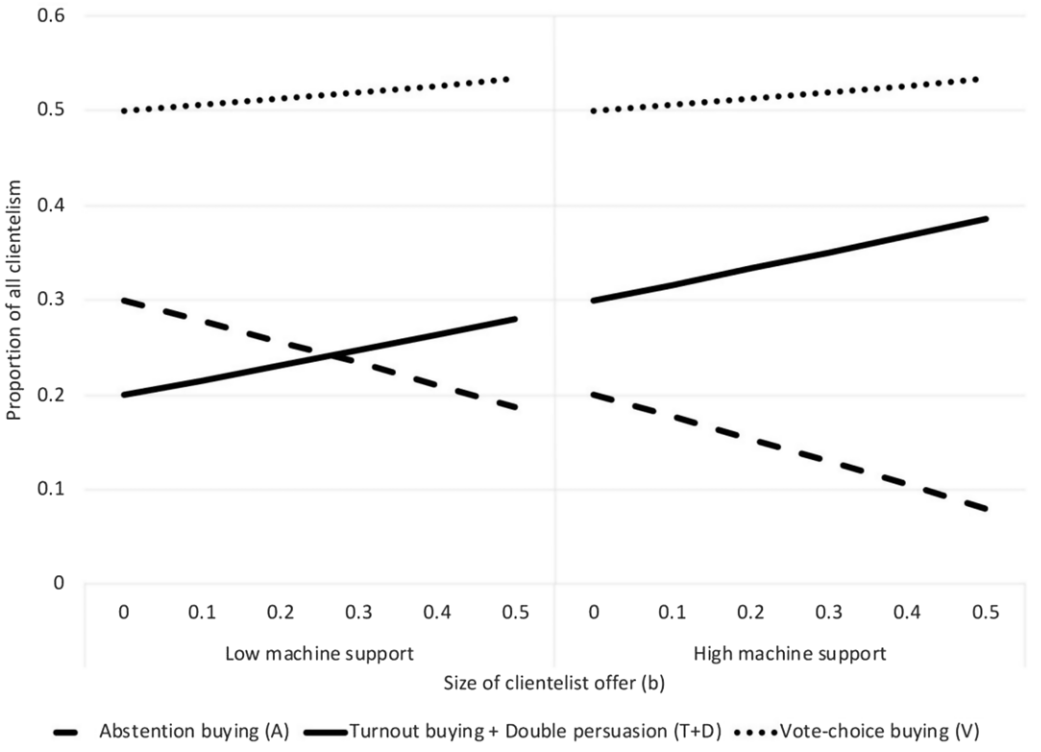

Figure 4. The proportion of voters targeted for abstention, turnout, and vote-choice buying.
Note: Simulations derive from the GNM model. Both panels set $x = 1$ and then vary $\tilde{x}$ to represent shifts in machine support. The left panel sets $\tilde{x} = 0.2$. such that the machine wins 40 per cent of the vote. The right panel sets $\tilde{x} = -.2$. such that the machine wins 60 per cent of the vote.

maximum at 38 per cent of all electoral clientelism, abstention buying at 9 per cent and vote choice buying at 53 per cent.[20]

If machines diversify their strategies as theory suggests, what would empirical models of turnout show when each form of electoral clientelism is measured separately or lumped together as current analyses do? To answer this question, I generated data ($n = 1{,}000$) where 10 per cent of voters are targeted for electoral clientelism. Among the 100 voters given electoral gifts, those targeted for abstention buying (A), vote-choice buying alone (V), and turnout buying alone, or turnout buying and double persuasion, (T + D) mirror the minimum and maximum proportions for T + D shown in Figure 4. Minimum turnout buying reflects the scenario that causes the greatest underestimation of turnout buying's effects while maximum turnout buying represents the circumstances that cause the least underestimation when using the compound measure. I assume each strategy works flawlessly so that all voters targeted for turnout vote and no citizens targeted for abstention vote. Turnout is randomly assigned for untargeted voters and those targeted for vote choice buying.

Figure 5 shows OLS coefficients and 95 per cent confidence intervals from regression models on the simulated data where turnout is the dependent variable. As one would expect, the estimate of turnout probability is negative and significant among voters targeted for abstention, and positive and significant among those given gifts to participate. T here is no effect on those asked

[20]Including abstention buying leads compound measures to underestimate the effect of turnout buying on turnout more than does including vote-choice buying. Using the simulated dataset and models represented in Fig. 5 shows that each added case of abstention buying causes the same degree of underestimate as 1.67 cases of vote-choice buying.

