Politicians, Patrons, and the People:

Influences on Targeted Government Redistribution in Pakistan

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Abstract

Powerful individuals often influence the delivery of government services for their own purposes. Officials may prefer inherently to direct assistance to their own relatives and social contacts (nepotistic preferences). Alternatively, they may use government services strategically in exchange for favors (patronage) or to gain voter support (clientelism). Most existing literature examines these three phenomena separately, or does not distinguish the motivations for politicians' influence on distribution. Causal identification has also been a problem in the empirical literature. In the first chapter of this thesis, I develop a theoretical model of interaction between three levels of actors: politicians, local patrons, and households. The model allows for politicians and patrons to influence government services for nepotistic, clientelistic, and patronage purposes. In chapters 2-4, I test the predictions of the model using two novel household survey datasets I collected along with my collaborators in rural Punjab, Pakistan. Chapter 2 tests the theoretical predictions for the interaction of politicians, patrons and voters. Chapters 3 and 4 provide quasi-experimental evidence on the causal effect of links with politicians on assistance. I find evidence that politicians exert dramatic influence on the targeting of government assistance in this setting. Consistent with the theoretical model, the most assistance goes to a small "inner circle" of their closest contacts. Politicians assist this "inner circle" based on their inherent preferences, regardless of electoral pressure. When politicians face electoral pressure, they also deliver assistance to a wider group, in particular members of the same clan. In contrast, local patrons do not appear to have significant independent influence over the targeting of the government assistance programs I study, but they do provide other types of assistance to households. Their behavior is more consistent with the idea that they are motivated by inherent preferences for assisting their contacts. The results have implications for the interpretation of empirical literature on nepotism, clientelism, and patronage. They can also inform the policies of donor agencies and civil society organizations who aim to engage or pressure governments to reduce corruption and improve public spending.

Politicians, Patrons, and the People

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Abstract

Powerful individuals often influence the delivery of government services for their own purposes. Officials may use government services strategically in exchange for favors (patronage) or to gain voter support (clientelism). Alternatively, they may simplify prefer to direct assistance to their own relatives and social contacts, due to group-specific altruism, warm glow, or social pressure; in other words, they may have nepotistic preferences. Most existing literature examines these three phenomena separately, which makes it difficult to distinguish between them and understand how they may interact. In this chapter, I develop a theoretical model of interaction between three levels of actors: politicians, local patrons, and voters. The model allows for politicians and patrons to influence government services for nepotistic, clientelistic, and patronage purposes. For politicians, the pattern of assistance differs depending on whether social connections matter because of personal preferences, or because politicians find it easier to build vote blocs through one's social connections. A politician with a personal, nepotistic preference to assist his contacts distributes assistance to a small inner circle, regardless of electoral competition. In contrast, politicians facing close electoral competition will use government assistance to buy the allegiance of a larger group of voters (clientelism). The difficulty of striking a bargain with voters also affects the range of clientelistic transfers. If buying voter allegiance is costly overall, politicians will focus on forming alliances with their close contacts. Unconnected voters may be left out. In contrast, if voters form alliances readily, the two politicians compete over unconnected voters, and these voters receive the most benefits. Politicians with both motivations may engage in both behaviors (nepotism to a small inner circle, and clientelism to a broader group) at once. Politicians deliver assistance through local patrons. These local patrons in turn distribute assistance for their own benefit. They may assist their own connections out of nepotistic preferences, or they may exchange assistance for free labor from voters (patronage). The model predicts that a nepotistic patron assists his close connections without demanding assistance in return. In contrast, a patron who exchanges assistance for free labor may interact more intensively with his connections because it is easier to work with them. This results in the patron's connections both giving and receiving more assistance. The results have implications for the interpretation of empirical literature on nepotism, clientelism, and patronage.

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1 Introduction

Powerful individuals such as politicians, officials, or elites with informal power often influence the delivery of government services for their own purposes. They may prefer inherently to direct assistance to their own relatives and social contacts (nepotism). These preferences may be driven by in-group altruism, warm glow, or social pressure. Alternatively, they may use government services strategically in exchange for favors such as unpaid work (patronage), or to gain voter support (clientelism).

Understanding why, how, and how much this influence takes place is important to understand its welfare implications, and whether it can be mitigated. However, there are two main gaps in the literature on these issues. First, most existing literature examines these three phenomena (nepotism, patronage, and clientelism) separately. Some of the theoretical literature and most of the empirical literature does not distinguish between different motivations for this interference. Yet these different motivations can result in very different patterns of outcomes.

Second, the behavior of politicians and that of local elites are generally analyzed separately and in isolation from one another. Yet qualitative evidence suggests that their interaction may be important (Scott, 1972).

This chapter helps to address these two gaps in the literature. I develop a theoretical model of interaction between politicians, local patrons, and voters that allows them to influence targeting of government benefits for both nepotistic and instrumental purposes.

The model allows a politician to have nepotistic preferences, which are satisfied by giving assistance to his connections, regardless of whether this helps him get elected. Such a politician will offer as much as possible to a small inner circle of connections. He offers the same amount to this inner circle, regardless of whether he faces an electoral challenge.

In contrast, politicians facing close electoral competition will use government assistance to buy the allegiance of a larger group of voters (clientelism). If buying voter allegiance is costly overall, politicians will focus on forming alliances with their close contacts. Unconnected voters may be left out, receiving nothing. In contrast, if voters form alliances readily, the two politicians compete over unconnected voters, and these voters receive the most benefits. The model predicts that if rents from office increase, politicians will compete more intensively over unconnected voters and they will receive more transfers.

Politicians with both motivations may engage in both behaviors (nepotism to a small inner circle, and clientelism to a broader group) at once.

The second level of the model examines the interaction between local patrons and voters. The model allows local patrons to offer assistance to voters (clients) in exchange for services (such as providing free labor to the patron). The patron may either act independently, using his own resources, or in alliance with a successful politician. Politicians may be unable to credibly commit assistance to voters in advance; patrons may solve this problem, because their direct interaction with the politician allows them to hold him accountable.

In return for delivering votes to the politician, the patron gets to act as gatekeeper for the government assistance that the politician delivers. The patron uses this role for his own objectives, which may also be inherently nepotistic or instrumental. Nepotistic (in-group) altruism will lead local patrons to give assistance to voters with whom they have a close social connection, asking nothing in return. In contrast, the patron will demand unpaid work from unconnected voters, threatening them with punishment if they do not comply. Conversely, if the mechanism is ease of interaction with social contacts, the patron uses assistance to incentivize connected voters to work for him. Improvements in the market options available to voters will cause an altruistic patron to allow his voters to work on the market more, rather than spending the time doing services for him.

To my knowledge, this chapter is the first paper to model politician behavior when politicians have both electoral and personal motivations; it is also the first to present a formal model of interaction between politicians and local patrons who have both types of motivations. The results have implications for how the empirical evidence on clientelism, nepotism, and patronage are interpreted.

The rest of the chapter proceeds as follows. Section 2 reviews several areas of relevant literature. Section 3 lays out the theoretical model; Subsection 3.1 examines the role of politicians, while Subsection 3.2 introduces the role of local patrons and the interaction of the two. Section 4 concludes and discusses directions for future research.

2 Literature

There are three areas of literature that are particularly relevant to this chapter. The first consists of voting models, which are relevant to the top level of our model. The second area includes papers that analyze how officeholders or others in positions of power influence the distribution of public resources for personal reasons. The third area includes analysis of the exchange relationship between local patrons and clients.

Two alternative voting models form a foundation for the literature on clientelism. Lindbeck and Weibull (1987) and Dixit and Londregan (1996) argue that politicians will focus on median or swing voters in their choice of policies, while Cox and McCubbins (1986) argue that they will focus on their core supporters. The empirical literature on this debate has been inconclusive.

