Distributive Politics Around the World

Miriam Golden[1] and Brian Min[2]

[1]Department of Political Science, University of California, Los Angeles, California 90095; email: golden@ucla.edu

[2]Department of Political Science, University of Michigan, Ann Arbor, Michigan 48109; email: brianmin@umich.edu

Keywords

patronage, pork-barrel, political capture, redistribution, accountability

Abstract

We inventory more than 150 studies of distributive politics in more than three dozen countries other than the United States. We organize existing studies under two theories: theories of democratic accountability and theories of government responsiveness. Studies that concern democratic accountability conceptualize distributive allocations as attempts by politicians to protect themselves electorally by targeting specific groups of voters. We identify four subsets: (*a*) studies of whether politicians target goods to core or swing voters; (*b*) studies of general political favoritism in targeting government goods; (*c*) studies of whether goods are disbursed according to the electoral cycle; and (*d*) studies of whether elected officials gain votes from the disbursement of government goods. We illustrate each with examples from the literature. We then discuss distributive politics as responsiveness to the median voter. This perspective entails a focus on the redistributive consequences of government policy and investigates whether special interests capture a disproportionate share of goods coming from government. To illustrate the utility of both perspectives, we analyze original data on electricity provision in India's largest state.

THE PROBLEM OF DISTRIBUTIVE POLITICS

If, as Harold Lasswell claimed, politics is all about "who gets what, when, how" (Lasswell 1936), then distributive politics, which involves allocations of government goods and services, lies at the heart of politics. Yet for many years research into the distribution of goods and services by governments was confined almost exclusively to the United States. Scholars of comparative politics barely attended to these phenomena. In the past decade, in part because of the increasing availability of relevant data for countries other than the United States and in part because of its theoretical fecundity, this once almost invisible topic now ranks among the liveliest for students of comparative politics. For this review, we have constructed an inventory of more than 150 studies of distributive politics that have been carried out in more than three dozen countries other than the United States. We use this inventory to classify how studies of distributive politics have been framed theoretically and analytically. Our meta-analysis identifies few systematic results because existing studies do not all seek to answer the same questions and because empirical requirements for testing competing theories are often not met, leading to results that cannot be easily interpreted. We discuss how this could be improved.

A few words of conceptual clarification are necessary at the outset. We understand distributive policies as those that involve taxes and transfers, and in particular the decisions about allocations of government goods and services to identifiable localities or groups. A standard way to think of this is how public authorities confer geographically concentrated benefits while diffusing costs across voters (Weingast et al. 1981). Most studies of distributive politics focus on electorally relevant aspects of taxes and transfers. These studies fall within what we call the accountability perspective. However, taxes and transfers also carry redistributive outcomes. Attention to how outcomes affect equity and inequality invokes what we call the responsiveness perspective. A complete understanding of taxes and transfers involves their politically consequential aspects as well as their redistributive consequences. Both the accountability perspective and the responsiveness perspective derive from well-developed theoretical structures based on micro foundations, and each set of studies grapples with one of two identifiable problems in democratic theory (Manin et al. 1999).

The fundamental theoretical concern of studies involving political accountability is the ability of voters to hold political representatives accountable for government performance. Within this broad category, we distinguish four specific classes of works. The first asks whether politicians allocate goods to their core constituents or whether they target swing voters; this is arguably the theoretically most precise issue that structures studies of distributive politics. Another related set of studies concerns political favoritism more broadly and asks whether politicians allocate disproportionately to population subgroups, variably identifiable by race, ethnicity, or partisanship. A third set of studies concerns political business cycles. These papers investigate the timing of the delivery of allocations in relation to the electoral cycle. A final group of studies examines electoral returns to government allocations; that is, whether politicians receive more votes by directing goods to electorally critical districts or to specific groups of voters. Underlying each set of studies is the assumption that the goal of politicians in allocating resources is exquisitely electoral, highly partisan, and ultimately a function of attempts at political survival.

A second way to think about distributive politics is as policy responsiveness by elected leaders to voter preferences. Operationally, this perspective involves investigating the welfare and redistributive consequences of patterns of allocation. Papers in this category investigate the benefits to voters rather than the electoral returns to politicians from allocations. They ask if the allocations that occur are welfare maximizing, equitable, or, to put it more concretely, preferred by the median voter; these are different ways of conceptualizing a baseline expectation for public allocations. If allocations do not accord with welfare maximization, then political capture by special interests,

such as industry lobbies, is inferred to exist. Studies are fundamentally interested in whether policy is responsive to voters rather than whether politicians are accountable to the electorate. [This distinction is highlighted by Przeworski et al. (1999); for a recent review, see Ashworth (2012).]

We contend that the responsiveness perspective is especially important at this phase in the evolution of the research agenda of distributive politics and that this perspective should be incorporated explicitly into studies of distributive politics. There are at least three reasons behind our view. First, studies of the welfare-maximizing impact of allocations are especially relevant in economically less developed societies, where much contemporary research is conducted. Improving the welfare of ordinary citizens in low-income societies has both normative and developmental importance. Second, data requirements are more easily met in the responsiveness approach, whereas further progress in the area of accountability likely depends on creating bespoke micro-level survey data. Although this is not impossible, it is expensive and time-consuming, and results may exhibit limited external validity. Finally, studying accountability before responsiveness is like putting the cart before the horse. If allocations are welfare maximizing, then the political conflicts that lie behind them are spurious, incidental, or irrelevant. We extend these observations in this review.

In the next two sections we introduce our inventory, clarify some concepts relevant to the study of distributive politics, and review institutional studies, which attempt to distinguish programmatic from nonprogrammatic (i.e., pork-barrel) allocations. A third section examines the four subtypes of studies that we classify under the theory of democratic accountability: (*a*) studies whose research design is intended to adjudicate the swing versus core voter debate, (*b*) studies concerned with political favoritism to culturally constructed groups in the population, (*c*) studies designed to investigate political business cycles, and (*d*) studies of the political returns to allocations. A fourth section presents the theory of democratic responsiveness. We discuss redistribution and political capture here. To illustrate the utility of the accountability and responsiveness perspectives, in a fifth section we analyze original data on the politics of electricity distribution in India's largest state, Uttar Pradesh. Finally, we offer some conclusions, focusing on how future research can integrate the accountability and responsiveness perspectives.

AN INVENTORY OF STUDIES ON DISTRIBUTIVE POLITICS

The material we draw on for this review comprises as thorough an inventory as possible of studies of distributive politics in countries other than the United States.[1] Studies of distributive politics have almost all been undertaken in democratic political systems (one recent exception is Blaydes 2011). Given the absence of reliable, systematic data on the allocation of government goods and services in nondemocratic regimes, this is not surprising. Yet research has identified some regularities in the patterns of government distributions distinguishing democratic from nondemocratic political systems. The allocation of goods and services across the national territory is more equitable in democracies than nondemocracies. Democracies provide more basic education (Stasavage 2005), improve infant mortality rates (Kudamatsu 2012), allow broader access to electricity (Min 2010, ch. 3), and redistribute a larger portion of the surplus than nondemocracies, whose leaders instead extract greater rents (Lake & Baum 2001). Overall, although we have reason to suspect that distributive politics is quantitatively more important in democratic than authoritarian regimes, we have little knowledge of the criteria used by unelected political leaders in the distribution of goods

---

[1]The inventory and related bibliography are available by following the **Supplemental Material link** from the Annual Reviews home page at **http://www.annualreviews.org/**.

and services. As a result, the analysis in this review focuses only on democratic political systems. We do not know how it would be modified were we able to incorporate nondemocracies.

CONCEPTS, TERMINOLOGY, AND INSTITUTIONAL VIEWS

The main reason most work on distributive politics is undertaken within single countries is because of difficulties in aligning data and concepts across countries. The same data—government spending on road construction, for instance, or the number of civil service jobs—cannot be interpreted as indicators of the same underlying concepts in different contexts. To illustrate this, we consider cross-national work on the distribution of programmatic versus pork-barrel goods, work that attempts to identify institutional determinants of policy differences.

Drawing on theoretical work by Lizzeri & Persico (2001), Milesi-Ferretti et al. (2002) hypothesize that where legislators are elected under proportional representation (PR), they will engage in programmatic politics, whereas their counterparts elected in single-member districts (SMDs) will focus instead on "pork" and patronage. Programmatic goods are those allocated to large classes of citizens regardless of their geographic location; by contrast, goods allocated to specific localities and to correspondingly small constituencies are pork. For instance, Milesi-Ferretti et al. classify pensions as programmatic goods and the construction of bridges, highways, and other public capital goods as pork. Their logic is that in PR, legislators are elected on a party list and are therefore not individually identifiable to voters, whereas in SMDs, individual candidates must distinguish themselves from their competitors. Where candidates seek such a "personal vote" (Cain et al. 1987, Carey & Shugart 1995), they will distribute pork and patronage to specific constituencies in order to increase their visibility among voters, thereby enhancing their prospects for reelection. Milesi-Ferretti et al. contend that, as a result, patterns of public spending tend to favor visible localizable goods (such as public construction) in SMD systems and programmatic goods (such as broad welfare policies) under PR.

Although plausible, this argument is difficult to test because of the operational difficulties involved in classifying allocations as programmatic goods or pork and patronage. Examining government spending data, for instance, produces information on how much is spent on social security, on capital construction, on government salaries, and on emergency relief, among other categories. Which represent programmatic goods and which pork and patronage? Providing government jobs is political patronage if recipients are selected on the basis of partisan identity but is programmatic if job creation falls under a Keynesian macroeconomic strategy. Accurately classifying spending requires information about program details, but information may be unavailable or may require disputable judgment calls and deep on-the-ground knowledge of the context. Classifying spending in theoretically appropriate ways in multiple countries is also fraught with difficulties. This has encouraged scholars to work within single countries, where an accurate understanding of policy design and implementation is more easily achieved.