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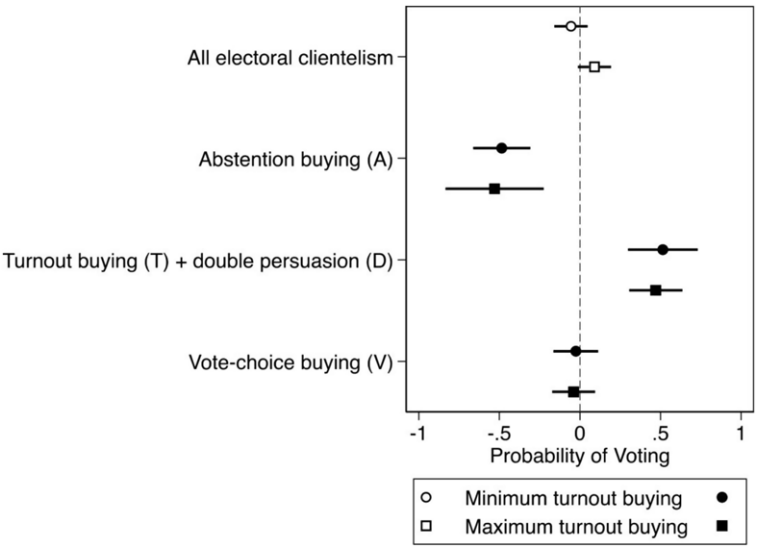

Figure 5. Regression estimates of the effect of vote buying on turnout.
Note: Models use $N = 1{,}000$ and assume 10 per cent of the electorate is targeted for electoral clientelism. The magnitude of each strategy comes from Figure 4. Minimum turnout buying is 20 per cent, with abstention buying at 30 per cent, and vote choice buying at 50 per cent (see the leftmost portion of Figure 4). Maximum abstention buying is 39 per cent, with abstention buying at 8 per cent, and vote choice buying at 53 per cent.

only to choose the machine. Yet lumping all forms of electoral clientelism together yields a null result, whether turnout buying is set to the minimum or maximum level predicted by the GNM model.

Assumptions and Best Practices

This section proposes two approaches for decomposing electoral clientelism into its components so that turnout-buying attempts can be measured independently of abstention and vote-choice buying. Before doing so, it briefly shows that repurposing the data in existing studies requires heroic assumptions that cause systematic bias in estimating the effects of turnout buying on voter participation.

Ideally, researchers could repurpose the compound measures in existing survey data sets that cover many country-election-years. A broad search generated a catalogue of 134 public opinion surveys covering 62 countries, all of which use compound measures of electoral clientelism. The data sets include well-known surveys from Afrobarometer, the Comparative National Elections Project (CNEP), the Comparative Study of Electoral Systems (CSES), the Latin American Public Opinion Project (LAPOP), González-Ocantos et al., as well as country-specific studies such as the Argentina, Brazil, Mexico, and Turkey panel studies. Hypothetically, researchers could use two possible approaches to decompose the existing compound measures, but both induce substantial measurement error.

In one approach, researchers could assume *perfect targeting*. As shown in Table 1, theory argues that machines should target supporters who are unlikely to vote for turnout and strongly opposed voters who are likely to vote for abstention. They should also target weakly opposed voters who are unlikely to turnout for double persuasion and those who are likely to turnout for vote choice buying (Gans-Morse, Mazzuca, and Nichter 2014; Nichter 2008; Stokes 2005). Researchers could impute the machine's intent from its targeting practices. See Appendix F for such an analysis using

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Table 1. Theorized intent of electoral gifts and effects on turnout (in parentheses)

Predicted probability of voting for the machine absent an electoral gift	Predicted turnout absent an electoral gift	
	High	Low
Supporter	No Gift (0)	Turnout buying (+)
Weakly opposed voter	Vote choice buying (0)	Double persuasion (+)
Strongly opposed voter	Abstention buying (–)	No gift (0)

Note: The signs in parentheses indicate the predicted effect on turnout. Both turnout buying alone and double persuasion (turnout buying and vote-choice buying) seek to increase turnout.

data from Mexico's 2000 elections and building on Buendía and Somuano (2003). Nevertheless, this approach runs into three problems.