Several authors have proposed theoretical frameworks to unify these two approaches. Nichter (2008) argues that with clientelistic transfers, parties are buying not voter allegiance, but rather the turnout of their passive supporters. In contrast Vaishnav and Sircar (2012) argue that parties inherently prefer to spend resources on their core constituencies (perhaps as part of maintaining them in the longer term, although this is not modeled explicitly). However, parties spend resources on swing constituencies when elections are close.

This chapter provides an alternative approach to bridging the gap between the core and swing theories. In some circumstances, the model predicts a combination of spending on the politician's closest connections, who are analogous to the "core" in these models, and the unconnected central voters, who are analogous to the "swing". However, my model makes explicit who the "core" are and why they form the core. This is based on personal connections, rather than ideology, which is relevant for many developing democracies.

Several papers taking other approaches to voting models are also relevant. Bardhan and Mookherjee (2012) develop a rich model of voting and government goods distribution which allows for both clientelism and capture through campaign finance. My model is distinct from theirs in several ways. First, they allow for politicians to obtain the votes of groups of voters with clientelistic transfers. However, they do not explore the basis on which these groups are selected. My model allows clientelism to be more costly for different groups of voters, and explores the consequences. Secondly, their politicians have only political motivations, not personal ones; their only objective is to win the election. My

model explores the personal motivations of politicians and local patrons.

Besley, Pande, and Rao (2011) analyze the role of elected local council members in South India in determining both between- and within-village allocation of government services and assistance. They present a theoretical model of coalition building between villages, in which a minimum winning coalition of villages agree to support the council member from one village to become head of the council. In return, the supporting coalition of villages receive a greater share of resources. Each council member is assumed to prefer to bring resources to his own village; the model does not specify whether this is due to nepotistic preferences, for electoral reasons, or both. They then discuss several existing political economy models in which two categories of factors influence *within-village* distribution of assistance: the councillors' own preferences (which could be nepotistic, e.g. a preference for delivering assistance to their relatives; policy-based, such as a preference for assisting the poor; or both); and electoral constraints, the exact nature of which are not specified. This chapter differs from their work in two important ways. First, my model makes the underlying motivations of all the actors more explicit: I allow politicians to influence the delivery of government assistance based on a combination of personal and political preferences. I allow local patrons to deliver assistance in exchange for unpaid work that households do for them, and/or because they have an altruistic preference for helping their social connections. Second, Besley et al. study the behavior of local elected officials in a decentralized system of elected local councils which have explicit power in determining targeting. Like much of the literature on this topic, they motivate their analysis in part with the rise of decentralization in many countries. In contrast, I argue that local influential patrons may well influence resource distribution even when targeting is centralized and they are not assigned any official role. My model allows local influentials to play a role as gatekeepers for assistance directed by politicians who cover large constituencies; in return they help to deliver the local vote bank to the politician.

Several key papers in the political economy literature examine the role of intermediaries between the political party and the voters.

Camp (2012) develops a theoretical model of bargaining between a party leader and its political brokers. The brokers organize voters with whom they have personal relationships to support a party, while bringing government resources to their voters. The innovation of this model is that brokers are independent agents who aim to maximize the resources available to their own group of voters, rather than working for the party's overall interests. In this respect they play a similar role to the patrons in my model. There are three main differences of importance in my work. First, in Camp's model,

the motivations of the political actors are purely clientelistic. Political party leaders use the transfer of resources to maximize their chances of winning, and they have no personal preference to help their own connections. In addition, political parties have a set amount of resources to distribute; there is no personal corruption in this model. In contrast, my model incorporates behavior which can be based on all three motivations. Secondly, in Camp's model, the brokers want to maximize the government resources available for their voters, which the author motivates by arguing this represents an increase in the broker's power. There is no analysis of how resources are distributed within the group. In contrast, I analyze the motivations of local patrons who decide how to distribute government resources within their group on the basis of two motivations: personal benefit through work the households do in exchange for access to the government services, and a preference for assisting their social connections.

Keefer and Vlaicu (2007) develop a two-level model in which politicians may organize voters either directly or through patrons. Committing credibly to their pre-election promises is costly, but working through patrons reduces this cost. The patrons in their model have a social relationship with the voters and can deliver their votes to the politician. However, the influence of this personal relationship on the distribution of transfers within the group is not explored. Instead, patrons benefit by extracting part of the transfers delivered to their group for their personal use. My model builds on their work by exploring how the link between politician and patron influences the relationship between patron and voter. In addition, their model does not allow for social proximity between the politician and patrons or voters. As a result, opposing candidates compete over and deliver transfers to the same winning coalition of groups. Their model therefore cannot explain politicians who influence the delivery of transfers towards their own social groups and away from their opponents' groups. My model, in contrast, predicts transfers along social or ethnic lines.

Stokes, Dunning, Nazareno, and Brusco (2013) argue that local party brokers may distribute assistance according to their own preferences. Linking this argument to the core-swing debate, they argue that while national parties focus resources on swing constituencies, local brokers spend resources on loyal supporters. This is because the brokers can observe more easily to verify supporters' loyalty. However, the motivations are again ultimately electoral rather than personal.

Several papers do examine the role of social links, such as kinship or caste, in determining transfers. Burgess, Miguel, Jedwab, and Morjaria (2013) develop a model in which a president wants to raise the tax rate (which applies to all groups) to increase his rents from office. He gives transfers to his own ethnic group so that he can maintain their support. In this model, the president's own ethnic group

supports him strategically to maximize their net transfers; but other ethnic groups do not support the president by construction. The reasons for this are not made explicit.

Some base the premise of assistance to one's own ethnic group on the ease of building electoral coalitions within social groups or electing candidates that the group can trust. Bueno de Mesquita, Smith, Siverson, and Morrow (2003) argue that "affinity" between politicians and voters makes it easier to form coalitions. Munshi and Rosenzweig (2013) argue that in India, sub-caste helps overcome the difficulty of politicians to make a credible commitment. Members of a sub-caste can hold a politician accountable. This allows them to elect a more competent representative, yet still trust him to represent their preferences, even if they do not reflect overall preferences of the group.

However, it is also possible that more personal preferences, such as a pure preference for the welfare of one's connections (group-specific altruism), a warm glow from giving to them (group-specific warm glow), or social pressure, play a role.

Finally, a separate literature examines patronage relationships, in which households exchange assistance with a patron who has greater social and economic power than they do (Eswaran and Kotwal, 1985, Platteau, 1995). One prediction from this literature is that these relationships are stronger where outside market options are weaker, and vice versa. My model explores this. However, the patrons in Eswaran and Kotwal (1985) and Platteau (1995) provide assistance from their own economic and social resources. These papers do not focus on the role patrons may have as gatekeepers of assistance from the state. Scott (1972) discusses patrons' use of public office as a source of assistance, including targeted individual assistance. He describes political parties bargaining over local patrons who will deliver their whole bloc. Patrons benefit from their role as gatekeepers for this assistance. My model formalizes some aspects of his qualitative argument.

My model brings together several elements from across these literatures. By allowing agents to have both political and personal motivations for assisting their contacts, and allowing politicians and patrons to interact to bring assistance to their beneficiaries, it allows for new insight into political and elite influences on targeting.

3 Model

3.1 Politicians

3.1.1 Setup

Players Two politicians, $j \in \{0, 1\}$.

$i = 1, ...N$ voters. Each voter is located at point $i \in (0, 1)$.

Voter i's social distance from a candidate is $\psi_{ij} = j - i$.

Decision space For each i, the politician chooses an amount of assistance to offer, $0 \leq g_i \leq 1$. g are promises of assistance if he wins; so his total payout g is $\sum_i g_i$ if he wins in the election; 0 otherwise.