Even given identical policy design, the impact of government allocations differs in different developmental settings. For instance, in a wealthy country that enjoys adequate transportation networks, construction of a new road may mainly benefit the locality during construction. Construction companies and individuals hired for construction jobs experience direct benefits, but the local community may be relatively indifferent to the new road. In this setting, road construction is typically considered pork, and arguably rightly so. But in a low-income country, in addition to the benefits that accrue to the individuals involved in construction, the entire community may enjoy benefits over many years from a paved road where none previously existed. If women once had to walk half a day to get their products to market or to obtain clean water, and now they may travel by cart or bike in a fraction of the time, construction projects do not look like patronage

or pork. Instead, they feed economic development, improve the quality of life for citizens, and indicate government commitments to ordinary villagers rather than urban elites. In this context, it seems patently inappropriate and misleading to classify road construction as pork. The policies that underpin infrastructure construction in low-income nations may be broadly conceived around developmental goals, thereby requiring large-scale fiscal transfers and investments that belie the standard "concentrated benefits and diffuse interests" definition of pork-barrel politics that is used in developed nations. This example illustrates why we doubt that it makes sense theoretically to classify expenditures in identical ways across wealthy and low-income countries. Moreover, as we discuss in more detail in the section on government responsiveness, we need to know what constitutes a welfare-maximizing allocation to calibrate politicized deviations.

We now provide some additional clarification of the terms used in the literature we review. Stokes et al. (2011, p. 36) define pork-barrel goods as nonprogrammatic allocations not targeted at individuals (hence, typically local public goods, in the sense of locally nonexcludable and nonrival) but decided on the basis of partisanship; they define nonprogrammatic, in turn, as allocations that occur on the basis of criteria that are not rendered fully public. To a large extent, the latter corresponds to the distinction between formulaic and discretionary criteria, terminology emphasized by Diaz-Cayeros et al. (2012). Stokes et al. (2011) define patronage goods as the contingent allocation of a nonprogrammatic material benefit for a vote; Hicken (2011) uses the term clientelism to refer to the same bundle of activities. Although the concept of a contingent exchange raises issues of monitoring and commitment that we do not pursue here, we preserve the idea that allocations that involve political patronage seek to create interests and obligations that reinforce or compel partisan loyalty on the part of recipients. Broadly, then, pork-barrel goods are local public goods, whereas patronage, or clientelistic, goods are not; the latter, in addition, target individuals with the aim of creating partisan or political loyalty that will endure over time. An example is a government job, especially if keeping it would be jeopardized if the governing party that awarded it were removed from office; even an appointment ostensibly made using meritocratic civil service criteria could be terminated if the entire job were abolished. Examples of pork-barrel goods include (some) discretionary allocations of capital construction, particularly those made out of the public eye with partisan goals.

As these definitions suggest, using these concepts accurately hinges on considerable local knowledge and scholarly judgment. What qualifies as pork in one setting may not in another. A considerable subnational literature has developed that extends insights drawn largely from Milesi-Ferretti et al. about how political institutions, especially electoral and party systems, affect programmatic versus pork-barrel goods within countries. Ames (1995), for instance, details how Brazil's open-list system of PR and executive dominance favors pork over programmatic policies. Chhibber & Nooruddin (2004) find that patronage policies, which they measure as the size of the public bureaucracy, are larger in Indian states with multiparty systems than in those with two-party systems and that public goods are better provided in the latter. Stratmann & Baur (2002) use an inventive research design to show that German legislators elected from party lists choose to serve on parliamentary committees with programmatic commitments, whereas those elected from SMDs choose committees that are able to deliver pork. These findings are all in line with what theory would lead us to expect about the impact of institutions on the type of policies selected. But most literature on distributive politics is not institutional in inspiration. We now turn to works rooted in theories of democratic accountability.

PROBLEMS OF DEMOCRATIC ACCOUNTABILITY

The theory of democratic accountability assumes that politicians are motivated by the desire to retain public office. It is this desire that enforces accountability: elections discipline incumbents

by threatening loss of office for inadequate performance. Rigorous exploration of the relevant issues began with Ferejohn (1986), and over the past 25 years a well-consolidated formal literature, recently reviewed by Dewan & Shepsle (2011), has developed whose micro foundations underpin our understanding of issues of accountability. Most empirical studies of distributive politics conducted in countries other than the United States share the assumptions that politicians are office seeking and that voters will penalize them with loss of office for inadequate performance.

Given the desire to retain office, the literature on distributive politics examines the success of elected officials in allocating specific types of goods to specific groups of constituents in electoral districts at specific times in the electoral cycle. These are usually analyzed separately: some studies focus on types of constituents (core and swing; voters and nonvoters; identifiable subgroups that share culturally constructed characteristics with the allocative agents); some on the timing of delivery (political business cycles); and some on the electoral returns to these various activities. Some studies tackle more than one of these problems, although in our inventory, we have classified each study according to what we determined comprised its most important contribution. Regardless of the research design and dependent variable investigated, all these studies share the presupposition that politicians are office seeking, and all analyze distributive politics as reflecting electoral concerns. Asking why we observe specific patterns of distribution, these studies (almost) all answer, "Politics!" We thus group all these studies under the theory of democratic accountability. The modal study of this sort seeks to document how vote seeking distorts the allocation of government goods and services.

Who Receives Allocations: The Core and Swing Voter Debate

The basic formal model for studying which types of voters receive allocations is due to Dixit & Londregan (1996), building on Lindbeck & Weibull (1987) and Cox & McCubbins (1986). In this model, politicians (modeled as two spatially arrayed political parties, one on the left and one on the right) make allocations across groups of voters within a single electoral district. Voters have fixed and exogenously given ideological (or what might be called partisan) preferences and also receive utility from whatever allocations they receive. A voter will prefer the party further from her partisan ideal point if the party offers her a transfer that is large enough to outweigh her ideological attachment to her own party. In this formulation, votes can be "bought" with distributive allocations; that is, material inducements shift voters from the party to which they are loyal (due to shared programmatic commitments) to the rival party.

The Dixit & Londregan model has some interesting implications. First, voters with strong partisan attachments require larger transfers than voters with weak partisan attachments to vote for the other party. Hence, parties compete for political moderates, also known as swing voters, because they are more cheaply purchased with transfers than voters whose partisan attachments are stronger. In addition, poorer voters require smaller per capita transfers to shift their votes than richer voters because the former receive higher utility from the additional income wrapped up in the transfer.

In the Dixit & Londregan model, parties usually engage in symmetrical strategies in which both target swing voters. However, the authors argue that in some situations, parties allocate goods to core voters: those with strong partisan preferences. "Machine politics," as this is called by Dixit & Londregan, occurs when a party has a substantial informational advantage about its constituents and is able to precisely target goods to specific individuals. In cases where a party operative knows who has just lost a job, who is recently bereaved and needs funeral money, or whose home has just been destroyed by fire and needs lodging, the party can allocate jobs, burial services, and housing to individual constituents. The rationale for allocating goods to one's core constituents

is that the transfers can be precisely targeted and there is effectively no waste. Giving such goods to swing voters, about whom the politician necessarily has little individualized information, is less efficient because not all swing voters who receive goods will switch their votes. Some unemployed voters have such strong partisan attachments that a job is not enough to induce the switch, but the politician handing out the jobs does not know ex ante for which voters this is the case (or, put another way, which swing voters are closer to being core constituents of the other party). When politicians have enough information about voters that they can target transfers with few deadweight losses, the Dixit & Londregan model produces the same results as an earlier analysis of transfers to core voters by Cox & McCubbins (1986).

This fertile model offers a simple and intuitive framework for thinking about problems of distributive allocations, targeting, and accountability.[2] It provides the main theoretical micro-foundations for studies of distributive politics. Many articles draw on it to investigate whether core or swing voters receive greater allocations by politicians. For reasons of space, we review only a selection of studies and refer readers again to the complete inventory that is available through the **Supplemental Material link** at the Annual Reviews home page (**http://www. annualreviews.org**).

Results in this line of research are usually interpreted as favoring the swing voter hypothesis. However, this interpretation is weakened by misalignment between key concepts and the level at which data are available. The concepts of "core" and "swing" voters concern individuals. With only a few exceptions (see in particular Stokes 2005, Stokes et al. 2011, and the discussion below), the available data refer to electoral units rather than individual voters. Unless scholars generate data by administering their own surveys to individual voters, available data are aggregated to electoral units and thus show that parties tend to distribute resources to swing *constituencies*, not swing voters. Cox (2010, p. 346) makes the point this way:

> In a multidistrict context, the core voter thesis would be that parties concentrate benefits on their core voters within whatever districts they target, swing or otherwise. For example, the Republicans certainly targeted resources toward the swing state of Ohio in the presidential campaign of 2004. However, by all accounts, they pushed those resources more into mobilizing their base than into persuading swing voters. Thus the targeting of Ohio in no way provides support for the swing voter thesis; it provides support for the quite different swing district thesis.

An example of the data predicament is provided by Dahlberg & Johansson (2002), who use survey data to identify core and swing voters. They examine 1998 disbursements by the Swedish government to 42 out of 115 municipal applicants (of a total pool of 288 municipalities) for discretionary intergovernmental grants. Survey data are used to construct estimates of the distribution of ideological preferences of each municipality's electorate. Dahlberg & Johansson use these to construct estimates of the critical value, or cutpoint, dividing supporters of the incumbent party (the socialists) from other (conservative) voters. The idea is that the incumbent party attempts to shift the cutpoint with transfers, moving some number of voters weakly predisposed toward the conservative bloc to vote for the socialist bloc instead. The cutpoint is empirically similar, under some assumptions, to the closeness of the last election, which the authors also investigate. The authors interpret their results as corroborating the hypothesis that the allocations studied are directed disproportionately to municipalities with more swing voters.

Despite their use of opinion data and the painstaking research design, Dahlberg & Johansson do not provide a precise test of the core versus swing voter theory because their research operates

---

[2]Dixit & Londregan use the term tactical redistribution for what we call distributive allocations.

at the aggregate municipal level. Their results cannot tell us whether more or fewer partisan voters received the allocations studied, only whether the localities receiving allocations had more swing voters. This cannot rule out that within municipalities receiving allocations, all the benefits were directed at or grabbed by core voters, i.e., those already strongly predisposed to support the incumbent government. There is simply no way to know this given the data.