First, measuring targeting requires well-timed panel surveys that tap machine support and turnout propensity *before* gifts are offered. Second, current theory offers no guide for distinguishing between weakly opposed and strongly opposed voters. The threshold emerges endogenously from the most opposed voter willing to accept the machine's offer and comply with the requested behaviour (Stokes 2005). Finally, machines make targeting errors (Ravanilla, Haim, and Hicken 2021; Schneider 2019) because brokers' incentives diverge from the machine's strategy (Stokes et al. 2013) and because the confounding effects of standard campaigns make it impossible to forecast voter type before Election Day (Greene 2021). The assumption of perfect targeting is thus impractical and requires too many unsupported assumptions to be viable.

Researchers could instead assume *perfect compliance* with abstention-buying efforts. Non-voters who receive electoral gifts would be dropped from the data, leaving only non-recipients and those targeted for turnout and vote choices. This assumption may not be as demanding as it seems because brokers can readily monitor the polls or, in countries that require voters to present identity cards, they can ensure abstention by renting credentials. Appendix G shows such an analysis using Afrobarometer data from seventeen countries in sub-Sahara Africa and building on Guardado and Wantchekon (2018). Nevertheless, assuming perfect compliance overestimates the effect of turnout buying because failed turnout buying would be incorrectly counted as successful abstention buying.

As an alternative, I propose two simple approaches that improve measures in new data collection efforts using post-treatment surveys. First, by employing a *demand-side measure,* researchers can simply ask recipients what they thought the machine wanted them to do, including abstaining, turning out, choosing the machine over the opposition, or more than one behaviour. The predicted effects of the various treatments on turnout appear in Table 1: turnout buying and double persuasion should increase the propensity to turnout, vote-choice buying should have no effect on turnout, and abstention buying should have a negative effect. To capture the impact of gifts intended to induce contrasting behaviours, such as one party paying for turnout and another party paying the same voter to abstain, each behaviour should be coded into a separate variable.[21]

Second, in some instances, researchers may be able to use a *supply-side measure* by discovering how the machine intended to treat individuals or constituencies. Clientelism research has generated dazzling fieldwork with researchers sometimes becoming deeply embedded in broker

[21]In competitive clientelism, voters targeted by two parties should have the strongest impetus to turnout when the parties play T + T, moderate impetus when they play T + V, null when they play V + V, moderately negative impetus when they play V + A, and very negative when they play A + A. The incentive when T + A is played is indeterminate but can be estimated empirically if each type of requested behavior is coded separately.

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networks and learning striking information about their operations (see, among others, Auerbach and Thachil 2022; Cruz, Keefer, and Labonne 2016; Nichter 2018; Ravanilla et al. 2021; Schneider 2019; Stokes et al. 2013; Szwarcberg 2015; Zarazaga 2016). It is thus not out of the question that researchers could reconstruct the machine's strategy from first-hand observation, interview data, or strategy documents.

Estimating the Effects of Turnout Buying

This section compares the estimated effect of turnout buying on electoral participation using the standard compound measure and the two newly proposed measures of turnout buying alone. Data for the demand-side measure come from an original sample survey fielded in Mexico just after the 2021 midterm elections. Data for the supply-side measure comes from a leaked strategy document from the incumbent PRI in the 2017 State of Mexico elections.

Mexico is a particularly useful case study for three reasons. First, estimates show that many voters are exposed to vote-buying offers, ranging from 21.2 per cent of the electorate in the 2012 elections (Nichter and Palmer-Rubin 2015) to over 42 per cent in the 2018 elections (Greene and Simpser 2020). Second, there are more estimates of the effect of electoral clientelism on turnout for Mexico than any other country and, like the overall estimates in Figures 1 and 2, a meta-analysis of the six Mexico estimates shows no impact on turnout (−6.9pp [95% confidence interval: −19.7, 5.8], see Appendix A). Finally, the election years analyzed below are associated with low levels of genuine support for the machine and thus, as predicted by the GNM model, less turnout buying. These circumstances make Mexico a hard case for testing the hypothesis that turnout buying is effective. I show that the practice does not appear to influence behaviour when using a compound measure, but using clean measures of turnout-buying attempts indicates that it is highly effective.