Voters can choose to ally with only one politician, so $a_{ij} = 1$ if voter i allies with politician j and 0 otherwise. To form an alliance, they incur some cost, and so require a minimum offer of g, $\underline{g}$. A voter who is allied turns out to vote $v_i = 1$ with exogenously fixed probability p. Voters cannot observe and react to other voters' alliances, or to the politicians' offers of assistance to other voters $g_{-i,j}$.

I do not explicitly model voter defection, detection and punishment. However, the voter has nothing to gain from defecting to the opponent in this setup. Once a voter makes an alliance with one politician, voting for the opponent will give him no benefits.

Payoffs Politicians want to hold office for some benefit R, which can be the intrinsic benefit of power or direct rents they earn in office.

If the politician wins office, he incurs a cost for providing g to voters, $c(g_i)$. A part of this cost is due to the difficulty of establishing a credible commitment; as in Keefer and Vlaicu (2007), this could entail repeated interactions with constituents to establish trust. This cost is smaller for voters who are more socially proximate to the politician:

$$c(g_{ij}) = (\alpha\psi_{ij} + \beta)g_{ij} \tag{1}$$

Where $\alpha \geq 0$ and $\beta > 0$. If $\alpha = 0$, social connections make no difference to the difficulty of commitment and coordination between politician and voter.

Politicians may experience some personal benefit from assisting their constituents, particularly for those who are socially closest to them:

$$b(g_{ij}) = \frac{\omega g_{ij}}{\psi_{ij}^{\gamma}} \tag{2}$$

ω is the strength of this preference overall. If $\omega = 0$, the politician does not have any personal motivation to assist any of his constituents.

γ represents the intensity with which the politician prefers to assist those who are closer to him. If $\gamma = 0$, the politician experiences equal personal benefit from assisting any voter.

The (risk-neutral) politician wants to maximize his expected utility:

$$\max U(g_{ij}) = \Pr(\text{j wins})(R + \sum_i (b_{ij} - c_{ij})) \tag{3}$$

$$= \Pr(\text{j wins})(R + \sum_i (\frac{\omega g_{ij}}{\psi_{ij}^{\gamma}} - (\alpha\psi + \beta)g_{ij})) \tag{4}$$

Voters have no intrinsic preference between the candidates and simply want to maximize their expected government goods received, g. This is motivated by many developing democracy settings where political parties do not have well-developed platforms and campaigning is primarily clientelistic (Wantchekon, 2003). The voters seek to maximize their expected utility:

$$\max V(a_{ij}) = E(g_i) = \sum_j \Pr(\text{j wins}) a_{ij}(g_{ij} - \underline{g}) \tag{5}$$

Note that since voters cannot observe other voters' behavior, if the candidates are symmetric their expectation of the probability of candidate j winning is 0.5. So this expression becomes:

$$\max V(a_{ij}) = \frac{1}{2}\sum_j a_{ij}(g_{ij} - \underline{g}) \tag{6}$$

So the voters will simply choose to ally with whichever politician offers a higher g_{ij}. If neither candidate offers at least $\underline{g}$, the voter will be better off by remaining unaligned.

3.1.2 Equilibrium play

To determine his offer of g to a particular voter, the politician must weigh the electoral benefit against the net cost $c - b$. To find a Nash equilibrium, consider politician j's decision when he has a given share A of allied voters, $\sum_i a_{ij}$, and his expected vote share is $\sum_i v_i a_{ij}$. Denote his probability of winning conditional on his *existing* alliances as $P(A)$. He considers whether to ally with an additional voter at i^+. This will increase j's probability of winning by $Q(A)$. Conditional on A, Q and P are both constants.

Note that allying with i^+ has the same effect on his probability of winning, regardless of where i^+ lies on the continuum. So to decide whether to ally with an additional voter, he only has to consider the cost $c(g) - b(g)$ required.

The politician will make the new alliance if and only if it improves his expected utility:

$$P(R - \sum_{i\in A} a_{ij}(c_{ij} - b_{ij})) \leq (P+Q)(R - \sum_{i\in A} a_{ij}(c_{ij} - b_{ij}) - c(g_{i^+j}) + b(g_{i^+j}))$$

rearranging:

$$c(g_{i^*j}) - b(g_{i^+j}) \leq \frac{Q}{P+Q}(R - \sum_{i\in A} a_{ij}(c_{ij} - b_{ij})) \tag{7}$$

Call the expression on the right hand side of equation 7 M; conditional on a given existing share of alliances A, M is a constant. So the politician can improve his utility by allying with i^+ if he can do so for $c - b \leq M$.

Because c and b are functions of i, for different points on the continuum, M will buy different amounts of g at different points. The voter will switch alliances if offered more than what the opponent is offering, $g_{ij} \geq g_{i,-j}$. The politician will have a profitable deviation if he can offer the voter more g

than his opponent for $c - b \leq M$. The inverse function of $c(g) - b(g) = M$ is the highest level of g at which the politician can benefit by forming an alliance with this voter. I call this g^*, the politician's "willingness to pay" for this additional voter, conditional on the alliances the politician already holds. If $c - b > 0$, this willingness to pay is:

$$g^* = \frac{M}{(\alpha\psi_i + \beta) - \frac{\omega}{\psi_i^\gamma}} \tag{8}$$

Notice that g^* is always positive. It also changes with R:

$$\frac{\delta g^*}{\delta R} = \frac{Q}{(P+Q)(\alpha\psi_{ij} + \beta - \frac{\omega}{\psi_{ij}})} \tag{9}$$

As the total rent to office increases, the willingness to pay for each voter increases. This effect is larger for a lower value of ψ, i.e. closer social contacts.

3.1.3 Nepotistic warm glow

First consider the case where connections matter for nepotistic warm glow b, but not for coordination costs c. Let $\alpha = 0$, $\omega > 0$, and $\gamma = 1$.

Figure 1 shows the net marginal cost, $b(g) - c(g)$, faced by politician 0 for voters at different points along the continuum, conditional on winning office.

Allying with additional voters has two effects: it increases the probability of winning, Pr(j wins), but it also increases the payout of g conditional on winning and the net cost $c - b$.

Note that there is an "inner circle" of connections for whom $c - b < 0$. The politician gets a net benefit directly from delivering assistance to them, besides the electoral benefit of winning their votes. The inner circle is defined by:

$$c - b \leq 0$$

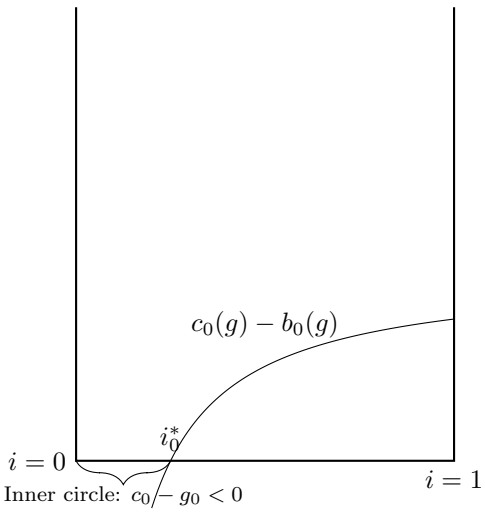


Figure 1: Direct net cost for politician 0 for $g_i = 1$ to different constituents i; case with social connections affecting warm glow but not coordination costs; $\alpha = 0$, $\beta = .5$, $\omega = \frac{1}{8}$, $\gamma = 1$

$$\alpha\psi_i + \beta \leq \frac{\omega}{\psi^\gamma} \tag{10}$$

As $\psi \to 0$, i.e. the politician's closest relatives and contacts, the expression on the right hand side of inequality 10 approaches infinity. Let i_j^* be the point where the two sides of 10 are equal. So the "inner circle" is in $(0, i_j^*]$. Note that with $\omega > 0$ and this functional form for b, there will always be an domain for this "inner circle", where $c - b$ is negative. Intuitively, one could think of an immediate family member as falling in this category, for example.