An attempt to improve empirically on this dilemma by analyzing independently generated survey data while also repositioning the Dixit & Londregan theory as a dynamic model comes from a series of studies by Susan Stokes (see especially Stokes 2005 and Stokes et al. 2011). Her empirical focus is Argentina, a setting in which there is evidence of machine politics. Echoing Dixit & Londregan's stress on information, Stokes argues that in Argentina, parties have the capacity to target individual voters with patronage goods thanks to specific information about individual needs. Stokes also contends that the organizational structure that permits political machines to know what goods to direct at which voters also allows the party to gain information on the vote itself; as a result, voters often believe that the vote is not fully secret. Even with a nominally secret ballot, Stokes claims that political machines have methods to monitor the voting behavior of the individuals to whom they distribute patronage in order to ensure that voters do not renege on the commitment implied by accepting the patronage good. Extending the Dixit & Londregan model to a dynamic setting, Stokes thus also incorporates into the model some probability that the vote is observed by the party. The main prediction of her model is that party machines distribute patronage to voters who can be bought; voters, that is, who are ideologically weakly opposed to the machine party and for whom the value of the patronage good effects a shift to the party offering it. This theoretical result differs from that of Dixit & Londregan, who instead claim that party machines target loyal voters with patronage. The Stokes model predicts that loyal voters will not receive patronage goods for the intuitively obvious reason that they have nowhere else to go ideologically.[3]

Collecting survey data from nearly 2,000 voters in three Argentine provinces specifically for her research allows Stokes to discriminate empirically in an unusually precise fashion between types of voters to see whether the Peronist party targets core or swing voters with patronage goods. Her analysis corroborates that the Peronist party in Argentina fails to distribute patronage to its most loyal supporters. But although Stokes finds that the party distributes patronage to those in the middle of the distribution of partisan preferences, her results fail to corroborate that those weakly opposed to the Peronist party are specifically favored with goods. Her results thus do not accord fully with her theory that patronage goods are aimed at buying votes from those who otherwise would vote for another party.

The Stokes (2005) article advances analysis in at least two ways. First, it extends the Dixit & Londregan model to a dynamic setting, modeling the interaction between parties and voters over multiple elections. This is not only more intuitively reasonable but naturally raises the commitment problem: if a party provides a patronage good to a voter, what induces the voter to maintain her side of the bargain and vote for the party? The original model skirts this important issue. Second, Stokes uses survey data that generate individual-level measures of ideology to test more precisely than in other studies whether parties target core or swing voters with patronage. However, Stokes' own results support her theory only in part.

One explanation for why Stokes' results fail to line up with her hypothesis about weakly opposed voters receiving patronage is offered by Nichter (2008). In contrast to Stokes, Nichter doubts that political parties that distribute patronage goods have much ability to monitor the vote in a secret

---

[3]The result hinges on the assumption that voters all vote, and no allowance is made for abstaining.

ballot context. He instead reelaborates Stokes' model to incorporate the decision by citizens to vote or abstain. The modified model predicts that a party targets its strongest supporters with goods in order to induce them to go to the polls. Nichter then reanalyzes Stokes' data to document that the patterns of distribution of patronage goods in Argentina are more consistent with his theory of turnout buying than with a theory of vote buying.

This introduces a new consideration into the debate about core versus swing voters. If voters do not have the option to abstain, then the standard theory, as elaborated by Stokes (2005), predicts that parties target distributive goods to swing voters in order to buy votes from those who are not ideologically loyal but also not very ideologically distant. If voters have the option to abstain, theory predicts conversely that parties target loyal voters in order to induce them to vote. Nichter's reanalysis of Stokes' data corroborates the latter, although this result needs to be replicated in other settings and using other data. However, without carefully crafted voter surveys or other appropriately designed methods, it is not possible to know if goods are intended to shift votes or to induce turnout. Future work is likely to center on this problem.

In contrast with this line of argument about what now has become the vote-buying versus turnout-buying debate, Vaishnav & Sircar (2010) argue in favor of approximating the concepts of core and swing by simply using the margin of victory in the electoral district. Recall that Dahlberg & Johansson (2002) had found this often functioned equivalently to survey data. Vaishnav & Sircar (2010) contend that a research design using margin of victory generates results that can be interpreted in terms of electoral pressures on politicians emanating from voters in each district while avoiding the requirement to collect individual-level data. Using a similar logic, Ward & John (1999) study whether electorally marginal constituencies in England received larger central government grants and report that those held by the party of the opposition did. The authors interpret this as evidence of attempts by the party of government to swing marginal constituencies away from the opposition. A similar research design is utilized by Arulampalam et al. (2009) in a study of transfers to Indian states by the central government. In contrast to the English results, in India it appears that the central government targets grants to states aligned with the center. All these studies shift the focus from individual swing voters to swing constituencies, a research design that is easier to operationalize and does not require any special data-collection effort.

This change eviscerates the voter-centered formal model of Dixit & Londregan that inspired the original debate. A constituency-level research design allows evaluation of whether the incumbent party rewards marginal constituencies, meaning constituencies where the party's margin of electoral victory is small and where the party is thus more likely to lose the seat, or even, as in the paper by Ward & John (1999), rewards marginal opposition constituencies. The strength of this way of studying the problem is that the data are easily available, consisting essentially in voter returns at the level of electoral constituencies. The weakness, however, is that results accord poorly with the individual-level theory that is usually held to be relevant. At the very least, scholars need to make this explicit.

In addition, a constituency-level research design is appropriate to SMD settings characterized by political parties that make decisions nationally about distributing resources across electoral districts (Stokes et al. 2011). Because swing constituencies as such do not exist in multimember settings, this approach is not useful there. In countries that use multimember electoral districts, it is difficult to study whether allocations go to an incumbent party's swing or core areas because all electoral areas are typically represented by multiple parties. Only in empirically unusual circumstances, such as where entire geographic regions of a country are core for an incumbent party (analyzed, for instance, by Golden & Picci 2008) or where highly localized "bailiwicks" associated with individual politicians can be identified and mapped onto the distribution of goods or services

(see Crisp & Desposato 2004), is it feasible to determine whether a party allocates goods to core or marginal areas in multimember settings.

Given these types of problems in aligning theory and operationalization, it is not surprising to find some scholars moving away from the concepts of core and swing altogether. Baldwin (2005), for instance, who studies the allocation of government jobs in newly democratic Mali after 1992, argues that the pattern of distribution found in her data likely reflects the political decision by the executive to distribute jobs to areas that harbor potentially destabilizing opposition activities. This fits neither the core nor the swing hypothesis, nor does it speak to the vote-buying versus turnout-buying debate. Rather, it evokes the literature on patrimonialism and on the cooptation of political rivals by regimes that are not (or are not fully) democratic, originating in Bates (1981). Similarly, Toha (2009) reports that in post-Soeharto Indonesia, disproportionate spending on public goods goes to districts that credibly threaten secession. This finding echoes Treisman (1996), who shows disproportionate allocations to regions in post-Soviet Russia that threatened civil unrest and disorder. These findings also suggest that the further research moves away from the stable institutional environments of Western Europe and North America, the more we may identify unexpected patterns of allocations by political elites as they grapple with the turbulent political world of postauthoritarianism.

Cultural, Ethnic, and Partisan Favoritism

Many studies of the provision of government goods and services take a theoretically more relaxed view than the swing versus core voter debate. The main question in this line of work is whether political or ethnic favoritism affects the allocation of goods and services. Like Baldwin (2005), these studies often draw on preexisting literatures on patronage politics, patrimonialism, clientelism, and other forms of nonprogrammatic "linkages" between political parties and voters (Kitschelt & Wilkinson 2007). Scholars seek to ascertain systematic determinants of the differential distribution of goods and services, drawing their hypotheses about which particular social groups are likely to be favored mainly from prior knowledge of the particular setting. A wide range of empirically relevant factors emerges out of this effort, including history, culture and religion, caste and ethnic affiliation, partisanship and political power, and features of government institutions.

Cadot et al. (2006) investigate infrastructure investments in the transportation sector in French regions between 1985 and 1992. The authors study whether regions with large firms and more partisan competition are associated with more new investments for public transportation. The reasoning is that large firms should be interested in improving regional transportation because they draw on large pools of labor. The authors contend that these firms are likely to lobby government for more investments, and that although lobbying efforts in France are not directly observable, lobbying is the mechanism that underlies any significant association observed between the extent of large firms and improvements in transportation infrastructure. The second main explanatory variable that Cadot et al. forward is the closeness of the vote in the region between the left and the right in recent elections. Their argument is that where the vote is closer, politicians have greater electoral incentives to improve infrastructure. The authors refer to the investments made in response to lobbying and electoral incentives as "pork barrel," and offer the following interpretation of their findings: "roads and railways are not built to reduce traffic jams: they are built essentially to get politicians reelected" (Cadot et al. 2006, p. 1,151).

In less-developed settings, socially constructed identities such as ethnic or religious groups (rather than organized economic interest groups) are typically the focus of studies of favoritism in distributive allocations. Drawing on work by Bates (1974, 1981), many studies of countries in Africa identify tribal affiliation or ethnicity as an important determinant of allocations. Similarly,

work on India finds that state governments controlled by political parties representing lower-caste voters direct larger fractions of government spending to services that will specifically benefit these voters (Teitelbaum & Thachil 2010) and that politically mobilized lower-caste groups gain more than their politically quiescent counterparts (Banerjee & Somanathan 2007).

A theoretically illuminating example is provided by Posner & Kramon (2011). The authors study five indicators of government goods and services (infant mortality rates, childhood vaccinations, educational attainments, access to improved water sources, and household electrification) in Kenya, Malawi, and Zambia to assess whether patterns of ethnic favoritism are consistent across types of goods. Their results show that they are not. Posner & Kramon argue that governments make allocative decisions for a large number of goods and services more or less simultaneously; if one group is favored with health care, another may be favored instead with electricity. Hence, favoritism toward one group with one good can be offset by favoritism toward another with an alternate good.