Demand-Side Measurement Using Survey Data

Clientelist transactions range from highly personal connections where local brokers interact with their neighbours to impersonal relations where machines distribute goods at rallies, in markets, or at community events. Whether explicitly or implicitly, machine personnel typically signal what they expect voters to do in return for electoral gifts during such transactions.

To measure voters' perceptions of the machine's expectations for their behaviour, I use data from the Mexico 2021 Electoral Justice Survey that was fielded to an online sample of 1,233 registered voters shortly after the June 6 midterm elections. Respondents were furnished by Netquest, a professional sample-supply firm with about 190,000 participants in Mexico. Because people with a lower socioeconomic status are more frequently targeted for electoral clientelism than affluent citizens (Calvo and Murillo 2019; Castro Cornejo and Beltrán 2020), recruitment was limited to lower middle-class-to-poor respondents that account for 70 per cent of Mexican citizens. Using the seven-level socioeconomic scores from the Mexican Association of Marketing Research and Public Opinion that range from A (most affluent) to E (least affluent), the sample includes 16 per cent C- respondents, 55 per cent D+, and 30 per cent D-/E.[22] See Appendix D.

To measure receipt of a vote-buying offer, I employed a modified direct question. Respondents first saw the following preamble:

> Now we would like to ask you about your own experiences. Sometimes, the political parties give groceries, cash, cash cards, construction materials, or medicine, or they may help get access to government programmes or services, give educational subsidies, or medical attention. They also may offer jobs or legal services.

[22]https://www.amai.org/descargas/CUESTIONARIO_AMAI_2022.pdf.

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On the next page, respondents were asked 'In 2021, were you offered any money, groceries, cash cards, gifts, favours, work, or access to any government programs or services by a political party?' Respondents who answered affirmatively were asked 'What did they ask you to do or imply that you should do? Check all that apply.' The options included 'Vote for a party' which I interpret as vote-choice buying, 'Go vote, without saying which party I should choose' which I interpret as turnout buying, and 'Don't vote' which I interpret as abstention buying. If respondents selected both vote for a party and turnout, I score them as targets of double persuasion.[23]

The question content and format were designed to diminish social desirability bias. Psychometricians argue that respondents fib to avoid embarrassment in the presence of an interviewer and potential repercussions from third parties (Tourangeau and Yan 2007). The survey avoided interviewer effects by having respondents self-administer on their own devices, a modality that is known to reduce social desirability bias relative to face-to-face and telephone interviewing (Kreuter, Presser, and Tourangeau 2008; Presser and Stinson 1998). In addition, the preamble sought to reduce stigma by communicating that survey researchers share with the respondents common knowledge about the existence of electoral handouts. To the extent that respondents underreport being targeted with electoral gifts for turnout, findings would most likely incline against my hypothesis that turnout buying is effective.

Some research on vote buying indicates that indirect questions such as the list experiment diminish social desirability bias more than direct questions (Corstange 2009; González-Ocantos et al. 2012; Nichter and Palmer-Rubin 2015). However, indirect measurement techniques disallow follow-up questions, such as which party made the offer and what respondents believed they were asked to do. In addition, Castro Cornejo and Beltrán (2020) show that list experiments' high cognitive demands cause greater measurement error among poor and less educated respondents who are more frequently targeted with vote-buying offers.

Using the modified direct question, a total of 17.3 per cent of respondents reported receiving some type of vote-buying offer in 2021. This is the proportion that would be captured using a compound measure and is in the range of other findings in Mexico using both direct questions and list experiments.[24] Decomposing the compound measure shows that vote-choice buying accounts for 54.4 per cent of the vote-buying attempts; turnout buying and double persuasion sum to 19.7 per cent; and abstention buying comprises 2.3 per cent.[25]

The circumstances surrounding Mexico's 2021 midterm elections make it a hard test of the hypothesis that turnout buying was successful. The anti-incumbent coalition formed by the Institutional Revolutionary Party (PRI), the National Action Party (PAN), and the Party of the Democratic Revolution (PRD) accounted for 71.4 per cent of vote-buying attempts but had low support at about 42 per cent of pre-election vote intentions.[26] As predicted in Figure 4, low machine support generated a small proportion of turnout buying, thus making the impact of such attempts challenging to detect in statistical models.

