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CHAPTER

Accidental Cumulation in Studies of Clientelism, Vote Buying, and Governance

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Abstract

The current focus of political science and political economy research in less developed countries lies with problems of governance, with a strong focus on clientelism and specifically on vote buying. With the exception of the Metaketa research on information campaigns, investigations into these topics have proceeded without formal coordination and, for the most part, via individually designed case studies. Despite this disjointedness and informality, some findings seem to have cumulated. This chapter argues that the main novel results to have emerged from this research stream come from uncovering new facts rather than articulating new causal questions or providing new answers to established questions. Thanks to the use of relatively standardized survey instruments and improved sampling methods, case studies now provide precise quantitative estimates of the frequency of vote buying. However, the wide range of new data points suggests rethinking our understanding of previous causal investigations into vote buying may be necessary.

Keywords: [Key words: clientelism, vote buying, causality, description, knowledge cumulation](#)

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Introduction

Recent years have witnessed extraordinary progress in developing analytic techniques to support general claims about causal social and political relationships regardless of site specificity. The innovative Metaketa protocol, developed under the auspices of Evidence in Governance and Politics (EGAP), has pioneered the development of harmonized multi-site studies. The Metaketa initiative represents the main example of a conscious and deliberate operational method for cumulating research findings (Dunning et al. 2019). The approach—as well as meta-analyses more generally—fulfills the ambition expressed more than fifty years ago by Adam Przeworski and Henry Teune that students of comparative politics develop general theories of micro-level relationships that hold regardless of characteristics of the larger systems in which they occur (Przeworski and Teune 1970). Moreover, the Metaketa do so within the modern framework of causal inference (Angrist and Pischke 2009).

The Metaketa protocol has come under scrutiny for its claim to successfully measure variables and causal relationships in uniform ways across different settings (Izzo, Dewan, and Wolton 2022; Slough and Tyson 2022). But even if the Metaketa could solve these challenges, such initiatives are logistically complex and financially demanding, requiring ongoing centralized monitoring and coordination of research teams across the globe. The Metaketa, like meta-analysis more broadly, also have other limitations. They are a good way to evaluate existing theories but are neither suitable for theory or concept development nor very practical for testing novel theories; moreover, they are not terribly well suited for uncovering new descriptive information about the world, since in many cases, that information is required before envisioning a meta-analysis. Are there other ways to generate cumulative knowledge in the social sciences that might be less operationally and financially demanding or that might be expected to unearth new facts?

In this chapter, I consider the example of the extensive but almost entirely uncoordinated research into clientelism and vote buying that has taken place in the field of comparative politics in the last two decades. Excluding the first Metaketa on information campaigns, this research agenda has proceeded unsystematically and informally, with no explicitly or formally coordinated theoretical or empirical agenda. Yet it has generated something that might be considered knowledge cumulation: that is, we can point to things we now know that we did not know a decade ago. The most important findings that have cumulated over recent decades are, I believe, relatively precise estimates of the frequency of clientelism, specifically in the sense of vote buying. Because these estimates have been collected using instruments that are similar, they are roughly comparable. The range of estimates shows that there is considerable divergence across different settings in the substantive importance of the relevant phenomenon under study. This new descriptive information should cause us to update our priors about the substantive importance of vote buying in settings around the world and to reconsider our theories about the strategies that politicians use to win votes. I spell out these arguments below.

Most scholars in the area of comparative politics (and political science more generally) aim to articulate new theories of causal relationships. Descriptive research is often derided or deemed a lower-ranked activity. But because our theories are only as good as the facts they seek to explain, committing resources to collecting descriptive information is a worthy (even if arguably undervalued) enterprise. In the area of comparative politics, which studies politics in hundreds of countries around the world, collecting, collating, and coding systematic information about political activities, institutions, behaviors, and processes is a demanding but critical requirement that underpins our collective research efforts. The hundreds of studies of clientelism that have been published in the last two decades have almost always sought to portray their main contributions as theoretical. In this chapter, I argue that the descriptive information cumulatively produced by this research stream is at least as important (and perhaps intellectually more compelling) than the theoretical contributions it has made.

This chapter proceeds as follows. In order to delineate the topic under discussion, I first provide a definition of clientelism. I then review recent literature. The review sketches the causal questions raised in the recent literature on clientelism and vote buying. I then evaluate what we have learned from this literature. My strategy for doing this is to turn to earlier literatures and examine whether the causal questions and/or answers articulated in recent work exhibit new ideas. I argue that recent work has mainly involved rearticulating questions (and answers) that had already been explored in earlier professional literature. While this does not mean the answers are not valuable, it does suggest little cumulation of theoretical knowledge has occurred. Finally, I document the range of estimates of vote buying available from recent studies and discuss their theoretical importance. I argue that the new descriptive information uncovered across the many studies that have been conducted raises interesting and novel questions, questions that on the whole have yet to be tackled. To conclude, I offer some speculation about the requirements for accidental accumulation of knowledge in our discipline.