The Posner & Kramon study highlights the problem of external validity. Their findings document that scholars' interpretations overgeneralize patterns of political favoritism based on the study of a single good. A study of household electricity that uses data from Kenya would report no ethnic favoritism, whereas a study of electricity in Zambia would; a study of primary education in Zambia would show no ethnic favoritism, but a study of primary education in Malawi would. In a similar but broader vein, Kasara (2007) refutes conventional wisdom about African ethnic favoritism in a study of the taxation of agriculture using data from 30 nations. Her analysis shows that cash-crop farmers who share an ethnic identification with the head of state pay higher taxes rather than, as commonly believed, being favored by government. Kasara's results hinge on the relatively large sample size that she uses, an improvement over prior studies that used only one or a few countries. Her results, like Posner & Kramon's (2011), show that interpretations based on a single country, a small number of countries, and/or a single or limited number of goods are often unrepresentative of larger patterns. The challenge for future research is thus to maintain data precision while simultaneously expanding the number of cases analyzed.

When Allocations Occur: Political Business Cycles

A third set of studies that concerns political accountability examines the timing of allocations to determine their association with the electoral cycle. The main question is whether politicians, seeking to improve their chances of winning a forthcoming election, deliberately allocate goods and services just prior to the election. There is a large cross-national literature on this that derives from the work of Tufte (1980) and, in economics, Nordhaus (1975). The main outcomes studied are those associated with macroeconomic fiscal and monetary policies. At the cross-national level, results have been inconsistent, perhaps because of the mixed effects of electoral competition on the macroeconomy.[4] In less-developed countries, by contrast, studies at the national level have found stronger and more consistent evidence of political business cycles (Shi & Svensson 2006), which are interpreted as stemming from the greater degree of executive discretion that typically characterizes such countries compared with established wealthy democracies.

---

[4]Canes-Wrone & Park (2012) contend that private spending on capital construction and other long-term investments is likely to be depressed prior to a national election, especially if the election is close and political parties are polarized, as private investors hold back on irreversible investments in a period of political uncertainty. With this in mind, Canes-Wrone & Park (2013) produce results for US national elections that are much more consistent with the political business cycle model than prior studies.

At the subnational level, relevant studies examine whether political authorities make allocations consistent with the theory of political business cycles, increasing allocations in periods proximate to elections. An intriguing variety of goods and outcomes has been examined. Cole (2009) finds that agricultural credit to Indian states, allocated by government-owned banks, increases in election years. Golden & Min (2012) report that agriculture receives more electricity, essential for crop irrigation, in election years in India's largest state, Uttar Pradesh. Ferraz (2006) finds that political favoritism in the enforcement of environmental regulations in Brazilian cities is more evident in election years and when the incumbent mayor faces stiffer electoral competition for reelection. These outcomes—agricultural credit, electricity, and regulatory enforcement—are not usually thought of as targetable goods that political officials can selectively allocate, but the studies just cited show that in some settings, these can be politically manipulated. Using more conventional outcomes, Drazen & Eslava (2005) report that pre-election expansions of investment spending and government jobs occur in Colombian municipalities; likewise, in South Korea, Kwon (2005) reports expansions of government spending to provinces in election years, and also that the expansions are concentrated in the more electorally competitive provinces.

These studies show that at the subnational level it is possible to study unusual distributive outcomes that are related to government decision making but that cannot be easily or appropriately researched cross-nationally. In economically developed countries, the politics of agricultural credit or electricity use are substantially different than in low-income nations. It is not analytically useful to study outcomes such as these in a global sample of nations because these outcomes are relevant almost exclusively to less-developed settings. Even if the sample were limited to countries whose level of economic development made these goods politically pertinent to large numbers of voters, it might not be possible to identify electoral cycle effects across time at the national level. The reason is that national-level analysis would fold together both very urban and very rural electoral constituencies, even though the electoral significance of these goods should be visible mainly in rural constituencies. One value of subnational work, therefore, is that it allows investigation of the allocation of goods and services to specific social groups (farmers, for instance) that may be geographically concentrated.

The literature discussed in this section identifies interesting examples of politically manipulatable electoral cycles to distributions. Estimating the frequency of these phenomena in different localities remains a challenge that has hardly been tackled yet.

The Electoral and Political Returns of Allocations

A final set of studies within the political accountability framework examines whether political incumbents are rewarded by voters for distributive allocations. If the intention of political authorities in making allocations is to improve their own electoral fortunes, these studies assess whether their effort is successful. This is a difficult question to study because doing so requires assumptions about how voters would have behaved in the absence of distributive allocations. One typical research design compares voter behavior in electoral constituencies that receive more allocations with voter behavior in constituencies that receive fewer allocations. By including relevant control variables, such as population density and ethnicity, analysts claim to capture the counterfactual conditions.

Studies overwhelmingly find that incumbent politicians are rewarded by voters for distributive allocations, and in particular for those that are clientelistic and from which recipients can be excluded. A study of the differential electoral returns to excludable and nonexcludable allocations in Venezuela reports that only the former type generates votes, and that the electoral returns are greater when electoral competition is most intense (Ortega & Penfold-Becerra 2008). A study of conditional cash transfers in Mexico reports that enrollment in the transfer program generates

a substantial increase in incumbent vote share (De La O 2008), and a study of antipoverty cash transfers in Uruguay likewise reports not only that beneficiary households are more likely to support the current government but also that political support persists even after the program ends (Manacorda et al. 2010). In Brazil, Zucco (2010) argues that the increase in electoral support for the president in 2006 among poorer voters was a function of the conditional cash-transfer program. An indirect measure of the electoral returns to allocations is offered in a study by Keefer & Khemeni (2009), who show that Indian legislators elected from party strongholds do not use all the funds available for discretionary allocations, presumably because, given their electoral security, they are relatively indifferent to the electoral returns that these allocations would bring. In Mexico, voters have only to receive information about the amount of money spent by the incumbent government to increase that party's vote share (Chong et al. 2010).

The well-known and increasingly indefensible publication bias against null findings undoubtedly contributes to the fact that most studies report that distributive politics is politically effective. Samuels (2002) provides one of the few studies to contend explicitly that distributive allocations do not generate electoral returns. In his study of Brazil, where 90% of budget amendments involve public construction, Samuels argues that these allocations benefit only a small number of persons, particularly those in the firms that are awarded the construction contracts. Many of the projects remain unfinished, and even those that are completed are, Samuels posits, often of little interest to voters. The political exchanges that Samuels argues are involved are more complex: the main returns to construction allocations are the campaign contributions that the contractors make to the elected officials who steer the projects to them. Samuels studies this by estimating whether reelection probabilities of incumbent national deputies in the governing parties who run again are affected by pork (measured as the deputy's percentage of the total value of all pork-barrel amendments released in his state, weighted by district magnitude) and by campaign finance (each deputy's percentage of all campaign finance in his state, also weighted by district magnitude), as well as a series of appropriate control variables. In no case does Samuels report evidence that government spending on what he classes as pork affects electoral returns, whereas campaign spending does.

Another kind of challenge to the theory that expenditures and allocations generate votes comes from Thachil's (2011) study of service provision by Hindu nationalist organizations in India. Thachil examines the ongoing and conspicuously nonclientelistic provision of basic social services, including schools and health care, by a family of organizations associated with India's major Hindu nationalist political party, the Bharatiya Janata Party (BJP), among low-caste Hindus in various localities. Given the BJP's longstanding upper-caste profile, low-caste voters would not be expected to support it and historically have not done so. Using both aggregate and individual-level data, Thachil documents that low-caste recipients of services delivered by BJP auxiliary organizations have a significantly higher probability of voting for the BJP than similar individuals who do not receive services. He interprets these findings as evidence of a successful BJP strategy in "winning hearts and minds" of low-caste voters through the delivery of basic services in an environment in which government fails to provide them.

Thachil's study should give pause to the almost reflexive interpretation of distributive allocations in low-income countries as comprising pork, patronage, and clientelism. It is true that the provision of basic social services by BJP auxiliary organizations is part of a broad strategy aimed at increasing electoral support among low-caste voters. But the services provided are indistinguishable from what in more developed countries comprise the infrastructure of the welfare state. In wealthy countries, such services are provided by government and are the result of decades-long struggles, usually by programmatic working-class political parties, to secure provisions against the risks that ordinary citizens experience in the modern labor market. Referring to these as pork simply because they are delivered by a party-affiliated organization that has electoral objectives seems

inappropriate. In the context in which Thachil works, delivery of these goods is politically neutral. We return full circle to remarks made earlier in this review. It is not the content of the policy that can tell us whether it is allocated as pork, as patronage, or for other reasons. It is rather the broader context in which the goods and services are delivered and perhaps the intentions of the providers.

Unresolved Problems in the Theory of Accountability

The literature on distributive politics and accountability has established an impressive record of systematic data collection and analysis across a variety of distributive policies in a large number of countries. Overall, the literature shows that political actors use their control over government resources to reinforce their electoral advantage. They distribute goods to their loyal supporters to reward them for voting, and to swing voters to buy their votes; they target marginal constituencies to preserve a majority in the national legislature; and they time the targeting so that voters will have the provision of government goods and services fresh in their minds when they head to the polls. Whether this undermines accountability is an open question and not one we consider here. But it is worth considering.

Despite its impressive cumulation of results, this line of work is bedeviled by at least three sorts of problems. We discuss them in order of theoretical importance.

The first is purely operational but can be devastating. It is the difficulty of matching levels and units in data and theory. We considered this in our discussion of the core versus swing debate, but it also affects the study of core and swing constituencies in multimember districts, for instance. Although this problem is technical and not theoretically deep, it is important to recognize at the outset of research projects. Scholars working on distributive politics who want to know the type of voter to whom politicians aim goods need to collect (probably original) information at the level of individual voters. If off-the-shelf data provided by governments are used instead, the main questions animating the project have to be calibrated to the appropriate level of aggregation. They may involve core and swing electoral constituencies, but they cannot involve core and swing voters.