If elected, the politician will always give this inner circle the maximum possible assistance, $g = 1$, because for this inner circle, the net cost of providing g is actually *negative*.

Figure 2 shows g^*, the "willingness to pay" function for each voter on the continuum. Voters that are closest to politician j, with lower ψ_{ij}, can be offered g at the lowest cost to him. Conversely, his opponent faces a high cost to offer those voters a given amount of g.

With symmetric opponents, a symmetric Nash equilibrium exists in which:

- Each politician j allies with all voters between $(j, 0.5)$, so each politician has a probability of winning of 50%;
- Each politician offers his inner circle the maximum possible ($g = 1$);

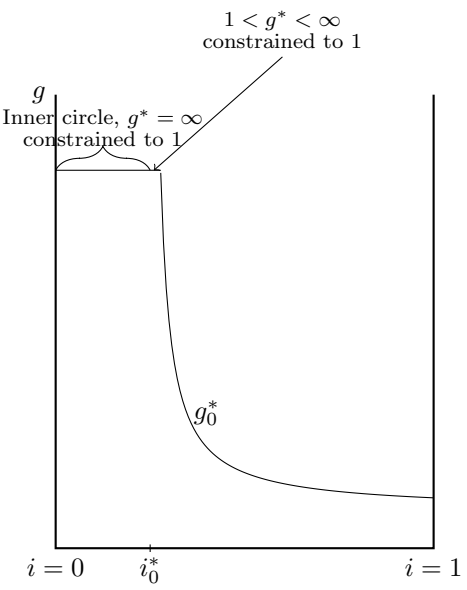


Figure 2: Politician 0's willingness to pay g for additional voters; case with social connections affecting warm glow but not coordination costs; $M = .05$; $\alpha = 0$, $\beta = .5$, $\omega = \frac{1}{8}$, $\gamma = 1$.

- If $g_{ij}^* < \underline{g}$, the politician does not offer any alliance to i.
- If $g_{ij}^* \geq \underline{g}$, then politician j offers other voters the level of g which corresponds to his *opponent's* willingness to pay those voters, i.e. $g_{i,-j}^*$, or $\underline{g}$, whichever is greater.

To see this, first note that in the range $(j, 0.5)$, $g_j^* > g_{-j}^*$.

Now consider whether either candidate has any profitable deviations from this position:

- j will not decrease his offer to any voter in the "inner circle," because he experiences a net benefit (negative cost) of paying them g due to their close social proximity to him.
- j will not increase his offer to any voter in $(j, 0.5)$, because they are already allied with him, hence an increase will result in his paying more without increasing his utility.
- j will not increase his offer to any voter between $(0.5, -j)$, because his opponent is offering these voters g_j^*, so he cannot outbid his opponent without paying more than these voters are worth to him, making him worse off.
- If j decreases his offer to any voter between $(j, 0.5)$ to $g < g_{-j}^*$, then his opponent $-j$ will outbid him and the voters will defect to the opponent. Since $g_j^* > g_{-j}^*$ for these voters, this will make him worse off.

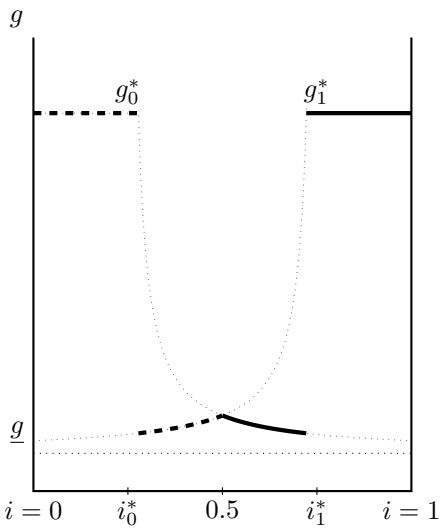


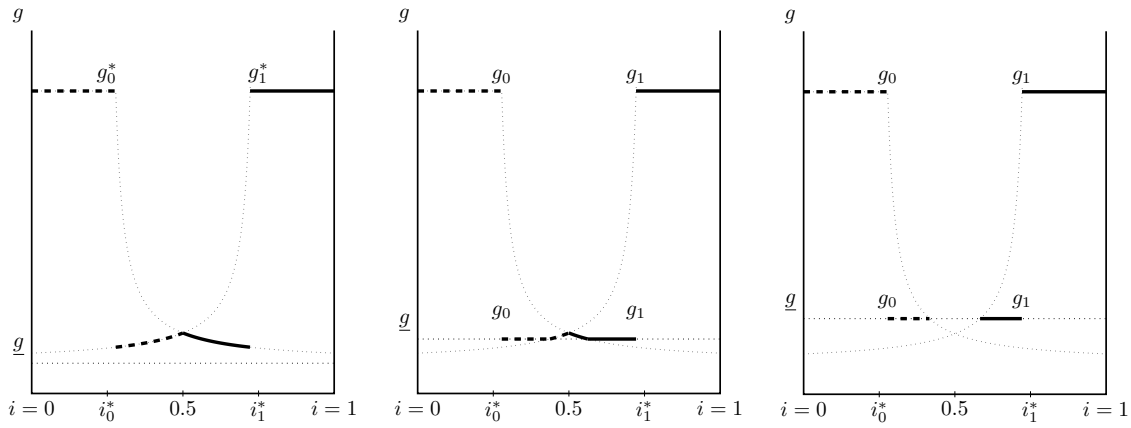
Figure 3: g offered in symmetric Nash equilibrium; case with social connections affecting warm glow but not coordination costs; $\alpha = 0$, $\beta = .5$, $\omega = \frac{1}{8}$, $\gamma = 1$, $\underline{g} = 0.1$. Light dotted lines show g^*, the politicians' "willingness to pay". Thick dashed lines show politician 0's offer of g in equilibrium. Thick solid lines show politician 1's offer of g in equilibrium.

Note that R is not exhausted with payouts of g in this equilibrium; the politician j pays $g_j < g_j^*$ for many voters. (This is possible because the politician has a kind of local market power over his closer connections.)

Figures 4a - 4c show the possible outcomes in this equilibrium. Figure 4a shows the case where $\underline{g}$ is not binding for any voters, while Figures 4b and 4c show the case where $\underline{g}$ binds for some voters. In both cases, this results in more g for the voters who are close to the politician but not in his inner circle. In Figure 4b, $\underline{g}$ is so high that the voters in the middle are too "expensive" for either politician. These voters no longer ally with either politician.

Because g^* is a function of R, changes in R can also produce these different equilibria (see Equation 9). If rents from holding office are decreased, this decreases g^* for all voters outside the "inner circle". This may push g^* below $\underline{g}$ for more voters. This could result in a change from the equilibrium in Figure 4b to Figure 4c, i.e. politicians drop alliances with socially distant voters who are no longer worth the trouble.

Since g^* can never be negative, if $\underline{g} = 0$, all voters in $(j, 0.5)$ will be offered some g.



(a) $\underline{g} = 0.1$, not binding (b) $\underline{g} = 0.18$, partially binding. (c) $\underline{g} = 0.25$, fully binding

Figure 4: g offered in symmetric Nash equilibrium with $\underline{g}$ fully binding. Case with social connections affecting warm glow but not coordination costs. $\alpha = 0$, $\beta = .5$, $\omega = \frac{1}{8}$, $\gamma = 1$. Panels show equilibria with different levels of $\underline{g}$.