What Is Clientelism?

The term clientelism has evolved over decades of research by social scientists and is currently used in a variety of ways. It now most often refers to the offer by politicians of material inducements to individuals or geographically specific groups in exchange for and in expectation of future electoral support. As we shall explore in what follows, this way of formulating the concept immediately generates a series of questions. What kind of voter would sell their vote, and for what in return? Why do politicians seek to buy votes rather than implement public policies that appeal to voters? If a politician provides a material good to a voter today, how can the former be sure the voter will reciprocate in the future? And how are these exchanges sustained in modern democracies, where use of the secret ballot obviates direct observation of voting behavior?

As the formulation just advanced suggests, the study of clientelism is roughly concerned with two dependent variables: whether and how citizens vote. Most attention is directed at the provision of material handouts by political actors prior to an election: do these handouts affect whether recipients vote and how they vote? These are the causal questions that animate much (although not all) of the clientelist research agenda.

These questions arise in part due to the current formulation of the concept of clientelism. When the concept was developed by anthropologists and political scientists in the 1950s and 1960s, similar questions were not asked because clientelism was defined as an ongoing *stable system of exchanges* between patrons and clients. In this earlier formulation, politicians were not buying votes; rather, clientelism was a more diffuse social phenomenon that bound patrons and clients into ongoing, hierarchical, reciprocal exchanges in which client loyalty was secured thanks to the expectation of eventual assistance when it might become necessary in the future (cf. Chubb 1982, 167). The definition used today has shrunk so that it mainly refers to spot market exchanges on the electoral market: handouts before an election in exchange for a vote (cf. Diaz-Cayeros 2008).

Because in this chapter the focus is on unpacking and analyzing recent literature on clientelism, we can be relatively relaxed about the definition of the term. I will simply follow what studies claim it means. The only limitation I will impose in this chapter is that we attend exclusively to electoral clientelism—that is, to material handouts in the period preceding an election or specifically included as part of political campaigning. Thus, I set aside the provision of material inducements to voters that may be less directly related to electioneering, including the ongoing provision of welfare assistance and constituency service that fall under the rubric of clientelist service provision, as discussed in Golden and Nazrullaeva (2023) and Nichter (2018). Clientelist service provision is certainly important; indeed, probably more important

empirically and theoretically than most recent literature might suggest. Such activities, which occur while politicians occupy public office but are not overtly involved in an electoral campaign, raise analytical issues that I will consider at the end of this chapter.

Recent Literature on Clientelism (2003–22)

The last two decades have witnessed an explosion of studies on the topic of clientelism. As Golden and Nazrullaeva (2023) report, drawing on data from Google Scholar, the number of entries under “clientelism” increased from a total of c.5,000 between 2001 and 2011 to a total of more than 25,000 in the following decade. This fivefold increase in the number of articles and books suggests that there has likely been a commensurate increase in the number of scholars working on the topic. Thus, clientelism has become a big topic that attracts much research by a large number of people. Moreover, perhaps because the literature has proceeded at a relatively fast pace, the main questions studied quickly acquired considerable analytical focus. Although working independently, scholars explicitly debated and corrected each other in a series of loosely-connected debates and discussions.

Foundational Causal Questions

Two studies kick-started recent interest in the topic of clientelism: Wantchekon (2003) and Stokes (2005). The first introduces questions about the informational underpinnings of clientelism, whereas the second introduces questions about the enforcement of clientelist exchanges. I discuss each in turn. Because I use these two studies as starting points, the discussion treats works published after 2003, the publication date of the first of the two.

The Information Problem

Wantchekon (2003) reports results of a field experiment conducted in collaboration with four political parties in Benin. The partnership allowed random alternation of the kinds of campaign message presented to voters in twenty villages before an election. Messages were scripted to emphasize either *clientelist* or *public goods* aspects of party platforms, where clientelist messages emphasize the delivery of material goods to select voters, and public goods messages by contrast emphasize the delivery of basic public infrastructure to the community. The main finding reported in the study is that voters exhibit a preference for clientelism over programmatic promises; candidates who deliver public goods messages lose support from voters, whereas candidates delivering clientelist messages mostly maintain electoral support.