A second problem has to do with the aggregate bias of the results reported. Because scholars want to work on problems that have significant results, and because journals want to publish such results, research in this area has been overwhelmingly structured to identify the political uses of public allocations. Indeed, it is hard not to suspect that the cases that are studied are often selected precisely because they display prima facie evidence of political distortions in allocative decisions. It is thus not surprising that political scientists have found evidence of such distortions; indeed, it is perhaps surprising that any politician ever loses elected office given the impressive evidence that has been amassed showing the politicization of the public purse. But of course, elected officials lose office with considerable if variable frequency. This suggests either that the study of allocations is incomplete, a problem identified by Cox (2010), or that the cumulative results of this research agenda are biased—or both. Because of difficulties in data collection, studies may be limited to single policy outputs or single countries. Scholars may overestimate the impact of material inducements, for instance, confusing evidence of targeted allocations with causality. Even if we find that a subset of voters has received gifts from candidates during political campaigns, this does not constitute evidence that the gifts and the vote are causally related. Indeed, voters cannot usually be "bought" with a bag of rice, but even if voters do not sell their civic rights so cheaply, how much does the bag of rice contribute to making the candidate appear more attractive? For results to cumulate, studies need to test hypotheses across countries and goods, as well as to establish causality using appropriate identification techniques.

A final and related problem is neglect of the relevant counterfactual or baseline. This issue is especially well exposed in the literature that studies whether allocations proceed on the basis

of political favoritism and political distortions. It is hard to determine how much favoritism is in play without an explicit discussion of what a neutral or bias-free distribution would look like. To illustrate distributive allocations in the absence of political favoritism, we turn to the theory of policy responsiveness.

PROBLEMS OF POLICY RESPONSIVENESS

An alternative way of thinking about government allocations to localities investigates the socioeconomic impact and redistributive consequences of these allocations. Basic questions involve whether goods and services are allocated in an equitable way or whether instead specific groups of the population, which may be called special interests, capture disproportionate shares. Whereas the accountability framework considers the partisan alignment between the groups receiving goods and services and the public authorities making the allocative decisions, an equity framework studies the alignment between the policy goals, the intended recipients, and the actual recipients of public allocations. The accountability framework asks whether politicians target localities and groups with goods because doing so will prove electorally most advantageous. The responsiveness framework, by contrast, asks whether allocations reflect the interests of voters, particularly the median voter. Loosely speaking, we might say that the latter approach asks whether different classes of citizens receive their "fair share" of allocations from government.

The canonical model of special interest politics is due to Grossman & Helpman (1996, 2001). In a simple Downsian model of voting, two political parties that compete to win an election will, given certain rather trivial assumptions, converge on the policy preferences of the median voter. The Downsian view of electoral competition is extremely abstract and simple but also very powerful. It forces us to consider what differs from this underlying scenario when we observe, as is so often the case, policies patently at odds with the preferences of the median voter. Grossman & Helpman's answer is that special interest groups, which are defined by policy preferences that differ from those of the median voter, can tilt policy in their preferred direction by offering campaign contributions and expert information to politicians. Politicians, in turn, use the campaign contributions to influence more impressionable or less sophisticated voters. This framework is often used to understand tariffs. Tariffs raise the price of protected goods for ordinary consumers; hence, the median voter's interests are antitariff. We observe tariffs nonetheless because producer groups, well known for their abilities to organize and lobby (Olson 1965), successfully bombard legislators with campaign contributions in order to secure legislation that raises the prices of goods in their industries.

The terminology used by the special interest perspective differs from that used in the accountability framework. The former is concerned with what it calls the median voter compared with special interests as well as with impressionable or less-informed voters. The latter, by contrast, is concerned with what it calls core and swing voters, or with specific subsets of ethnically or otherwise identifiable voters. Despite these differences in terminology, both are probabilistic voting models with features in common. Each begins with two political parties that appeal to voters on the basis of fixed characteristics such as program or ideology, as well as on the basis of election-specific activities, such as material inducements or campaign spending. Voters vary in the degree to which they are responsive to the latter. What Dixit & Londregan call swing voters are similar to the less-informed voters of Grossman & Helpman. These are voters who are politically responsive to public spending, material inducements, or campaign spending, either because they exhibit low partisanship or are poor (Dixit & Londregan) or because they lack political information and are hence impressionable (Grossman & Helpman). They are voters who can be easily bought or swayed by material or informational inducements. If the voting population did not consist in part

of people like this, then, absent demographic change, elections would be entirely predictable on the basis of fixed partisan loyalties.

Redistribution and Political Capture

An extension of the Grossman & Helman framework particularly pertinent to our purposes is found in a set of interrelated papers by Bardhan & Mookherjee (2000, 2005, 2006a). Set theoretically within the literature on fiscal federalism, their work seeks to evaluate whether in developing countries decentralized service delivery provides more equitable or efficient outcomes than centralized systems. An important motivation for this line of work comes from the chronic political distortions and compromised accountability so often observed in service delivery in low-income countries (Keefer & Khemani 2005). Precisely because of the phenomena discussed in the first part of this review, such as targeting goods and services to electorally important subgroups, local governments may exhibit low levels of accountability; that is, local public officials may routinely direct government benefits intended for certain classes of recipients ("the poor") to specific other subgroups ("local elites") without suffering loss of office. What we could call the local capture research agenda is especially relevant to situations marked by high inequality and high poverty rates, where special interests principally consist of traditional landed elites rather than modern producer groups. Bardhan & Mookherjee offer a framework for identifying the equity and welfare impacts of decentralized versus centralized delivery mechanisms in such settings.[5]

Bardhan & Mookherjee adapt the Grossman & Helpman model to the local level. In the reformulation, the poor experience disadvantages in allocations of government services because of their lack of political information, typically proxied by literacy or poverty rates, and because local elites provide campaign contributions or other resources to local political candidates that the poor, by definition, cannot. The outcomes that Bardhan & Mookherjee observe include the diversion of government goods and services meant for the poor into the hands of local elites. They refer to this as political capture.

Bardhan & Mookherjee's main concern is to evaluate centralized versus decentralized delivery systems within a federal political system. This is a standard way to investigate political responsiveness. Many of the papers classed in the responsiveness category in our inventory concern allocations from a central political authority to subnational units, such as states. Porto & Sanguinetti (2001) study intergovernmental grants in Argentina; Khemani (2007), intergovernmental grants in India; Pereira (1996), intergovernmental grants in Portugal; and Dall'erba (2005 and other papers), the use of Structural Funds in the European Union, funds aimed at improvements in Europe's economically depressed regions.

Bardhan & Mookherjee highlight the redistributive consequences of the accountability issues raised in the first part of our review. Students of distributive politics have focused on voter types defined by their degree of party loyalty, by socially constructed identities (ethnicity, religion, and so forth), and by other context-specific identities (such as region). There has been little attention to the underlying left-right dimension that has informed political conflict in Europe and elsewhere since the French Revolution. Social class and income have hardly figured in discussions of distributive politics, except incidentally. We find in this literature no systematic attempts to evaluate the redistributive consequences of allocations. Bardhan & Mookherjee, by contrast, are fundamentally concerned with whether poor voters receive the allocations to which they are in principle entitled or whether these are diverted by wealthier groups.

---

[5]We do not attend to the issue of centralization versus decentralization in this review.

A useful empirical example is Bardhan & Mookherjee's (2006a) study of allocations to poor households in 89 West Bengal villages. Using data collected over two decades (1979 to 1998), the authors study distributions of credit, agricultural input kits (packages of seeds and other basic agricultural inputs that farmers may be too poor to purchase), employment programs, and fiscal grants both across and within villages. They identify antipoor distortions in allocations of agricultural minikits and credit across but not within villages. Within villages, those in need receive the grants and minikits as intended, probably because all villagers are aware of which households are entitled to these benefits. However, villages with more poor households of low caste status receive significantly fewer minikits and grants. In addition, local governments select infrastructure construction projects that generate less employment for the poor when landlessness and inequality within the village are greater. The effect, the authors conclude, discriminates against villages whose need for the resources is greater and favors villages with more effective lobbies of medium and small landowners (rather than marginal landholders). Despite West Bengal's longstanding leftist government, discretionary allocations across villages may reduce policy responsiveness. Bardhan & Mookherjee find that responsiveness is especially compromised in construction projects, probably because they are inherently less transparent than allocations intended for individuals and households.

On the whole, existing studies can be divided neatly into two groups: those using an accountability perspective and those using a responsiveness perspective. Only a handful of studies attempt both and simultaneously examine the political objectives of allocations as well as welfare outcomes. Examples include studies by Castells & Solé-Ollé (2005) and Diaz-Cayeros et al. (2012). Castells & Solé-Ollé (2005) investigate infrastructure investments in Spain from 1987 to 1996. They specify an allocative baseline from a general specification of the government's objective function and then study political deviations from that. Political deviations reflect electoral needs. Castells & Solé-Ollé find that allocations mainly reflect regional need (a redistributive consideration) and political returns. Diaz-Cayeros et al. (2012) trace government transfers for poverty relief in Mexico over a half century as policy evolved from clientelistic to programmatic. Their pathbreaking study provides inspiration for our view that a complete understanding of allocations requires attention to both the accountability and responsiveness aspects. By examining both, the authors assess connections between the degree of accountability and the extent to which policy reflects voter preferences. Diaz-Cayeros et al. show that as Mexican welfare policy moved from more to less discretionary, welfare effects improved. In particular, they compare (ch. 8) changes in infant mortality rates as a result of improvements in the provision of public goods, conditional cash transfers, and discretionary transfers to individuals based on patron-client networks. Their results suggest that discretionary transfers are less welfare improving than provisions of public goods or means-tested cash transfers to low-income households. The study is unique in its comparison of the welfare effects of discretionary and nondiscretionary policies.

The responsiveness perspective has at least three analytic and operational features that distinguish it from the accountability approach. First, in the accountability literature, as we have noted, it is often unclear whether a distortion-free allocation exists or what it would look like. Political parties in power target resources to their constituents. There is nothing inequitable or inefficient about this. Nor is it surprising or puzzling to uncover evidence that political parties and politicians seek reelection and that they devise policies appealing to specific groups of voters to do so. Some of the issues analyzed in an accountability framework, such as whether core or swing voters are targeted and under what conditions, are subtler and theoretically more compelling, but studies of political favoritism easily fall prey to failure to specify what a bias-free allocation would be. Because parties may have programmatic commitments and distinct constituencies, they can be expected to favor certain groups, and a study's failure to identify a significant partisan element to allocations (the null hypothesis) is not necessarily evidence of neutrality; more likely

it reflects the analyst's mismeasurement or incomplete understanding of the larger patterns of politics.