To examine the effects of vote-buying offers on voting, I construct regression models where self-reported turnout is the dependent variable. Offers from all political parties are included. Explanatory variables include total electoral clientelism – the compound measure used in existing observational studies – and each of its components measured separately, along with demographics

[23]Respondents could experience double persuasion either because machine personnel asked them to turnout and to vote for the machine during one or more contact episodes or because they were asked to engage in one behavior but thought it implied that they also engage in the other.

[24]There are no other estimates for 2021 in publicly available datasets. In 2012, Castro Cornejo and Beltrán (2020) found 27 per cent received a vote-buying offer using a direct question and 8 per cent using a list experiment. Employing different 2012 data, Nichter and Palmer-Rubin (2015) found 6 per cent using the direct question and 21 per cent using the list experiment.

[25]$N = 1{,}233$ total respondents. Respondents were considered targets for double persuasion if they responded yes to both the vote-choice and turnout buying questions. Percentages do not add to 100 because some respondents reported that they were asked to do other things or nothing.

[26]https://www.ine.mx/voto-y-elecciones/encuestas-electorales/encuestas-proceso-electoral-2020-2021/.

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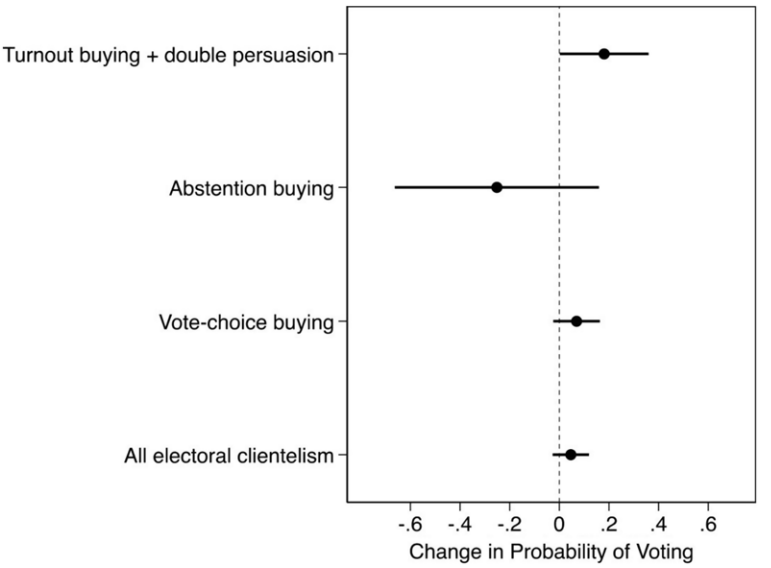

Figure 6. Electoral clientelism in Mexico's 2021 midterm elections.
Note: $N = 680$. Results show findings from two regressions, one with 'all electoral clientelism' and one measuring each component of electoral clientelism separately. I sum turnout buying and double persuasion because the latter includes turnout buying. Estimates show average marginal effects and 95 per cent confidence intervals. See Appendix E.

as controls. I account for differences across those who did and did not receive gifts by pre-processing the data using coarsened-exact matching on demographic variables (Iacus, King, and Porro 2012). This procedure reduced the multivariate L1 distance, a measure of imbalance, from 0.45 to just 0.005. See Appendix E.

Figure 6 shows the striking results of the analysis. Using the compound measure that lumps all forms of electoral clientelism together shows that gifts *may* have increased turnout by 4.7pp [95% confidence interval: −2.6, 12], but this estimate is far from statistically significant. Measuring turnout buying separately tells a different story. Now the estimated effect of turnout buying on turnout is a whopping 18.1pp [95% confidence interval: 0.2, 36].[27] Appendix E shows models using the matched and unmatched data, as well as results using alternative measures of turnout buying.

The decomposed results make machine politics appear much more viable. However, self-reports of turnout can be inflated and response bias can depress reports of vote-buying offers. As an alternative, the next section uses supply-side measures of electoral clientelism and public records of precinct-level turnout.