The Wantchekon (2003) study was path-breaking on a number of different dimensions. It was, as far as I know, the first research to partner with organized political parties in a collaborative effort to learn more about voter preferences and behavior; additionally, it was one of the first field experiments conducted in political science as part of the revival of experimental work emulating the growth of randomized controlled trials in economics (for a summary, see Davenport, Gerber, and Green (2010)). In addition to its methodological novelty, the paper also made a major substantive contribution by demonstrating the propensity of voters in highly clientelist settings to prefer candidates who promise individualized material goods over candidates promising community-level developmental and infrastructure improvements.¹ This result underpins the idea that clientelism (and some related aspects of corruption, particularly vote buying) form a self-reinforcing equilibrium that voters will not spontaneously overturn. Public goods promises to voters prove insufficient to shift their political support away from politicians who provide direct, individual material assistance.

This finding lays the groundwork for the first Metaketa, which studies whether providing voters with new information about political candidates reduces their support of clientelist candidates (Dunning et al. 2019). Working in seven different settings, teams of researchers implemented parallel experimental studies to try to shift political support from rent-seeking candidates to more competent and honest ones. The results of the Metaketa largely reinforce those reported by Wantchekon (2003): even new information that explicitly exposes rent-seeking and poor performance on the part of political incumbents fails to nudge voters to switch electoral support to a challenger. That is, providing information to voters about political malfeasance and candidate quality has no effect on whether voters vote or for whom they endorse electorally.

The importance of the null results reported in the Metaketa is substantial. They demonstrate that simple informational interventions aimed at individual voters are inadequate for changing voting behavior.² Other studies—most notably Ferraz and Finan (2008)—report positive results in efforts to use information to change voting behavior. So the debate is not over. Nonetheless, thanks to its unusually careful research design and meticulous analysis, the Metaketa pushes scholars away from simple informational treatments and suggests that changing voter behavior will be more difficult than imagined.

One obvious next step is to investigate whether interventions that publicly provide information allowing voters to coordinate their behavior are more successful.³ And indeed, studies that implement public (as opposed to private) interventions deliver promising results (George, Gupta, and Neggers 2018; Platas and Raffler 2021). Thus, as hypothesized more than a decade ago (Chang, Golden, and Hill 2010; Magaloni, Diaz-Cayeros, and Estévez 2007), public changes in the informational environment that facilitate *voter coordination* may produce dramatic effects on electoral behavior. When voters know that other voters will do likewise, they become more likely to move away from established clientelist options and toward new candidates and political parties. Even so, bottom-up attempts to overturn clientelist equilibria rarely succeed; possibly, the precise informational conditions along with the availability of new political entrants are difficult to achieve. A promising next step may be to find ways to manipulate experimentally the incentives facing party politicians charged with candidate selection so that they drive out incumbents mired in vote buying and clientelism and provide voters honest replacements (Asquer, Golden, and Hamel 2020).

The research agenda introduced by Wantchekon (2003) asks why voters subscribe to clientelism. It hypothesizes that voters lack complete information about the political arena and studies, whether dissemination of new information—whether by politicians or by others—can nudge voters to repudiate clientelist inducements and shift onto programmatic terrain. The information may consist of reminding voters that political parties provide public goods and not only individual clientelist inducements, or it may consist of providing new information to voters about the activities or characteristics of elected officials. The agenda produces mixed findings; on the whole, there is weak evidence in support of the informational hypothesis.

The Commitment Problem

Published only two years after that of Wantchekon (2003), Stokes (2005) introduces a subsidiary theoretical problem into the study of clientelism: if candidates promise voters individualized material goods in exchange for their votes, how are politicians sure that recipients deliver their votes in return? The commitment problem obviously arises only in the modern context of the secret ballot. It was not a relevant analytic issue under pre-modern clientelist systems when, for instance, landlords lined up their tenant farmers and gave them the ballots to use in voting or when employers registered their employees strategically (De Luca 2014). Nor was it an issue when patron–client relationships suffused the European, Latin American, and Asian countryside, locking landlords and tenants into hierarchical relationships of reciprocal obligations that pre-date suffrage extension (Baland and Robinson 2008; Mares 2015). The main question is thus: how can clientelism persist in the modern context of universal suffrage and the secret ballot, when political patrons cannot observe how clients vote?

One answer is that political party organizations (labelled “political machines”) infer voting behavior based on their on-the-ground knowledge of individuals and families in the communities in which they operate. Stokes et al. (2013) study how this is carried out by brokers, the lower-level operatives who collect information about voters and distribute the clientelist goods that secure votes.