The responsiveness perspective, by contrast, because of its roots in median voter theory, offers a natural benchmark against which to assess policy distortion. The median voter's income is below average (Meltzer & Richard 1981); hence, policies that favor below-average incomes are responsive, whereas those favoring upper-income groups are not. It is straightforward to identify whether policies favor the median voter or not.

Second, data collection in this framework is more easily achieved than in the accountability framework. We stressed in our discussion of the core versus swing voter debate that proper testing requires individual-level data on political preferences, data that are often difficult to collect. Although it may be useful to collect survey data to know if policy is responsive, as Bardhan & Mookherjee (2006b) did, it is often unnecessary. In many instances, it is possible to evaluate policy outputs by simply gauging their distance from the median voter's presumed ideal point. Average income locates this ideal point.

Third, because the benchmark of the welfare-maximizing allocation is more easily identified and distortions thereby easier to measure, results of this line of work are likely to cumulate more effectively than results of the studies we have classed under the accountability perspective.

Unresolved Problems in the Theory of Policy Responsiveness

The capture framework provided by Grossman & Helpman and Bardhan & Mookherjee offers much that is new and complementary to studies of political accountability. But it also suffers from at least one glaring unanswered question. How can politicians permit chronic capture of government goods and services without eventually losing public office? In democratic settings, political distortions that continue for multiple electoral periods and are highly visible to voters ought to inspire electoral repudiation of the incumbent. Such situations ought to bring new political entrepreneurs into the electoral arena to mobilize voter discontent with existing outcomes. Candidates should emerge who promise to end political capture or at least to counter capture with organization by those who are most affected. Even in low-information environments, the extreme political capture depicted by Bardhan & Mookherjee, for instance, cannot be perpetuated indefinitely, and certainly not as economic growth lifts the poorest segments of society out of subsistence. The Olson and Grossman & Helpman answer—that small, organized groups can shift policy in their preferred direction—should not prove durable if previously unorganized groups of poor voters can be mobilized to counterpressure local elites.

Why the median voter is unable to wrest policy out of the hands of special interests is a question of longstanding interest in high-income democracies. An interesting line of work is likely to emerge as scholars seek to understand how and when this occurs in developing nations, where political rule has so often been in the hands of small cliques of politicians who exercise weak governance. If political capture is due to voter ignorance and poverty, then rising literacy, access to information, and economic growth can reset policy in a direction closer to that preferred by the median voter. The kind of political capture depicted by Bardhan & Mookherjee is not likely to prove enduring over the long term.

ILLUSTRATING THE TWO THEORIES WITH AN ANALYSIS OF ELECTRICITY PROVISION IN UTTAR PRADESH, INDIA

To illustrate the issues we have discussed regarding the accountability and responsiveness perspectives, we assembled data on the provision of electricity in India's most populous state, Uttar

Pradesh (UP). The UP electricity data permit us to evaluate both theories, although imperfectly because of issues, described shortly, that limit matching the levels of analysis for which different types of data are available. In addition, the data illustrate some common operational hurdles confronting scholars working on distributive politics. Mainly for reasons of space, but also for theoretical reasons, we do not illustrate all the aspects of the accountability perspective that we have reviewed in this article. We focus on the core versus swing debate, which is theoretically the most carefully specified, comparing results from tests of this theory with results from the theory of political capture. Our analysis highlights benefits to broadening the concerns of political scientists working in the area of distributive politics to include problems of capture and redistribution.

Our dataset contains information on the flow of government goods—kilowatts of electricity—to local areas. The localities comprise ~150 service divisions used by the government-owned UP Power Corporation Ltd. (UPPCL) to track the distribution of electricity. These divisions map imperfectly onto the state's political units (403 state assembly constituencies), and because the precise geography of UPPCL's divisions is not reported, we have no straightforward way to match electricity to electoral data. This problem is typical of those normally confronting scholars of distributive politics, and our discussion highlights the operational challenges in managing data and analysis in settings where administrative and electoral boundaries often differ and accepted algorithms for aligning them do not exist.

Despite the vexing issues of matching geographic units, our electricity data contain rich information worth analyzing. We know the number of customers and the connected load in each of the power corporation's divisions disaggregated by sector (domestic, agricultural, industrial, commercial, and other smaller categories). This disaggregation allows us to investigate variations across types of users who are provided a scarce and valuable government good. In addition, our data are available on a monthly basis from April 2001 through April 2008, spanning 85 months.

Within India's federal system, the provision of electricity falls under the jurisdiction of the state and not the national level. Accordingly, we analyze electricity provision in relation to state-level political data. UP comprises 403 SMD electoral constituencies that elect Members of Legislative Assemblies (MLAs) to the state assembly (the Vidhan Sabha) roughly every five years. State elections were held in 2002 and 2007. We hypothesize that various political characteristics of state assembly constituencies could prove relevant to electricity provision in a context of chronic inadequate supply: how close the election was, which party held the assembly seat in the constituency, which party controlled the state's Chief Minister, the electoral strengths of the major political parties in the locality, and so forth.

Absent information on the boundaries of the UPPCL divisions, we cannot map directly the UPPCL's geographic service divisions to assembly constituencies. Each assembly constituency and UPPCL service division can, however, be matched to a single administrative district, which is a unit roughly comparable to a US county. We aggregate data from the UPPCL and from state assembly elections to the level of administrative districts, of which there are 70 in UP. The mismatch in geographic levels between the power corporation data and the political data entails an obvious loss of information and introduces imprecision into the statistical results that we generate. We are forced to estimate whether, at the district level, the proportion of electricity going to each sector relates to political indicators such as the proportion of MLAs in each district affiliated with each party. In most countries, data on the distribution of government goods and services are collected by bureaucracies operating in administrative units that differ geographically from electoral units and are often not perfect subsets of them, so most studies of distributive politics confront similar problems.

We conduct multivariate regression analyses to examine whether electricity appears to be provided differentially to core or swing regions of electoral support and to examine whether local

elites appear to be capturing a disproportionate share of this public resource. We examine the core versus swing agenda by studying whether households in districts where assembly seats are more closely contested receive more or less electricity. We examine the political capture agenda by studying whether elite farmers who own electric irrigation equipment receive more electricity regardless of the partisanship of their state assembly representatives and in situations characterized by greater electoral competition. Overall, our results support the core hypothesis as well as the view that the closer the electoral race, the more wealthy farmers can extract policy concessions. We also find some support for the view that even political parties whose platforms reflect urban priorities allocate more electricity to wealthy farmers, suggesting that farmers successfully operate as a special interest within the state.

Our interpretation of the results hinges on a few stylized facts about UP's political environment that inform our theoretical setup. First, we assume that the public utility corporation is subject to relentless political pressure by elected officials as well as by citizens for more electricity. We operationalize this political pressure with attention to the partisan affiliation of MLAs and of the Chief Minister. State politics during the period we study are predominantly played out among four political parties: the once dominant but increasingly marginal Congress Party, representing a mix of voters from all castes and classes; the Hindu nationalist BJP, representing an alliance of upper-caste and middle-class voters, especially in urban areas; the Bahujan Samaj Party (BSP), a party of the Scheduled Castes and other historically disadvantaged groups; and the Samajwadi Party (SP), which draws voters mainly from Other Backward Classes (OBCs) and Muslims.

Second, we conceptualize the conflicts between the median voter and special interests along sectoral lines, with a focus on the relative allocation of electricity to the domestic sector relative to agriculture. We interpret the domestic sector as representing the preferences of the median household. Agricultural needs for electricity mainly involve tubewells or pumpsets, which are used to draw groundwater for irrigation during the dry season. The equipment is expensive to install and maintain, although, thanks to generous pricing subsidies, it is generally inexpensive to operate. In UP, only relatively well-off farmers own electrical equipment that permits uninterrupted irrigation; in 2008, there were 729,000 electric tubewells amid 35 million persons employed in the agricultural sector. These farmers are commonly believed to represent a powerful interest group (Lal 2006, Modi 2007), and we interpret the owners of tubewells as representing a special interest.

We operationalize the core versus swing issue the following way. First, we create a measure of the margin of victory of the winning candidate over the runner-up in the 2002 and 2007 state assembly elections for each of the 403 state assembly constituencies. This generates a measure of marginality, with smaller numbers indicating a constituency is "swing" and larger numbers indicating it is "safe" for the winning party. We then aggregate these indicators into district-level averages to create a single measure of how competitive the assembly seats are in the district (on average, each district comprises eight assembly constituency seats). We examine whether the average extent of political marginality is associated with the subsequent provision of more electricity to households on a monthly basis. A statistically significant negative relationship between marginality and electricity provision to households suggests that incumbent parties strategically allocate more electricity directly to voters where they most fear losing seats. A statistically significant positive relationship between marginality and electricity provision to households, by contrast, suggests that MLAs provide electricity to areas where their seats are most secure.

Second, to study whether electricity provision appears to be captured by local elites, we investigate whether tubewell owners appear to be systematically favored with disproportionate access to electricity. The capture hypothesis would be supported by finding that even the BJP, the political party most identified with urban voters, provides the same levels of electricity to agriculture as the SP, the party most closely associated with large farmers and land-owning interests.

We run separate ordinary least squares (OLS) regressions for the proportion of the electricity supply directed at the domestic sector and the proportion of the supply going to tubewells in each district in each month from April 2001 to April 2008. Both sets of regressions feature similar regressors. The first theoretically relevant independent variable is the average extent of electoral marginality, used to study whether swing or core areas receive more or less electricity. We control for the proportion of seats in the district controlled by the BSP, the BJP, and the SP. The omitted category comprises the Congress and legislators from other political parties. Note that these partisanship indicators vary only three times over the 85 months of data, before and after the February 2002 and the May 2007 state elections. We also control for the party holding the office of Chief Minister of the state, which varies four times in the period we study, including a brief period of President's Rule in March–April 2002, which is the omitted category. We include an interaction effect for this variable and the proportion of seats in the district controlled by the same party as the Chief Minister. This allows us to test whether the Chief Minister uses her influence to provide "extra" electricity to households in districts that gave more votes to her party. We also include controls for the size of each sector (that is, the proportion of consumers in the domestic or agricultural sector), which in our dataset varies monthly and by district, and the level of development, using an aggregate human development index compiled by the state government combining income, education, and health measures. Finally, we include fixed effects for each district and for each month. The district fixed effects control for unmeasured characteristics of a district that are constant over time, such as geographic characteristics and general differences in rural-urban concentration. The month fixed effects help account for seasonal effects and time trends that affect all districts similarly and are especially important in a context where the total electricity supply and demand vary seasonally and secularly. Regression results are reported in **Table 1**.