3.1.4 Cost of commitment

Now consider the case in which the social link ψ affects commitment cost c, but there is no warm glow b. Let $\alpha > 0$, $\omega = 0$. Now the net cost $c - b$ is always positive, so there is no "inner circle."

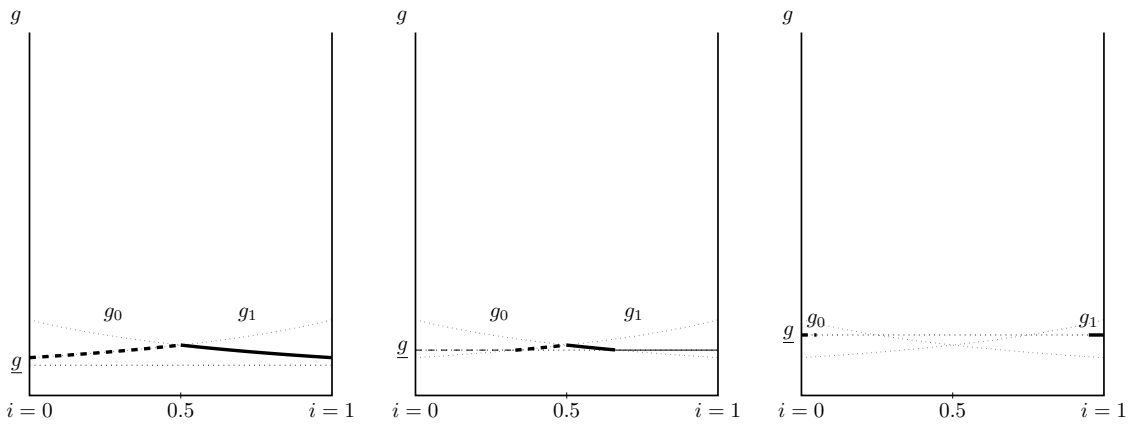
Otherwise the equilibrium is the same, so the median voters, who are socially distant from both candidates, will either receive the most g or no g at all, depending on $\underline{g}$, as shown in Figures 5a - 5c.

3.1.5 Warm glow and commitment costs

Now consider the case where both warm glow and commitment costs exist.

Warm glow not dependent on ψ Let $\gamma = 0$. Then $b = \omega g$. The politician benefits from warm glow regardless of his social link with the voter ψ. In this case, an "inner circle" can still exist where $c - b < 0$. This will only occur if $\omega > \beta$.

Warm glow dependent on ψ Let $\gamma > 0$. The politician receives greater warm glow from assisting voters to whom he has a closer link (lower ψ). In this case, there is always a domain in which costs are negative and there is an "inner circle". The results will be qualitatively similar to those in Figure



(a) $\underline{g} = .1$ (b) $\underline{g} = .15$ (c) $\underline{g} = .2$

Figure 5: g offered in symmetric Nash equilibrium with coordination costs and no warm glow, $\underline{g}$ fully binding. $\omega = 0$, $\alpha = 0.2$, $\beta = 0.2$, $M = 0.05$. Panels show equilibria with different values of $\underline{g}$.

4.

3.1.6 The case with warm glow but without political competition

So far the politicians have been symmetric. Consider the opposite case. If a politician has a complete hold over the constituency and effectively runs unopposed, only the "inner circle" effect holds, and unconnected voters will receive nothing. If there is no warm glow, no voters will receive anything.

3.1.7 Predictions

Prediction 1. *Central voters - those close to* 0.5 *- will either be completely unaligned and receive no g, or they will be highly contested and receive more g than any voters other than the "inner circle".*

Prediction 2. *If voters have a high cost of alliance* $\underline{g}$*, it is more likely that central voters will be unaligned with any politician and receive no g.*

Prediction 3. *As R increases, g to voters in* $(i_j^*, 0.5)$ *will increase. The effect is strongest for voters with the lowest* ψ*, i.e. the closest connection to the politician. Alliances may be created with less connected voters where they did not exist before.*

Prediction 4. *If the politician has any "warm glow" preferences to assist those close to him, he offers the maximum possible assistance to his "inner circle",* $g_{i,j} = 1 \forall i \in (j, i_j^*]$.

Prediction 5. *If the politician faces political competition, he will offer assistance to unattached voters who are neither among his own close connections nor those of his opponent.* $g_{i,j} > 0 \forall i \in (i_0^*, i_1^*)$.

Prediction 6. *An increase in R increases* g^* *for those outside the inner circle:* $\frac{\delta g_{ij}^*}{\delta R} > 0 \forall i \in (i_j^*, 0.5)$. *This increases g bids to all voters who previously received bids in* $(\underline{g}, 1)$*. It also decreases the range in which* $g^* < \underline{g}$*, thus creating new alliances where previously there were none. So, if politicians value power more, or if they can gain more rent from office, they will compete more intensely over median voters.*

3.2 Voter blocs and patrons

Now consider the case where the continuum of voters is composed of K discrete, exogenously formed blocs, such as villages or clans. Bloc k falls between i_{k-} and i_{k+}, so its size $s_k = i_{k+} - i_{k-}$ (so $\sum_K s_k = 1$) and number of voters is Ns_k. I assume that no bloc covers the majority of voters, $s_k < 0.5$. I again assume that the continuum of blocs is symmetric, but I do not need to assume that the blocs are of equal sizes.

Each bloc of voters is controlled by a single local patron, such as a landlord or clan leader. The patron can make an alliance for the whole bloc, but an individual voter cannot make an alliance. A bloc can be more easily monitored for compliance with the alliance (for example using polling-station wise voting data), and a patron could more easily hold the politician to account in delivering the promised services g, because repeated face-to-face interaction is easier (cf. Keefer and Vlaicu (2007), Munshi and Rosenzweig (2013)). The voters vote for the candidate with which their bloc allied, again with

probability p.

The politician can now only offer g to a bloc, not to individual voters. However, this makes it easier to make a credible commitment to more socially distant blocs. It reduces α.

The politician's social distance from a bloc is defined by distance to its midpoint, $\psi_{jk} = j - \frac{i_{k+} - i_{k-}}{2}$.

I define g_{jk} as the assistance level at each point i (like a per-person level of assistance), so total assistance to block k is $G_{jk} = N s_k g_{jk}$.

Politician j's cost and warm-glow benefit of providing assistance to the bloc is:

$$c(g_{jk}) = G_{jk}(\alpha\psi_{jk} + \beta)$$
$$b(g_{jk}) = \frac{G_{jk}}{\psi_{jk}^{\gamma}}$$

The patron will simply choose the politician to maximize the expected g offered to the bloc. Again with symmetric politicians, the patrons' expectation of the probability of each politician winning is 0.5. Each patron will simply choose to ally with the politician who offers a higher g to his bloc.

Equilibrium follows as in the case without voter blocs. If the direct warm glow benefit $b(g_{jk})$ exceeds the cost $c(g_{jk})$, that bloc falls in j's inner circle and will receive the maximum possible, $g = 1$ for each voter and total $G_{jk} = N s_k$.

For all other blocs, the politician will be willing to pay g to ally with bloc k^+ up to g^* such that:

$$\frac{Q}{P+Q}(R - \sum_k a_{jk}(c(g_{jk}) - b(g_{jk}))) \geq c(g_{jk^+}) - b(g_{jk^+})$$

Each politician j pays g^*_{-j}, the maximum willingness to pay of his opponent. Again, this results in either no g or high g for voters who are equidistant from the two politicians.

However, since working with patrons reduces coordination costs α, this will increase the size of the "inner circle," and decrease the domain of unallied central voters.