This line of argument has been controversial. An initial attack came in Nichter (2008), which reanalyzes the observational Argentine data used in Stokes (2005) to demonstrate that political machines use material inducements to convince loyal voters to go to the polls on election day rather than to convince marginal voters to support a particular political party or candidate. Nichter’s reinterpretation of the Argentine data solves the secrecy conundrum, since whether someone goes to vote is observable, unlike how they vote. It also casts doubt on the assertion made in Stokes (2005) that political machines are capable of gathering information about how voters cast their ballots despite the nominally secret ballot.

A second line of work seeks to solve the commitment problem with a psychosocial addendum. Finan and Schechter (2012), Lawson and Greene (2014), Ravanilla, Haim, and Hicken (2022) study whether norms of reciprocity motivate voters to return handouts from political operatives with votes. Where politicians can play on norms of reciprocity, they can induce deeper voter loyalty.⁴ This research seeks to reinforce the contention that even with the secret ballot, it may be possible for politicians to reliably buy votes.

A third line of research into the commitment problem focuses on unusual contexts where violations of the secret ballot are in fact possible. At least two distinct lines of investigation have emerged. First, partial violations of the secret ballot appear to be considerably easier in small precincts (Caselli and Falco 2022; Gingerich 2013; Rueda 2014), where it is possible for brokers to interact directly with a sufficient proportion of the electorate to make a difference and where monitoring of aggregate totals may suffice to enforce compliance (cf. Catalinac, Bueno De Mesquita, and Smith 2020). Second, scholars have identified specific albeit unusual on-the-ground situations that permit direct ballot observation even when the ballot is nominally secret. Voters may be asked to photograph their ballot with their cell phone (although in some countries, photography at a polling place is illegal [Guttemann 2018]) or, despite its illegality, they may have ballots handed to them in advance of entering the polling place (Lehoucq and Molina 2002; Schaffer and Schedler 2007). These examples implicitly illustrate the obvious difficulties in organizing ballot violations on a widespread basis as well as the obstacles to perpetuating them over multiple elections. Thus, although they solve the commitment problem on an ad hoc basis, they also suggest that the commitment problem cannot usually be solved in modern democracies.

A fourth line of thought about the commitment problem asserts that the distribution of material goods to voters in the pre-electoral period is not aimed at buying votes at all but instead at creating some kind of informational signal. Muñoz (2018), for instance, argues that politicians use clientelism to purchase

participation at campaign rallies, where larger rallies signal a more viable candidate. Kramon (2018) similarly contends that the distribution of handouts in the pre-electoral period signals candidate largesse and thus, a future ability to provide benefits and services to constituents. This research takes issue with the standard idea that handouts buy votes; instead, it reinterprets the purpose of the distribution of material goods prior to an election. Under this interpretation, the commitment problem evaporates because activities that apparently resemble “exchanges” are not interpreted as aimed at getting votes in return.

Finally, there is a line of research that questions the idea that brokers who possess deep local knowledge are able to monitor and control the voters they organize, a point emphasized in Hicken and Nathan (2020). Schneider (2020), for instance, rejects the claim that brokers know the voting behavior of the constituents they monitor and Brierley and Nathan (2021) show that political parties do not select as brokers those individuals with the political and social characteristics expected. Along similar lines, Auerbach and Thachil (2018) study how voters select brokers when there are multiple possibilities, arguing that voters exercise greater autonomy in the relationship than anticipated. This line of scholarship highlights that the theory of machine politics may not solve the commitment problem, introducing the suspicion that political parties are aware it is fundamentally unresolvable in the context of the secret ballot.

The research agenda introduced by Stokes (2005) asks how politicians can enforce clientelist exchanges. It hypothesizes that they do so via street-level operatives who possess detailed information about voters. Of the various objections raised in response, perhaps the most compelling is that brokers do not and cannot solve the theoretical problem at hand. They do not necessarily know their voters, and even if they did, that would not allow them to know how ballots were cast except in highly unusual situations.

What Have We Learned?

I have thus far summarized two theoretical streams of thought arising in recent research into clientelism, which I refer to as the informational and commitment problems. I now ask what if anything we have learned from these studies.

Let us begin by clarifying what we might mean by “learning.” As proposed recently: We take a Bayesian perspective and envision a learning process by which prior beliefs about causal parameters are updated in the wake of new findings in order to form posterior beliefs ... These beliefs may be described as distributions, so that at any point in time a researcher’s beliefs may be characterized in terms of their location and dispersion. The mean of this distribution indicates what the researcher takes to be the expected value of the causal parameter; the wider the dispersion of these beliefs, the greater the researcher’s uncertainty about the causal parameter.