Supply-Side Measurement Using Information on the Machine's Strategy

In the run-up to the hotly contested 2017 gubernatorial election in the State of Mexico, the incumbent PRI engaged in a massive vote-buying effort (Beltrán and Castro Cornejo 2017). The state is home to the most powerful contemporary faction of the PRI, the Grupo Atlacomulco, which produced the then-president Enrique Peña Nieto (2012–2018). Despite this vaunted history, pre-election surveys showed the party's candidate, Alfredo del Mazo, running just a few percentage points ahead of Delfina Gómez from the new leftist MORENA party. The close

[27]This increase comes mainly from removing the (predicted) null effect of vote-choice buying from the compound measure. In this case, there were so few reports of abstention buying that this variable exerted minimal impact in dragging down the influence of turnout buying.

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election attracted extra attention because it was considered a bellwether for the presidential election the following year which would pit MORENA's Andrés Manuel López Obrador against struggling rivals. Given the substantial stakes, the PRI's storied clientelist machine shifted into high gear. The investigative journalism group Animal Político reported that 245 million pesos (about USD 13 million) were channelled from the federal budget through a complex corruption scheme to the PRI's vote-buying effort in the State of Mexico (Ureste and Roldán 2022).

In the closing days of the gubernatorial campaigns, López Obrador posted online what he claimed was a PRI strategy document. The report, dated May 2017, is titled 'Activation of Priority Locations' but is colloquially known as 'The Pentagon' after the name of the neighborhood-level tracking poll that informed its recommendations. It also carries the name Jordi Segarra and his company, Equipo de Campaña, a high-profile international campaign consultancy based in Mexico City, with multiple awards and a vita showing work in 29 countries.

The 154-page report recommends specific strategies to win the gubernatorial race by increasing vote share in 730 of the state's 6,459 electoral precincts, covering about 29 per cent of registered voters. For each precinct, the report uses surveys to characterize the lead or lag of the PRI's candidate (ADM) relative to the candidates from MORENA (DG) and the rightist PAN (JVM). Table 2 shows the report's translated executive summary.[28]

The report recommends strategies consistent with electoral clientelism in 186 precincts. I interpret the mobilization strategy as turnout buying and label the associated districts in bold and underlined text in Table 2. I interpret the demobilizing strategy as abstention buying and label the relevant precincts with bold and italics. I interpret the 'operation nodes' strategy in markets, public squares, shopping centres, and schools as vote-choice buying efforts, labelled in bold.

My confidence that the strategies represent electoral clientelism comes from three elements. First, ample information documents the PRI's use of clientelism in the 2017 state elections. The Specialized Prosecutor for Election Crime (FEPADE) received over 400 accusations of electoral clientelism on Election Day alone. The Pentagon report itself was used as evidence of electoral clientelism in Mexico's Senate (LXIII/2SPR-7-2091/71579) in an investigation by the National Electoral Institute (INE/Q-COF-UTF/58/2017/EDOMEX), and a judgment of the Federal Elections Tribunal (SUP-RAP-185/2017).[29]

Second, the language used in the Pentagon report mirrors terminology that vote brokers used during interviews with the author. Consistent with my interpretation of 'operation nodes' (Strategy 4 in Table 2) as vote-choice buying, brokers said they routinely use market vendors to unobtrusively distribute cash, and that adult education programmes have long formed part of the PRI's clientelist network. Consistent with my interpretation of the demobilizing strategy as abstention buying, brokers refer to 'Campaign B' (Strategy 5 in Table 2) as the set of dirty deeds, including clientelism, used to depress turnout. Brokers frequently used the term 'D-Day' mobilization (Strategy 6 in Table 2) when setting up cash distribution centres and deploying poll watchers on Election Day. I thus interpret this strategy as turnout buying.

Finally, contemporaneous public opinion survey data reinforce the interpretation of D-Day mobilization as turnout buying. I fielded a list experiment embedded in a statewide representative sample survey three weeks before Election Day. The survey generated respondents in sixty-six of the precincts also featured in the Pentagon report. Where the report recommended turnout buying, the list experiment shows that 40 per cent of respondents received electoral gifts, significant at the 0.1 level. The list experiment did not detect any gift recipients in precincts where the Pentagon report did not recommend electoral clientelism.

[28]https://www.scribd.com/document/349133315/349121448-El-Pentagono-EDOMEX.