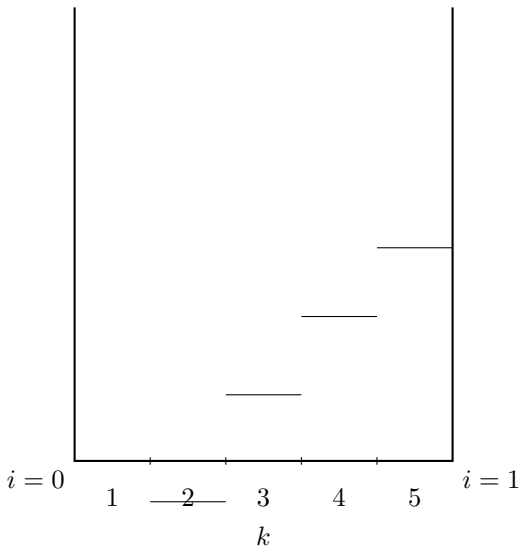


Figure 6: Direct net cost to politician 0 of providing g to voter blocs; case with both warm glow and interaction costs

3.2.1 Patrons' interaction with voters

Now I allow the patrons to act as independent agents. Patrons may provide assistance from their own resources. They may also form an alliance with a politician and deliver votes to him. If that politician wins, they then act as "gatekeepers" for the assistance he provides.

3.2.2 Politician-patron interaction

I assume the local patron allocates the g that the politician provides among individual voters in his bloc. He can offer g to voters in exchange for effort e in assisting him, $0 \leq e \leq 1$. Here the patron's social connections to the voters, $0 \leq \xi_{ik} \leq 1$, will play a role in the interaction. A higher ξ denotes a stronger connection. I do not impose any restriction on the distribution of ξ_{ik}, unlike with ψ_{ij} above. Note that ψ (the voter's relationship with the politician) does not determine ξ (the voter's relationship with the patron). As above, ψ is the distance to the midpoint of the bloc. This implies that if the patron is close to the politician, so is his community.

I show results under two different assumptions: that ξ affects the patron's concern for the voters' welfare (in-network or nepotistic altruism), or that it affects the ease of interaction between the patron and client.

Let the utility of the intermediary for bloc k be a combination of personal benefit from e and an altruistic concern for voters. Altruism is greater for voters with whom the intermediary has a greater social connection ξ_{ik}.

$$W_k = \sum_{i \in k} (\theta f(e_i) + \tau \xi_{ik} V_i) \tag{11}$$

I assume that $f(e)$ is monotonically increasing and that $\theta > 0$, $\tau \geq 0$.

The voter's utility is a function of the g he receives in exchange for e and of wages earned in the remainder of his time, $w(1-e)$:

$$V_i = w(1 - e_i) + g \tag{12}$$

Voting still happens in the same way as described above, because the patron controls the votes and maximizes the total g available to him for distribution.

After the election, the patron and the voter play a sequential game. First, the intermediary offers the voter a contract for g. The intermediary can tailor his offer of g to each individual:

$$g_i = m_i f(e_i) + n_i$$

However, he is also constrained by the amount of g he can offer to individuals by the limitations of the value of government programs and services available ($0 \leq g \leq 1$), and by the total amount of assistance he can give out. He can offer some amount of assistance using his own connections and resources, $\underline{G}_k$. If the patron allied with the winning politician, he also gets resources from the politician, G_{jk}. Total resources cannot exceed Ns_k, i.e. $g = 1$ per person.

$$0 \leq g_i \leq 1 \tag{13}$$

$$\sum_{i \in k} g_i \leq G_k \tag{14}$$

where

$$G_k = \min \underline{G}_k + G_{jk}(jwins)$$
$$Ns_k$$

(If bloc k is in the winning politician's "inner circle," the second constraint adds no additional information, because $G_{jk} = Ns_k$.)

Note that there are no constraints on n, which can be negative as long as g is still non-negative. In other words, the patron can induce a voter to work but offer him no g, by setting a high enough m and a negative n, which represents a threat of punishment if he does not work.

The voter then chooses a level of e and the benefits to both are realized.

In an interior solution, where $0 \leq e \leq 1$, the voter will choose e such that:

$$\frac{w}{m} = \frac{\delta f(e)}{\delta e}$$

Note that e does not depend on n. Note also that this is only efficient if $m = 1$.

3.2.3 Patron in-group (nepotistic) altruism

The patron has to choose m_i and n_i for each voter to maximize:

$$\begin{aligned}\max W &= \sum_i (\theta f(e_i) + \tau \xi_i V_i) \\ &= \sum_i (\theta f(e_i) + \tau \xi_i (w(1 - e_i) + m_i f(e_i) + n_i))\end{aligned}$$

Subject to constraints 13 - 14.

To illustrate a simple case, let $f(e_i) = \kappa_i e_i$. Then the voter's optimal choice is a corner solution:

$$e = 1 \text{ if } m\kappa_i > w$$

$$e = 0 \text{ if } m\kappa_i < w$$

Because e does not depend on n and n is not constrained, the patron can allocate G however he chooses (by setting a negative or positive n) and still induce the voters he chooses to work for him. So, the decisions of how to distribute g and what level of e to induce are separable. The patron simply optimizes by allocating g to his closest connections (highest ξ) first to maximize the altruism benefit, giving them the maximum possible ($g = 1$), then moving on to the next closest, and so on. So the G most well-connected patrons will all benefit at the maximum level. This echoes the "inner circle" result for the politician.

The patron induces the client to work for him by setting $m\kappa > w$ and inducing $e = 1$ iff:

$$\tau\xi(w+g) < \theta\kappa + \tau\xi g \tag{15}$$

$$\tau\xi_i w_i < \theta\kappa_i \tag{16}$$

The left hand side of inequality 15 represents the altruism effect: his overall level of altruism τ, his connection to the client ξ, and the benefit to the client of working on the market, w. The right hand side represents the self-interest effect: his overall level of self-interest θ and the client's productivity κ_i.

Note that well-connected voters (high ξ) are more likely to be allowed to exert no effort for the patron, $e = 0$. They may receive $g = 1$ or $g = 0$, depending on whether they are among the G most connected voters in the constituency.

Note also that an increase in the client's outside option w may induce the patron to encourage him to work on the market instead of for the patron - but only for those who have at least some connection to the patron ξ.

Finally, note that if $\tau = 0$, i.e. a totally selfish patron, or $\xi = 0$, i.e. a completely unconnected client, this inequality will *always* hold, and the patron will always induce all voters to work for him.

Note that warm glow will not produce the same pattern of behavior that altruism does. Altruistic patrons are more likely to send their clients to work on the market (i.e. not induce unpaid work e for them) because it will improve their well-being. If they are motivated instead by warm glow based on their own provision of g only, they will not benefit from the voters' economic wellbeing.

These results are not limited to the linear case with corner solutions. If $f(e) = \sqrt{e}$, the patron's optimal $m = \frac{\theta}{\tau\xi}$; again the patron induces less work from his social contacts, and less work if he is more altruistic.

3.2.4 Ease of interaction

An alternative reason that social connections between the intermediary and the voters could be important is due to ease of interaction between the two, meaning that the intermediary benefits more for

a given level of effort e. In this case the intermediary's problem is:

$$\max_{m_i,n_i} W = \sum_i \theta\xi_i f(e_i) + \tau V_i$$

subject to the same constraints 13 - 14. Note that ξ now enters the first term (benefit from voter effort), rather than the second term (altruism) as before.

Again let $f(e_i) = \kappa_i e_i$. Now the patron will induce $e = 1$ iff:

$$\tau w_i < \theta\kappa_i\xi_i \tag{17}$$

Notice that the effect of a social connection is now reversed: closer social connections are *more* likely to be induced to work for the patron, because their work for the patron is more productive.

Again if the patron is completely selfish ($\tau = 0$), he will induce all the voters to work for him ($e = 1$).