From this vantage point, learning is a matter of reducing uncertainty. Even if the mean of the prior and posterior distribution remains unchanged in the wake of new data, the narrowing of the distribution indicates that learning has occurred.

(Davenport, Gerber, and Green 2010, 8)

Note that in this depiction, “learning” is relevant to causal parameters and not specifically to “facts,” which consist of new data points or observations. But of course, collecting new observations may change causal parameters; this will be the case if data points included in the first set of observations were selected because they exhibit a limited range of values. So, as I suggest below, if most of what we know about clientelism comes from settings that exhibit high rates of vote buying, our understanding may shift substantially once we add cases with low rates of vote buying to the mix. As is well known from basic statistics, estimating and interpreting causal parameters can only be done on the basis of a relatively

complete dataset, one that spans the entire frequency of the underlying concept. Where data are censored or truncated, we should be skeptical of any proposed causal model.

To evaluate if the kind of learning identified by Davenport, Gerber, and Green (2010) has occurred about clientelism, we should begin with some idea of the discipline's priors. What did we know before the recent wave of research about the various analytic issues investigated by the clientelist literature? That is, was the literature already aware of the analytical questions and hypothesized answers that have absorbed scholars of clientelism in the last few decades? To consider these questions, I turn to the case study literature on clientelism and vote buying from the 1970s, 1980s, and 1990s (through 2002). Most research in this period was conducted by individual scholars using the accepted research methods of the time; namely, participant-observation and open-ended interviews in single locations.

What Did We Know Previously?

I again separate discussion into two streams: the first concerning informational issues and the second concerning the commitment problem.

Earlier Work on Informational Issues

Did earlier research identify voter ignorance—about party platforms or candidate characteristics—as possible reasons for the persistence of clientelism?

In fact, it did, although it conceived of voter ignorance in slightly different terms. In twenty-first-century social scientific scholarship, voters worldwide are generally considered rational actors with the capacity to evaluate information if it is delivered in ways appropriate to their level of literacy and cognitive skills. When working in low-literacy environments, researchers deliver information verbally, via pictographs, on video, and so forth. The researcher assumes the burden of well-designed communication, with the view that if that is delivered appropriately, voters are fully capable of absorbing and responding rationally to the information. Voters in turn are believed to operate using standards and criteria that are more or less indistinguishable worldwide.

In the 1970s, 1980s, and 1990s, social scientists working in the global South stressed that poverty, low levels of education, and recent urbanization combined to create an electorate with particularly short time horizons. This comes out nicely in the classic work of Scott (1972), which states:

Perhaps the most fundamental quality shared by the mass clientele of [political] machines is poverty ... Poverty shortens a man's [sic] time horizon and maximizes the effectiveness of short-run material inducements. Quite rationally, he is willing to accept a job, cash, or simply the promise of assistance when he needs it, in return for his vote and that of his family. Attachments to policy goals or to an ideology imply both some degree of future orientation and the presence of wide loyalties, whereas poverty discounts future gains and focuses unavoidably on the here and now.

(Scott 1972, 117–18)

In this understanding, voters are susceptible to clientelism because their discount rate is low; political parties, knowing this, emphasize immediate handouts rather than public policy. Informing voters of a party's policy commitments—or, in Scott's formulation, making ideological appeals—would not weaken the appeal of clientelism. Scott's attention to discount rates seems to have been largely neglected in contemporary interpretations of the failure of information treatments, although the literature is now beginning to incorporate issues of scarcity and time horizons (Bobonis et al. 2022, for example).

Earlier Work on the Commitment Problem

Let us turn now to the commitment problem. Was prior literature aware of the issue? Absolutely. As Chubb (1982) writes, in her detailed study of clientelism in southern Italy: “Increasingly people accept the pasta or other electoral bribes and proceed to vote as they please” (171). But, she explains:

The reason such tactics can continue to produce results, even in the absence of any dependable means for controlling whether an individual does in fact vote as he has promised, is that many among the urban poor remain so totally alienated from the political system that they see no particular reason to prefer one party or candidate over another. Given such an outlook, a kilo of pasta or 5,000 lire is as good a basis as any other for deciding how to vote. (171)

This statement shows that for earlier scholars, the commitment problem was understood but deemed theoretically innocuous. Parties and candidates that distribute material incentives prior to an election know not everyone who receives a handout will vote as hoped. Nonetheless, “vote buying” is an accepted, longstanding part of the political landscape for the urban poor and the political machines that organize them. Parties do not try to monitor how someone votes in response to a handout. Instead, they know that handouts constitute an inefficient “leaky bucket” (Dixit and Londregan 1996). They mix handouts with alternative electoral strategies aimed at social groups other than the urban poor, especially the growing middle classes (Chubb 1982). The answer to the question of how parties enforce voter commitment in exchange for handouts is simply that they do not.