The results reported in **Table 1** under Model 1 show that as the proportion of assembly seats held by the SP or BSP increases within a district compared with the proportion of seats held by Congress and other parties, so too does the electricity load going to the domestic sector. The BJP, by contrast, does not direct a larger electricity load to households even where its share of assembly seats increases. These results reflect differences between parties (notably, the BSP and the SP) that heavily represent scheduled caste and OBC voters, on the one hand, and parties (the BJP) representing more elite castes or (the Congress) mixed social groups. The findings remain robust to additional controls added in Model 3, including indicators for which party controls the Chief Minister's office. Consistent with our expectations, a Chief Minister from the SP increases the share of power going to the domestic sector. A BJP Chief Minister is associated with a decline in electricity provision to households, perhaps reflecting the importance of industrial and commercial interests to the party. The interaction terms, which estimate the effects of representation by a party when it also controls the Chief Minister's office, suggest that the negative impact of the BJP on the domestic sector is even higher in such contexts.

An additional theoretically important finding here is that higher average win margins are also associated with increases in the load going to the domestic sector. For every one-point increase in average win margin in a district, the share of electricity going to the domestic sector increases by 0.21 points. These results appear to show that parties reward households in their strongholds with electricity, all else equal.

Models 2 and 4 present the results of parallel regressions over the proportion of the electricity load going to agricultural special interests, specifically to privately owned electric tubewells used for irrigation. The results show that the proportion of the electrical load is unaffected by changes in the party composition of MLAs elected in the district. When the proportion of assembly seats going to the BSP, to the SP, or the BJP changes, there is no statistically significant change in the load to tubewells. We interpret these results as corroboration that wealthy farmers receive a

Table 1 Shares of electrical load to domestic sector and tubewells, Uttar Pradesh (April 2001–April 2008); monthly data for 70 administrative districts

	(1) Domestic consumers	(2) Tubewell owners	(3) Domestic consumers	(4) Tubewell owners
Share SP MLAs	0.015[b]	−0.007	0.011[a]	0.005
	(0.006)	(0.004)	(0.006)	(0.004)
Share BJP MLAs	0.003	−0.002	0.008	0.006
	(0.004)	(0.005)	(0.009)	(0.006)
Share BSP MLAs	0.013[a]	−0.004	0.017[a]	0.004
	(0.008)	(0.005)	(0.009)	(0.005)
Chief Minister SP			0.017[b]	−0.008
			(0.007)	(0.005)
Chief Minister BJP			−0.022[b]	0.011[a]
			(0.009)	(0.006)
Chief Minister BSP			−0.001	−0.002
			(0.007)	(0.005)
Share SP MLAs × CM SP			0.002	−0.012[c]
			(0.004)	(0.003)
Share BJP MLAs × CM BJP			−0.021[a]	0.000
			(0.011)	(0.007)
Share BSP MLAs × CM BSP			−0.009	0.001
			(0.006)	(0.004)
Average electoral marginality			0.212[c]	−0.104[c]
			(0.028)	(0.021)
Share domestic consumers			0.709[c]	−0.112[a]
			(0.127)	(0.062)
Share industry consumers			−0.789[c]	0.250[a]
			(0.216)	(0.107)
Share commercial consumers			0.287[a]	−0.607[c]
			(0.160)	(0.096)
Share tubewell consumers			−1.025[c]	0.931[c]
			(0.165)	(0.111)
Level of development (HDI)			0.001	0.717[c]
			(0.140)	(0.077)
District fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
N	5664	5664	5664	5664
R-squared	0.915	0.950	0.925	0.955

BJP, Bharatiya Janata Party; BSP, Bahujan Samaj Party; CM, Chief Minister; HDI, district-level Human Development Index (0–1 scale, coded 2001 and 2005); MLAs, Members of the Legislative Assembly; SP, Samajwati Party. Robust standard errors in parentheses.
[a] $p < 0.1$, [b] $p < 0.05$, [c] $p < 0.01$.

relatively unaltered share of electricity regardless of changes in partisan strength in the district. Even when the BJP, for instance, grows in electoral influence, it does not significantly reduce the share of power going to wealthy farmers, nor does the BSP, which represents low-caste voters. In fact, BJP control of the Chief Minister's office is associated with a slight increase in the share of power going to wealthy farmers. These results suggest that wealthy farmers have successfully captured the state polity; all political representatives appear similarly responsive to the needs of the state's largest farmers for electricity to run their tubewells. This finding corroborates the common impression that politicians of all stripes are afraid to take on the Indian agricultural sector and reduce agricultural subsidies. The results also show that as the district experiences greater electoral competitiveness, indicated by smaller values in the variable measuring the average marginality in the district, the proportion of the load going to tubewells increases. A plausible interpretation is that tubewell-owning farmers lobby for more electricity and receive more because parties compete to outbid each other as electoral races tighten.

Taken together, these analyses corroborate, first, that political parties reward voters in their strongholds and, second, that all political parties in UP are responsive to the interests of the state's wealthiest farmers. The first finding speaks to the importance of electoral competition in the allocation of a scarce government-provided resource in UP; a political party rewards household consumers with more electricity in the areas of the party's greatest electoral strength. It seems plausible that political parties compete to establish geographically demarcated areas of influence in the state and to build loyal vote blocs within them. The second finding, however, supports the quite different notion that energy policy in UP is responsive to a small rural elite. Even as each party seeks to direct electricity to the households in the areas where its own voters are most concentrated, all parties provide extra electricity to a very small special interest composed of farmers with electric tubewells.

The two perspectives thus generate results that lead to different interpretations of which groups are favored in electricity allocations in India's most populous state. The accountability framework leads to the interpretation that each party's loyal voters and their neighbors are favored. The responsiveness framework leads to the interpretation that a small rural elite is favored, perhaps at the expense of ordinary voters. The results generated by the responsiveness framework are relevant to an intense debate occurring throughout South Asia on the consequences of subsidized inputs for agriculture, especially energy inputs. These consequences include massive debt on the part of the energy providers, which are typically government owned, and a potentially catastrophic overdrawing of groundwater in the region. These important issues arise naturally when the responsiveness framework is used in this context.

CONCLUSIONS

Distributive politics is an area of inquiry that has flourished in both developed and less-developed settings, and in the United States as well as in other countries. We have reviewed studies of distributive politics around the world. We have identified a variety of questions that students of comparative politics analyze, which mainly concern issues of accountability and policy responsiveness. Our primary goals have been to impose some conceptual structure on what has become a large and analytically disparate literature; to provide (although not to summarize) an inventory of the relevant literature; and to highlight the importance of establishing benchmark allocations against which to calibrate the extent and nature of political distortions.

Our study of how electrical power is allocated in northern India shows, we believe, that theories that begin with issues of accountability before considering whether policy is responsive to the interests of voters put the cart before the horse. If allocations were welfare maximizing, then

attempts to unpack the specific political conflicts that lie behind them would be spurious or incidental. If they are not—as appears to be the case in the provision of electricity to tubewell owners in UP—then the accountability issue emerges naturally as a second-order research question. If rural elites benefit disproportionately from electricity provision in UP, why do voters not hold elected officials accountable, replacing them with new legislators who will shift policy toward the interests of the median voter? Accountability issues arise when policy is not responsive to voter interests. In our view, a more complete account of distributive politics would first demarcate the parameters of policy responsiveness to voter preferences, next document whether allocations reflect such preferences, and only then turn to accountability issues and ask how politicians benefit electorally from their distributional strategies.

DISCLOSURE STATEMENT

The authors are not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

LITERATURE CITED

Ames B. 1995. Electoral strategy under open-list proportional representation. *Am. J. Polit. Sci.* 39(2):406–33

Arulampalam W, Daspupta S, Dhillon A, Dutta B. 2009. Electoral goals and center-state transfers: a theoretical model and empirical evidence from India. *J. Dev. Econ.* 88:103–19

Ashworth S. 2012. Electoral accountability: recent theoretical and empirical work. *Annu. Rev. Polit. Sci.* 15:183–201

Baldwin K. 2005. *Who gets the jobs? Dynamics of opposition and redistribution in Mali*. Unpublished paper, Dep. Polit. Sci., Columbia Univ.

Banerjee A, Somanathan R. 2007. The political economy of public goods: some evidence from India. *J. Dev. Econ.* 82(2):287–314

Bardhan P, Mookherjee D. 2000. Capture and governance at local and national levels. *Am. Econ. Rev.* 90(2):135–39

Bardhan P, Mookherjee D. 2005. Decentralizing antipoverty program delivery in developing countries. *J. Public Econ.* 89:675–704

Bardhan P, Mookherjee D. 2006a. Decentralisation and accountability in infrastructure delivery in developing countries. *Econ. J.* 116(508):101–27

Bardhan P, Mookherjee D. 2006b. Pro-poor targeting and accountability of local governments in West Bengal. *J. Dev. Econ.* 79(2):303–27

Bates R. 1974. Ethnic competition and modernization in contemporary Africa. *Comp. Polit. Stud.* 6(4):457–84

Bates R. 1981. *Markets and States in Tropical Africa*. Berkeley: Univ. Calif. Press

Blaydes L. 2011. *Elections and Distributive Politics in Mubarak's Egypt*. New York: Cambridge Univ. Press

Cadot O, Röller L-H, Stephan A. 2006. Contribution to productivity or pork barrel? The two faces of infrastructure investment. *J. Public Econ.* 90:1133–53

Cain B, Ferejohn J, Fiorina M. 1987. *The Personal Vote: Constituency Service and Electoral Independence*. Cambridge, MA: Harvard Univ. Press

Canes-Wrone B, Park J-K. 2012. Electoral business cycles in OECD countries. *Am. Polit. Sci. Rev.* 106(1):103–22

Canes-Wrone B, Park J-K. 2013. Elections, uncertainty, and irreversible investment. *Br. J. Polit. Sci.* In press

Carey JM, Shugart MS. 1995. Incentives to cultivate a personal vote: a rank ordering of electoral formulas. *Electoral Stud.* 14(4):417–39

Castells A, Solé-Ollé A. 2005. The regional allocation of infrastructure investment: the role of equity, efficiency and political factors. *Eur. Econ. Rev.* 49(5):1165–205

Chhibber P, Nooruddin I. 2004. Do party systems count? The number of parties and government performance in the Indian states. *Comp. Polit. Stud.* 37(2):152–87

Chong A, De La O AL, Karlan D, Wantchekron L. 2010. *Information dissemination and local governments' electoral returns, evidence from a field experiment in Mexico*. Unpublished paper, Dep. Polit. Sci., Yale Univ.