(Because this version of the model has no variation between voters in the altruism term (and client utility is still linear in g) this version of the model yields no predictions for how the patron will distribute G.)

3.2.5 Local patrons as independent providers of assistance

If local patrons act as independent providers of assistance, rather than acting as gatekeepers for the politician's assistance, then the patron's total endowment of G_k is fixed at the local level $\underline{G}_k$. This is the case for a non-allied patron. However, it may also be the case if politicians and patrons do not collaborate. In that case, voters would receive $g = g_k + g_j$, i.e. separate assistance from both the patron and the politician. Under a simple linear utility model, the amount the voter receives from the politician will not make a difference to the relationship between the patron and the voter. (If utility is concave in g, however, and the two types of g are substitutable, then patrons would have to offer

more g to induce voters to work for them.)

3.2.6 Predictions

Prediction 7. *If working with patrons reduces commitment cost for less connected voters α, then these alliances will increase g^* to voters near the median. This may result in new alliances formed among unconnected blocs of voters, and more government assistance to them.*

Prediction 8. *If the patron can act as a "gatekeeper" for the politician's assistance, assistance from the patron will be greater in communities where the patron has a closer social connection to the politician, $G_k = \underline{G}_k + G_{jk}$.*

Prediction 9. *If a patron is motivated by altruism towards his close contacts, he will offer his contacts more assistance in exchange for less effort. In the linear case, $g = 1$ and $e = 0$ for the closest connections $i \mid \xi_i > \frac{\theta\kappa_i}{\tau w_i}$.*

Prediction 10. *If a patron is motivated by easier interactions with his close contacts, he will demand greater effort from them. In the linear case, $e = 1$ for the closest connections, $i \mid \xi_i > \frac{\tau w_i}{\theta\kappa_i}$.*

Prediction 11. *An increase in the outside option of voters makes the patron more likely to encourage them to work on the market instead of for him. However, this only occurs if he is altruistic towards those voters. In the linear case with in-group altruism, $e = 0$ if $w_i \geq \frac{\theta\kappa_i}{\tau\xi_i}$, if $\tau > 0$ and $\xi_i > 0$. In the case of ease of interaction, $e = 0$ if $w_i \geq \frac{\theta\kappa_i\xi_i}{\tau}$.*

4 Conclusions

In this paper, I have presented a model which examines the role of two important types of agents influencing government assistance - politicians and local elite patrons. The model brings together both personal preferences and political motivations. This allows for nepotism, clientelism and patronage to co-exist and be interrelated.

The model predicts very different patterns of assistance depending on these motivations. In particular, politicians with inherent nepotistic preferences will provide high levels of assistance to an "inner circle" of contacts and relatives, regardless of electoral pressure.

To use transfers for clientelistic purposes, politicians may focus on either connected or unconnected

voters, depending on how costly it is to form an alliance in general. The model's predictions thus also provide a theoretical framework in which either core or swing voters could be targeted with clientelism, providing an alternative approach to this ongoing debate.

The model also adds to the literature on patronage, providing empirically testable predictions for patron behavior under alternative mechanisms. These can help interpret empirical evidence of own-group favoritism by these patrons. If patrons prefer their own group because of a nepotistic form of altruism, they will assist them while asking nothing in return. If on the other hand it is simply easier to work with members of their own group, they will compel group members to do more for them in return.

Potential directions for future work include incorporating limits to the patron's power to punish clients (n), and incorporating the option for households to obtain services through bribes, as an alternative of using the assistance of the patron.

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Politicians, Patrons, and Public Assistance: Evidence from Punjab, Pakistan

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Abstract

In this paper, we test the predictions of the model developed in Chapter 1 on a novel household survey dataset we collected in rural Punjab, Pakistan. The results provide clear support for a model in which politicians are motivated by both nepotistic personal motivations to assist their closest contacts, and clientelistic competition over less connected groups. The "inner circle" of the politician's closest contacts receive the most assistance, regardless of electoral pressure. Other members of his clan receive some assistance as well, but only when the politician faces a competitive election. The results also support the model of local patrons motivated by nepotistic preferences to help their relatives, and extracting unpaid labor from other households in their community. Co-clan members get more assistance from the local patron than non-members, despite doing less unpaid work for him. This finding is not consistent with the model in which exchange relationships are easier within the clan. These effects persist despite controlling for household observables and clan fixed effects. The results have implications for our understanding of the theoretical and empirical literature on clientelism and resource distribution. The results support an alternative approach to reconciling the core-swing debate in political economy. They also have implications for the interpretation of empirical literature on targeting of government assistance programs.

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1 Introduction

People in positions of power often influence the delivery of government services for their own purposes. They may also influence who receives government services for instrumental purposes, in exchange for favors (patronage) or to gain voter support (clientelism). Alternatively, politicians, officials or influentials may direct government assistance to beneficiaries whom they prefer intrinsically, such as their own ethnic group or clan, or social connections and family members. We use the phrase "nepotism" to denote assistance to one's clan, family members or social contacts due to these personal preferences.

Most existing literature examines these phenomena separately, or may not distinguish between different motivations. In addition, the behavior of politicians and that of local elites are also generally analyzed separately, although qualitative evidence suggests that their interaction may be important. The theoretical model in chapter 1 provides a framework for understanding how politicians and local elites may assist households based on their inherent preferences, for instrumental purposes, or both.

In this chapter, we test the predictions of the theoretical model on primary data representative of rural Punjab, Pakistan. We find evidence that politicians influence delivery of targeted assistance programs towards their own clan. The results provide clear support for a model in which politicians are motived by both nepotistic warm glow for their closest contacts, and clientelistic competition over less connected groups. The "inner circle" of the politician's closest contacts receive the most assistance, regardless of electoral pressure. Other members of his clan receive some assistance as well, but only when the politician faced a competitive election.

The results also indicate that local patrons direct this assistance disproportionately to their relatives: members of the politician's and patron's own clan are more likely to receive assistance. The local patrons' behavior is consistent with social connections influencing their decisions through nepotistic preferences (in-group altruism), rather than by ease of exchange. Households who are not connected to the local patron get less assistance despite doing more unpaid services for them.

These effects persist despite controlling for household observables and clan fixed effects. They are also robust to clustering standard errors at different levels.

The results have implications for our understanding of the theoretical and empirical literature on clientelism and resource distribution. The results support an alternative approach to reconciling the

core-swing debate, as discussed in Chapter 1. They also have implications for the interpretation of empirical literature on targeting of government assistance programs. For example, we may better understand an empirical finding on political influence on targeting if we distinguish between high value assistance to a very small group (the inner circle), which will not generalize beyond that group, and low-value assistance to a much larger group for clientelistic purposes. Our framework also formalizes an explanation for why political competition plays a role in determining some types of assistance from politicians and not others.

The rest of the paper proceeds as follows. Section 2 reviews key literature. Section 3 describes the institutional context and the data. In section 4 we review the predictions from the theoretical model in Chapter 1, and describe how we will test them empirically. Section 5 presents the empirical results, and Section 6 concludes and discusses directions for future research.

2 Literature

Chapter 1 gives a comprehensive review of theoretical literature. Here, we emphasize the relevant empirical work.

A number of papers estimate the value that households derive from social or family connections to officials. (This is distinct from most literature on corruption or elite capture, in which the official himself receives the assistance.) In general, however, this literature has struggled to distinguish between different possible motivations for officials to assist their contacts. Caeyers and Dercon (2012) find that households who are part of a risk-sharing group with local elected officials in Ethiopia receive food aid more quickly. The nature of these groups suggest that the officials could be motivated by the expectation of reciprocal assistance. However, group composition may also reflect other social or political ties, so it is difficult to rule out other motivations. Fafchamps and Labonne (2013) find that the relatives of winning politicians in the Philippines get better jobs, while losing politicians' relatives are punished with worse jobs than unconnected individuals. They argue that this effect is unlikely to be due simply to altruism, because it is stronger for more educated relatives. This may suggest that politicians help their relatives into good positions in order to have contacts they can rely on. However, this could arguably still be explained by altruism if the cost of getting an unqualified but well-connected social contact into a position is greater than for a qualified contact, which is plausible.