What about the various responses to the broker theory of commitment that were formulated in the decades after 2003? First, had prior literature identified the manipulation of turnout as a possible reason for the pre-electoral distribution of material incentives? Turnout manipulation is key to the analysis advanced in Cox and Kousser (1981) about how parties responded to the introduction of the secret ballot in the United States. Clientelism’s relevance to the modern political system has been generally described in terms of the ability of brokers to mobilize electoral turnout (Powell 1970).

Is there evidence in prior literature that scholars were familiar with the idea that norms of reciprocity might promote successful vote buying? Auyero (2000), in a study of clientelism among the urban poor in Argentina, shows that some voters are deeply sensitive to such feelings of obligation. Earlier work by anthropologists also stressed the relevance of norms of reciprocity for clientelist exchanges (Gouldner 1977).

What about violations of the secret ballot? Had earlier studies documented these and identified their importance for clientelism? In a study of Costa Rica, Lehoucq and Molina (2002) assemble evidence regarding voter monitoring after the introduction of the secret ballot in the mid-1920s. Similarly, the debate over the introduction of the secret ballot in nineteenth-century England was replete with discussions of how material inducements as well as coercive practices might violate the integrity of the ballot:

A threat from the landlord would cause men to vote and to get others to vote as he desired, lest all be ousted; bribery could not be touched because both giver and receiver were interested; the corrupt voter would see that unless he kept faith with the bribing candidate, he would lose rewards, i.e. the market would be spoiled; rewards might be promised in case the election of the candidate should be accomplished.

(Park 1931, 55)

Thus, issues surrounding violations of the secret ballot were well known in the literature.

Fourth, was the idea known that distributing material benefits prior to an election might be aimed at providing an information signal rather than buying votes? Auyero's (2000) study of Argentina makes this argument explicitly: it documents that the distribution of goods during electioneering was aimed at enticing voters to attend campaign rallies and only secondarily at vote buying per se.

And finally, was there literature on brokers that documents their independence and even their ability to select among competing politicians? Studies of Indian politics going back to the 1950s reveal that in villages there, brokers selected among politicians and delivered their "vote banks" to the candidate whom they deemed most likely to provide the most benefits to their community (Bailey 1963). As one overview of clientelism in multiple regions around the world explains, "At the level of the village, we find competition among brokers and potential brokers for peasant votes which can be delivered to a particular political patron or potential patron. Such competition ... has been described as 'factionalism'" (Powell 1970, 416).

Thus, the ideas that are currently debated in the literature on clientelism were already visible decades earlier. Recent work documents empirically that clientelism is alive and well in many places around the world, but whether it has produced new concepts, theories, or hypotheses is less certain. Showing the persistence of clientelist practices is in itself an important enterprise, and the last few decades have produced overwhelming evidence of persistent clientelism in the global South. We may not have developed better scientific understanding of it than scholars of previous generations, however.

New Estimates of Vote Buying

Although recent literature may not have produced new theories it has, however, produced new facts—or data points—thanks to the ability of scholars to use modern instruments of data collection. In particular, the drive to quantify social science research has inspired investigators to undertake systematic data collection using surveys, and these new data collection techniques have substantially improved the nature of the descriptive information we possess about relevant behaviors. As a result, recent literature on clientelism has updated and improved available information about the frequency of vote buying in many countries in Africa, Asia, and Latin America.

Prior to around 2000, comparative politics research into the kinds of phenomena discussed in this chapter relied almost completely on unstandardized "ethnographic" research and open-ended or semi-structured interviews with brokers, politicians, and voters. Little attempt was made to quantify the frequency of the phenomena under study. With the exception of some elite surveys of politicians and (even less often) of high-level bureaucrats (for instance Aberbach, Putnam, and Rockman 1981; Suleiman 1974), scholars amassed information by talking to persons who might now be labeled "key informants" and by collecting relatively impressionistic data. Interview responses were rarely coded systematically and were often conducted with only small and unrepresentative samples. Even the most meticulous of studies—ones that clearly identified differential responses by different types of voters to material handouts prior to an election, for instance (such as Chubb 1982)—did not provide estimates of the frequency of handouts. We really had very little idea of how commonly voters were offered material inducements by candidates; the literature was exceedingly imprecise about this, suggesting merely that such inducements were "common."