[29]Senate: https://www.senado.gob.mx/65/gaceta_del_senado/documento/71579, INE: https://www.ine.mx/wp-content/uploads/2017/07/CGex201707-14-rp_3-10.pdf, Tribunal: https://docs.mexico.justia.com/federales/sentencias/tribunal-electoral/2017-09-14/sup-rap-0185-2017.pdf.

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Table 2. 'The Pentagon' report executive summary

1. IDENTIFICATION OF PRIORITY LOCATIONS WHICH DISTRICTS REQUIRE SPECIAL ATTENTION?			
Strategy	Definition	District Characteristics	30 Local Districts * with PENTAGON tracking poll information (April 3 to May 7, 2017)
1. Contrasts	Clearly communicate risk of a MORENA government	Where DG is winning but with a very weak advantage (less than 1.5%)	D2-Toluca, D11-Tultitlán, D42-Ecatepec
2. Direct contact	Direct contact with candidate	Where ADM is winning with a very weak advantage (less than 1.5%)	D3-Chimalhuacán, D14-Jilotepec, D20-Zumpango, D36-Zinacantepec, D38-Coacalco
3. Mass media	Surrogates without candidate	Where ADM is winning with a weak advantage (between 1.5% and 5.2%)	D4-Lerma, D6-Ecatepec, D21-Ecatepec, D31-Los Reyes, D32-Naucalpan
	Social media	Where JVM is winning with a very weak advantage (less than 5.2%)	D30-Naucalpan, D34-Toluca
4. Ground game	Implement 'operation nodes' (markets, public squares, shopping centres, schools, etc.)	Where ADM is winning or losing against DG, but very competitive (less than 1.5%)	**D3-Chimalhuacán, D11-Tultitlán, D14-Jilotepec, D20-Zumpango, D36-Zinacantepec, D38-Coacalco, D42- Ecatepec [vote-choice buying]**
	Establish ties to PAN and PRD	Where ADM has a durable vote; winning by more than 9.2%	D9-Tejupilco, D10-V. de Bravo, D13-Atlacomulco, D45-Almoloya de J.
	Multilevel women's groups	Districts where the PAN vote is durable (winning by more than 9.2%)	D17-Huixquilucan, D29-Naucalpan
	Breakfast with ADM	Nodes, markets, swing voters	Markets in all districts
5. Campaign B	Promote abstention	Where ADM is losing against dominant DG (by more than 9.2%)	***D23-Texcoco, D37-Tlalnepantla, D43-C. Izcalli, D44-N. Romero [abstention buying]***
	Deactivate vote through call centre using push poll	Where DG vote is durable.	D5-Chicoloapan, D37-Tlalnepantla, D43-C. Izcalli
	Activate campaign to undermine JVM	Municipalities governed by PAN	18 municipalities
	Portray DG negatively	ADM losing or winning against DG, but very competitive (less than 1.5%)	D12-Teoloyucan, D28-Amecameca, D33-Tecamac, D40-Ixtapaluca
6. D-Day	Mobilize	Where ADM vote is durable; winning by more than 9.2%	**D9-Tejupilco, D10-V. de Bravo, D13- Atlacomulco, D45-Almoloya de J. [turnout buying]**
	Demobilize	Where ADM losing against dominant DG (more than 9.2%)	***D23-Texcoco, D37-Tlalnepantla, D43-C. Izcalli, D44-N. Romero [abstention buying]***

Note: Bold signifies vote-choice buying; underlined bold indicates turnout buying; italicized bold denotes abstention buying. Text effects and information in brackets added by the author. Translated from Spanish by the author.