Cole S. 2009. Fixing market failures or fixing elections? Agricultural credit in India. *Am. Econ. J. Appl. Econ.* 1(1):219–50

Cox G. 2010. Swing voters, core voters and distributive politics. In *Political Representation*, ed. I Shapiro, SC Stokes, EJ Wood, AS Kirshner, pp. 342–57. Cambridge, UK: Cambridge Univ. Press

Cox GW, McCubbins MD. 1986. Electoral politics as a redistributive game. *J. Polit.* 48(2):370–89

Crisp BF, Desposato SW. 2004. Constituency building in multimember districts: collusion or conflict? *J. Polit.* 66(1):136–56

Dahlberg M, Johansson E. 2002. On the vote-purchasing behavior of incumbent governments. *Am. Polit. Sci. Rev.* 96(1):27–40

Dall'erba S. 2005. Distribution of regional income and regional funds in Europe 1989–1999: an exploratory spatial data analysis. *Ann. Reg. Sci.* 39:121–48

De La O AL. 2008. *Do conditional cash transfers affect electoral behavior? Evidence from a randomized experiment in Mexico*. Unpublished paper, Dep. Polit. Sci., Yale Univ.

Dewan T, Shepsle KA. 2011. Political economy models of elections. *Annu. Rev. Polit. Sci.* 14:311–30

Diaz-Cayeros A, Estévez F, Magaloni B. 2012. *Strategies of vote buying: Democracy, clientelism and poverty reduction in Mexico*. Unpublished book manuscript, Dep. Polit. Sci., Univ. Calif. San Diego

Dixit A, Londregan J. 1996. The determinants of success of special interests in redistributive politics. *J. Polit.* 58(4):1132–55

Drazen A, Eslava M. 2005. *Electoral manipulation via expenditure composition: theory and evidence*. NBER Work. Pap. No. 11085

Ferejohn J. 1986. Incumbent performance and electoral control. *Public Choice* 50(1–3):5–25

Ferraz C. 2006. *Political influence and environmental regulation: evidence from municipal electoral cycles in urban Brazil*. Unpublished paper, Dep. Econ., Univ. Calif. Berkeley

Golden MA, Min B. 2013. *Electoral cycles in electricity losses in India*. Unpublished paper, Dep. Polit. Sci., Univ. Calif. Los Angeles

Golden MA, Picci L. 2008. Pork barrel politics in postwar Italy, 1953–1994. *Am. J. Polit. Sci.* 52(2):268–89

Grossman GM, Helpman E. 1996. Electoral competition and special interest politics. *Rev. Econ. Stud.* 63:265–86

Grossman GM, Helpman E. 2001. *Special Interest Politics*. Cambridge, MA: MIT Press

Hicken A. 2011. Clientelism. *Annu. Rev. Polit. Sci.* 14:289–310

Kasara K. 2007. Tax me if you can: ethnic geography, democracy, and the taxation of agriculture in Africa. *Am. Polit. Sci. Rev.* 101(1):159–72

Keefer P, Khemani S. 2005. Democracy, public expenditures, and the poor: understanding political incentives for providing public services. *World Bank Res. Obs.* 20(1):1–27

Keefer P, Khemeni S. 2009. When do legislators pass on pork? The role of political parties in determining legislator effort. *Am. Polit. Sci. Rev.* 103(1):99–112

Khemani S. 2007. Does delegation of fiscal policy to an independent agency make a difference? Evidence from intergovernmental transfers in India. *J. Dev. Econ.* 82(2):464–84

Kitschelt H, Wilkinson SI. 2007. Citizen-politician linkages: an introduction. In *Patrons, Clients, and Policies: Patterns of Democratic Accountability and Political Competition*, ed. H Kitschelt, SI Wilkinson, pp. 1–49. New York: Cambridge Univ. Press

Kudamatsu M. 2012. Has democratization reduced infant mortality in sub-Saharan Africa? Evidence from micro data. *J. Eur. Econ. Assoc.* 10(6):1294–317

Kwon HY. 2005. Targeting public spending in a new democracy: evidence from South Korea. *Br. J. Polit. Sci.* 35(2):321–41

Lake DA, Baum MA. 2001. The invisible hand of democracy: political control and the provision of public services. *Comp. Polit. Stud.* 34:587–621

Lal S. 2006. *Can good economics ever be good politics? Case study of the power sector in India*. Work. Pap. No. 83, World Bank, Washington, DC

Lasswell HD. 1936. *Politics: Who Gets What, When, How*. New York: McGraw-Hill

Lindbeck A, Weibull J. 1987. Balanced budget redistribution and the outcome of political competition. *Public Choice* 52:273–97

Lizzeri A, Persico N. 2001. The provision of public goods under alternative electoral incentives. *Am. Econ. Rev.* 91:225–45

Manacorda M, Miguel E, Vigorito A. 2009. *Government transfers and political support*. NBER Work. Pap. No. 14702

Manin B, Przeworski A, Stokes SC. 1999. Elections and representation. In *Democracy, Accountability, and Representation*, ed. A Przeworski, SC Stokes, pp. 29–54. Cambridge, UK: Cambridge Univ. Press

Meltzer AH, Richard SF. 1981. A rational theory of the size of government. *J. Polit. Econ.* 89(5):914–27

Milesi-Ferretti G, Perotti R, Rostagno M. 2002. Electoral systems and the composition of public spending. *Q. J. Econ.* 117:609–57

Min B. 2010. *Democracy and light: public service provision in the developing world*. PhD thesis, Dep. Polit. Sci., Univ. Calif. Los Angeles

Modi V. 2007. Improving electricity services in rural India. *Electricity Today* 16–21

Nichter S. 2008. Vote buying or turnout buying? Machine politics and the secret ballot. *Am. Polit. Sci. Rev.* 102(1):19–31

Nordhaus WD. 1975. The political business cycle. *Rev. Econ. Stud.* 42(2):169–90

Olson M. 1965. *The Logic of Collective Action: Public Goods and the Theory of Groups*. Cambridge, MA: Harvard Univ. Press

Ortega D, Penfold-Becerra M. 2008. *Does clientelism work? Electoral returns of excludable and non-excludable goods in Chavez's Misiones programs in Venezuela*. Unpublished paper, Inst. Estud. Superiores de Administración, Caracas, Venezuela

Pereira PT. 1996. A politico-economic approach to intergovernmental lump-sum grants. *Public Choice* 88:185–201

Porto A, Sanguinetti P. 2001. Political determinants of intergovernmental grants: evidence from Argentina. *Econ. Policy* 13(3):237–56

Posner DN, Kramon E. 2011. *Who benefits from distributive politics? How the outcome one studies affects the answer one gets*. Unpublished paper, Dep. Polit. Sci., Mass. Inst. Technol.

Przeworski A, Stokes SC, Manin B, eds. 1999. *Democracy, Accountability, and Representation*. Cambridge, UK: Cambridge Univ. Press

Samuels DJ. 2002. Pork barreling is not credit claiming or advertising: campaign finance and the sources of the personal vote in Brazil. *J. Polit.* 64(3):845–63

Shi M, Svensson J. 2006. Political budget cycles: Do they differ across countries and why? *J. Public Econ.* 90(8–9):1367–89

Stasavage D. 2005. Democracy and education spending in Africa. *Am. J. Polit. Sci.* 49(2):343–58

Stokes SC. 2005. Perverse accountability: a formal model of machine politics with evidence from Argentina. *Am. Polit. Sci. Rev.* 99(3):315–25

Stokes SC, Dunning T, Nazareno M, Brusco V. 2011. *Buying votes: distributive politics in democracies*. Unpublished manuscript, Dep. Polit. Sci., Yale Univ.

Stratmann T, Baur M. 2002. Plurality rule, proportional representation, and the German Bundestag: how incentives to pork-barrel differ across electoral systems. *Am. J. Polit. Sci.* 46(3):506–14

Teitelbaum E, Thachil T. 2010. *Demanding deeper democracy: party fragmentation, ethnic mobilization, and programmatic politics in the Indian states*. Unpublished paper, Dep. Polit. Sci. and Intl. Aff., George Washington Univ.

Thachil T. 2011. Embedded mobilization: social services as electoral strategy in India. *World Polit.* 63(3):434–69

Toha R. 2009. *Public goods transfers and national unity: evidence from post-Soeharto Indonesia*. Presented at Annu. Meet. Am. Polit. Sci. Assoc., Toronto, Sep. 3–6

Treisman D. 1996. The politics of intergovernmental transfers in post-Soviet Russia. *Br. J. Polit. Sci.* 26(3):299–335

Tufte ER. 1980. *Political Control of the Economy*. Princeton, NJ: Princeton Univ. Press

Vaishnav M, Sircar N. 2010. *The politics of pork: building schools and rewarding voters in Tamil Nadu*. Unpublished paper, Dep. Polit. Sci., Columbia Univ.

Ward H, John P. 1999. Targeting benefits for electoral gain: constituency marginality and the distribution of grants to English local authorities. *Polit. Stud.* 47:32–52

Weingast BR, Shepsle KA, Johnsen C. 1981. The political economy of benefits and costs: a neoclassical approach to distributive politics. *J. Polit. Econ.* 89(4):642–64

Zucco C. 2010. *Cash-transfers and voting behavior: electoral impacts of Brazil's Bolsa Famíla program*. Unpublished paper, Dep. Polit. Sci., Rutgers Univ.