In this regard, things are quite different now. Field techniques have undergone substantial modernization, as standards and methods developed in the study of political behavior in the United States have been imported into comparative politics. The increasing stress on replicability has, likewise, pressed scholars in comparative politics to adopt standardized, explicit, and replicable protocols for data collection and analysis. One result of these various pressures has been the vastly more frequent deployment of closed-ended questionnaires and surveys in the field. Sampling methods have likewise improved, as scholars have acquired a basket of methods to create data frames even in the absence of accurate census data.

Table 1 Frequency of vote buying reported in 24 countries

| Author/year | country setting | % voters reporting handouts |
|--------------------------------|----------------------|-----------------------------|
| Gonzalez-Ocantos et al. (2012) | Nicaragua | 2 |
| Nichter (2014) | Mexico | 4 |
| Rauschenbach and Katin (2019) | Ghana | 10 |
| Stokes (2005) | Argentina | 12 |
| Khemani (2015) | Philippines | 15 |
| Jensen and Justesen (2014) | 18 African countries | 18 |
| Muhtadi (2019) | Indonesia | 25–35 |
| Cornelius (2002) | Mexico | 26 |
| Cruz (2019) | Philippines | 32 |
| Çarkoğlu and Aytaç (2015) | Turkey | 35 |
| Blattman et al. (2019) | Uganda | 40 |
| Kramon (2018) | Kenya | 40 |

Source: Data all drawn from surveys that report percentage of subjects replying in the affirmative to a question about direct vote buying (e.g. “Did someone offer you something/a gift for your vote?”).

Note: The figures reported in Jensen and Justesen (2014) for 18 countries range from 1.2 percent to 40.5 percent; 18 percent is the unweighted mean.

In Table 1, I enumerate precise estimates of the frequency of vote buying gleaned from recent studies. In the table, I limit reporting estimates from studies that asked questions of voters about first-person experiences of vote buying, where the question is something along the lines of “Did someone offer you something/a gift for your vote?”⁵ I thus exclude survey data reporting how common respondents estimate vote buying to be in their country or area, or whether they know others who engaged in selling their votes. The type of question I include is likely to produce estimates of vote buying that are more reliable than asking voters for indirect information about how common they think vote buying is or asking whether receiving something from a politician influenced the voter’s vote choice.⁶

The data reported in Table 1 shows that the frequency of handouts in the pre-electoral period that is reported in surveys of voters in the developing world ranges twentyfold, from 2 to 40 percent. Indeed, the country-level numbers that make up the 18 percent average for the 18 African countries analyzed in Jensen and Justesen (2014) themselves range from a low of only 1.2 percent to a high of 40.5 percent. This is a considerable range. As the authors comment: “[T]he magnitude of vote buying varies significantly between countries, suggesting important differences in clientelist electoral practices even within democracies located in the same geographical region” (Jensen and Justesen 2014, 222). Where only 1 or 2—or even 4—percent of voters receives a material benefit aimed at buying their vote, we can safely say that vote buying is empirically trivial and unlikely to swing an election. Where, on the other hand, 40 percent of voters receive handouts, then whether those handouts influence voting behavior becomes a theoretically and empirically consequential question because vote buying may—indeed, likely *does*—influence electoral outcomes. But which is it—four or 40 percent? We have no reliable way of knowing which estimate is generalizable to unstudied contexts because we do not have strong and compelling causal theories of vote buying. Rather, we

know only that empirically, the frequency of vote buying is highly variable. In some places, vote buying is relatively infrequent and in others, it is common; without standardized data collection in countries across the globe, we do not know the distribution of values in countries or regions, or at different levels of development, or in different partisan and political contexts.

Moreover, there is good reason to believe that scholars interested in clientelism and vote buying conduct field work in countries where there are obvious displays of these phenomena and that the data we have therefore is biased toward countries with higher levels of vote buying. In Lesotho, according to Afrobarometer data reported in Jensen and Justesen (2014), only 1.2 percent of voters report vote buying, a figure not much smaller than that reported for Botswana (1.8 percent), Cape Verde (5.1 percent), South Africa (5.4 percent), and Tanzania (6.3 percent), to enumerate only some of the countries with vote buying rates lower than 10 percent. Not surprisingly, it is difficult to locate case study research of vote buying in those countries. Instead, field research has been concentrated at the other end of the spectrum, where vote buying is reported to be on the order of 30 or 40 percent of voters, including in Uganda, Indonesia, the Philippines, and Kenya. But without a complete map of the average incidence of vote buying for countries of the world, we really cannot be sure whether less developed countries tend to cluster on the Lesotho end of the spectrum or instead more often resemble Kenya.