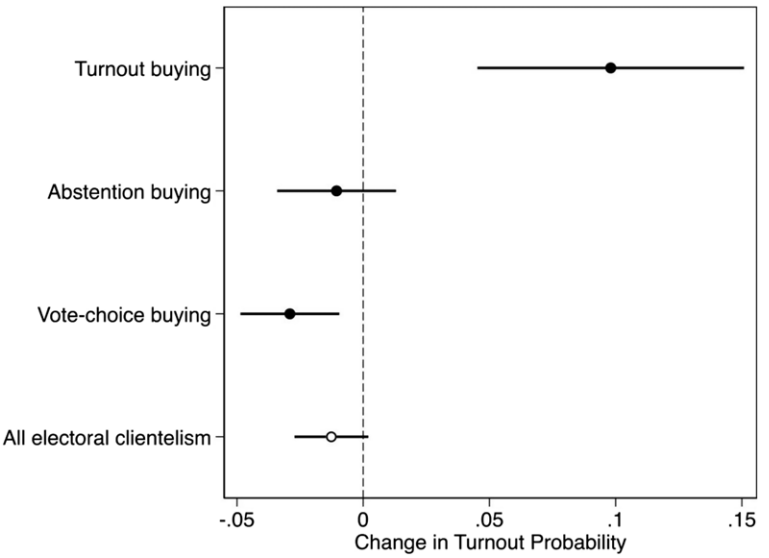

Figure 7. PRI electoral clientelism in the State of Mexico, 2017.
Note: $N = 5{,}951$ precincts. Results show findings from two regressions, one with 'all electoral clientelism' and one measuring each component separately. Estimates show average marginal effects and 95 per cent confidence intervals. See Appendix E.

I first test for the effects of all electoral clientelism on precinct-level turnout data obtained from the State of Mexico's Electoral Institute for the 2017 election. To take account of the machine's targeting strategy, I pre-process the data by matching on precinct level socioeconomic criteria and turnout in the 2015 federal elections. See Appendix E.

As demonstrated in Figure 7, regressing turnout on the compound measure of 'all electoral clientelism' shows null results (−1.2pp [95% confidence interval: −2.7, 0.2]). By contrast, when measured alone, turnout buying alone is associated with a 9.8pp [95% confidence interval: 4.5, 15.1] increase in turnout, significant at the 0.001 level. Neither abstention buying nor vote-choice buying show positive effects, indicating that including these other forms of electoral clientelism washed out the turnout-buying effect when using the compound measure.

An alternative influence could have produced the apparent positive effect of turnout buying. First, as shown in Table 2, 'The Pentagon' report singles out the same precincts where turnout buying was recommended for 'establishing ties to the PAN and PRD'. This could mean attempting to win opposing voters where their parties were weak or demobilizing party leaders. Yet neither effort would have involved buying turnout, and thus is unlikely to cause the observed increased participation in treated precincts.[30]

Conclusion

Political machines have proven to be durable actors in new democracies, yet empirical studies seem to demonstrate that turnout buying is ineffective. I argue that the apparent irrationality of machine politics is an illusion of measurement. Theory indicates that machines should diversify their strategies, and studies using multiple methodologies show that they do. Yet despite the recognition that machines buy turnout, abstention, and vote choices, empirical research using both experimental and observational methods lump all three forms together. Using such

[30]The precincts slated for abstention buying were also marked for 'deactivating the vote' through push polls, but these efforts were ineffective (see Fig. 7).

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compound measures, existing studies imply that turnout buying fails. Without this central strategy, machine politics appears irrational.

Simple adjustments can mitigate this measurement problem, bringing the predictions of standard theory about electoral clientelism and empirical findings into line. Surveys can ask recipients what they thought the machine wanted them to do to uphold their part of the clientelist bargain. This demand-side approach decomposes compound measures, is easy to collect, and allows for nuanced analyses of individual-level variation. Researchers may also be able to measure the machine's intent to treat constituencies with each type of vote-buying offer. Such supply-side measures are not readily available, but scholars have been so adept at discovering how machines operate that measures like those in the PRI's 2017 leaked strategy document may be within reach. Tests using data from recent elections in Mexico demonstrate that turnout buying appears to fail when using the compound measure, but raises turnout by about 10 to 20 percentage points when using clean measures of turnout-buying attempts. Effects may be even larger where machines focus more on spurring electoral participation than they have in recent Mexican elections.

Quantitative analyses of machine politics have blossomed in the last two decades. The next step is to add nuance. Measures that tap specific forms of electoral clientelism can improve descriptive inference by showing the relative use of each strategy and discovering the traits that lead voters to be targeted with one or another strategy. They can also improve estimates of the effectiveness not just of turnout buying, but also abstention buying and vote-choice buying. Better measurement can thus put some of the exciting new theories as well as long-lived arguments and folk wisdom to the test.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/S0007123424000334.

Data availability statement. Replication data for this article can be found in Harvard Dataverse at: https://doi.org/10.7910/DVN/VOB5JL.

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