A world with a low incidence of vote buying is a world in which other political strategies by candidates and political parties are effective in winning electoral support. Although vote buying may remain normatively troubling, if only 3 or 4 percent of voters are approached with handouts in most of the global South, perhaps our collective research resources are better directed at understanding other activities politicians engage in to win votes. If, alternatively, a third or more of voters are offered handouts, intervention in poor nations to reduce vote buying gains empirical and theoretical urgency.

One implication of this line of reasoning is that scholars conducting experimental interventions to reduce vote buying need to consider carefully—and before they begin—the likely frequency of vote buying they will encounter in the field where they plan to work. The frequency of vote buying affects the power calculations that determine sample sizes; proceeding in an absence of good data on vote-buying frequency means that sample sizes are likely to be mis-calibrated to the expected treatment effects.

There is an additional important theoretical point to be drawn from the newly uncovered descriptive data in combination with the causally oriented literature on clientelism. It is distinct from how frequent vote buying appears to be and instead directs attention to the causal role of handouts. Maybe the reason for the null results on informational intervention is that vote choice does not hinge on clientelist handouts in the first place, or for not more than a subset of voters. Instead, perhaps the broader arsenal of clientelist practices, including patronage jobs and the distribution of welfare between elections, lock voters into supporting the patrons who guarantee access to these resources.

The vast research on clientelism that has been undertaken by the foot soldiers of comparative politics provides some evidence in support of this view; for instance, if, as some research shows, politicians distribute pre-election handouts to voters indiscriminately (Kramon 2018), then handouts are not effective in vote buying. If handouts are not a good proxy for clientelism more broadly—an issue I have thus far set to one side in this chapter—then it may not be surprising that informational intervention aimed at reducing clientelist appeals fails. That is, vote choice may be a function of more deeply embedded and persistent clientelist transactions than a response to a pre-election handout. If this is the case, reducing the incidence of pre-election handouts will not disturb a clientelist equilibrium.

Conclusions

Learning new facts—collecting new data points about the frequency of important social and political phenomena—nudges us to interpret existing causal evidence differently and to reconsider our theories (cf. Heckman 2000). In political science, theoretical work and causal analysis often proceed prematurely, in contexts of incomplete and imprecise descriptive information about the social and political world. But the underlying descriptive and factual information that we possess animates the theories that are tested. Truncated or censored datasets will lead to inaccurate results when considering causal relationships.

In the example that I have considered in this chapter, I have shown that the range of frequencies of vote buying in countries around the world is much greater than previous research seems to have imagined. New and more precise estimates have arisen out of research that uses modern methods of data collection. Collecting data on the underlying frequency of what we call vote buying—the distribution of material handouts prior to an election—is just as important as testing theories of vote buying and clientelism. Indeed, our theories evolve as we learn new facts. Recent research has advanced thanks to the emergence of standardized data collection techniques and instruments that allow us to measure phenomena that are roughly comparable across sites. We know more than we used to, even if our ability to explain and improve the variation we observe may have only marginally improved.

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Notes

- 1 In the paper, the latter are considered programmatic. Whether this is a fully defensible conceptual formulation is not entirely clear; for a discussion, see Golden and Nazrullaeva (2023).
- 2 Some have extended this idea to argue that bottom-up informational interventions aimed at voters may generally be ineffective (Raffler, Posner, and Parkerson 2022).
- 3 On the difference between the public and private provision of information, see Chwe (2013) and for a related theory of social norms, Bicchieri and Xiao (2009).
- 4 The idea that voters observe a norm of reciprocity naturally raises the question of how this norm is sustained and operates over time. If reciprocity were key, interventions by NGOs that provide information to voters that their votes are not monitored should find no effect. Studies that provide this specific information, however, tend to be effective in reducing clientelism (Arias et al. 2019; Blattman et al. 2019; Vasudevan 2019; Vicente 2014). Voter education against vote buying successfully convinces citizens to vote with their conscience. Thus, norms of reciprocity are easily disrupted and appear inadequate to sustain large-scale and on-going vote buying. Note that these results rely on experimental data collection.
- 5 For instance, the Afrobarometer question whose results are analyzed in Jensen and Justesen (2014) is: “[D]uring the [20xx] election, 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?”

- 6 Comparing the results of list experiments with direct questions, Blair, Coppock and Moor (2020) shows that in general, voters moderately underreport vote buying in direct questioning, although there is considerable variation across settings. The figures in Table 1 may thus underestimate the frequency of vote buying; the underestimates are likely to vary between countries, however, making the data correspondingly more difficult to interpret.