Besley, Pande, and Rao (2011) study the allocation of government assistance in South India. They find that when the local council head position is reserved for a member of disadvantaged caste groups, these groups are more likely to receive BPL cards. This suggests favoritism within caste, although it is not clear whether the motivations are electoral, nepotistic, or both.

Some literature has addressed the use of targeted assistance along ethnic or caste lines for electoral purposes. Wantchekon (2003) uses a novel field experiment in Benin to test the impact of clientelist versus public policy platforms. He finds that clientelist appeals are more successful at obtaining votes, and this effect is stronger with regional or ethnic candidates. He argues that this suggests ethnic solidarity reinforces clientelism. Experimental non-compliance was high in competitive districts, so the analysis is based only on the non-competitive districts. However, if voters perceive that their vote will not be pivotal, then there is more reason for them to vote only instrumentally, i.e. in a quid-pro-quo exchange of their vote for a clientelistic promise (cf. Bardhan and Mookherjee (2012)). If there is no such promise, then it is arguably questionable why a voter who expects not to be pivotal to the outcome should vote at all (Grofman, 1993). Conversely, it is not obvious why politicians should offer substantial promises of clientelistic assistance in districts where they face little competition. We argue that this makes it particularly important to study clientelism in a competitive-elections context.

Munshi and Rosenzweig (2013) argue that in India, sub-caste helps overcome the difficulty politicians have in making their commitments to voters credible. Without commitment, voters must seek to elect the representative whose preferences most closely match their own, because they know he will follow his own preferences (following the citizen-candidate model of Besley and Coate (1997), Osborne and Slivinski (1996)). Within a sub-caste, voters can hold a politician accountable; this allows them to elect a more competent representative, yet still trust him to represent their preferences, even if his preferences do not reflect those of the group. To test this theory, they exploit the rotating reservation system in local councils in India. They find that when the largest eligible subcaste is sufficiently large to form a coalition, the community is more likely to elect educated candidates with business experience. They focus on preferences on a policy such as public goods allocation, rather than individual targeting. However, their argument could logically lead to a politician targeting benefits to individual members of his own group for the same reasons.

The literature on patron-client relationships is also relevant for the second level of the model. A number of studies examine historical socio-political institutions set up by the British in colonial India which strengthened local elites. Banerjee, Iyer, and Somanathan (2005) and Cheema, Naqvi, Naseer,

and Siddiqi (2012) use historical natural experiments to provide causal evidence that these reduced public goods provision - an effect which persists today, despite the fact that these elites no longer have any official role. But these studies do not reveal exactly how and why these local elites may influence the targeting of government assistance, in particular within communities. The few quantitative papers that do examine these interactions in detail have significant limitations. For example, Joshi and Mason (2008) argue that the Maoist insurgency in Nepal disrupted a system of patronage and clientelism similar to the one we model in Chapter 1. They use voter turnout as an indicator of local patron power, and find that turnout is lower both in districts with higher political violence, and in districts with more households who have limited attachment to their landlord. However, they only have district level observations, and they do not exploit the panel nature of the data or the interaction between tenancy and violence. Shami (2010a,b) studies landlord-peasant dynamics in rural Punjab, Pakistan. She finds that after a new motorway was built, households nearby received more drains and paved roads - but only in villages where there was a strong local landlord. She argues that the motorway provided better outside options through market work opportunities, giving peasants stronger bargaining power vis-a-vis local patrons. However, the sample is small (380 households) and concentrated in just eight villages, and the author does not cluster standard errors. In addition, alternative mechanisms could explain the empirical results. Infrastructure may be cheaper to build and have a higher marginal benefit in the presence of a motorway connection. The market opportunities may simply have raised household income, making contributions towards collective building of infrastructure more affordable. Finally, Cheema and Mohmand (2006) study targeting of local drain construction in Pakistani Punjab, during a period of decentralization. They find that construction was targeted to the blocs of voters who had a strong relationship with the local elite patrons who voted with the union council nazim. They do not explore further the role of social connections including clan in bloc formation, or discuss how the patrons benefit from playing the role of broker. The small sample size and small number of clusters is also a concern in this study (see Cameron, Gelbach, and Miller (2008)).

This chapter builds on both these areas of literature. We test the predictions of the theoretical model in Chapter 1, providing evidence on which groups benefit from assistance from politicians and local patrons, and why. To our knowledge, it is the first paper to try to distinguish empirically between electoral, exchange-based and inherent preference-based motivations for officials' intereference in targeting. We believe it is also the first to test theories of exchange between local patrons and clients with a large-sample survey data set. We are able to control for clan fixed effects because of differences in officials' clan between villages and constituencies. This presents a significant improvement

in identification over the existing literature on local patron-client exchange relationships.

3 Context and data

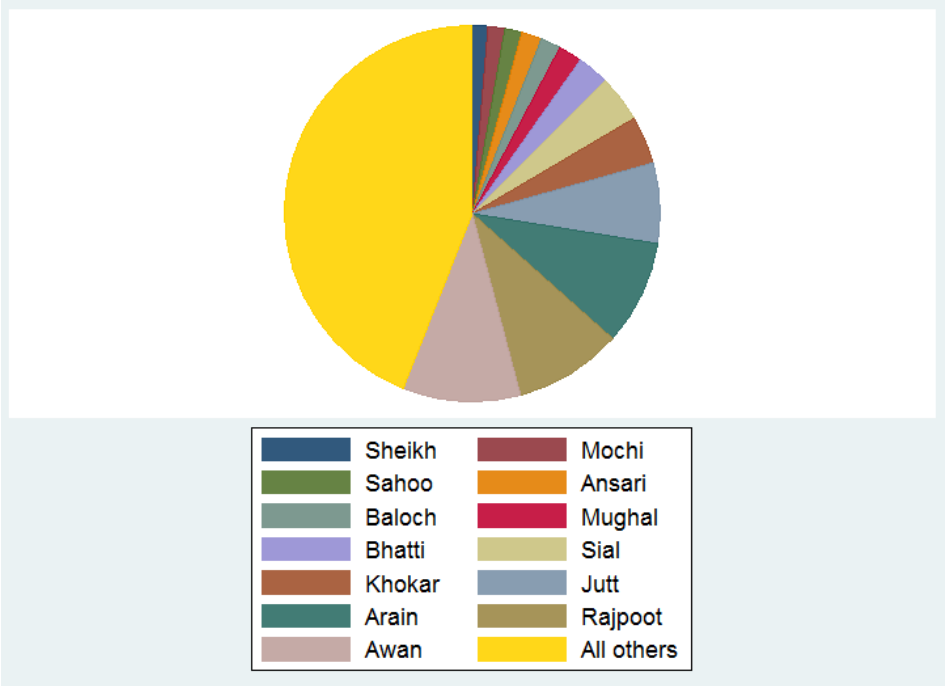
We test the predictions of our model using primary data from a survey of 924 rural households in 64 villages in 13 legislative constituencies; the sample is representative of Punjab, Pakistan[1]. Punjab is the largest province of Pakistan by population, with 80 million of the country's 180 million people. Approximately 50 million of Punjab's population live in rural areas. We collected these data in collaboration with colleagues at the Center for Research on Economics and Business at the Lahore School of Economics and the Punjab Bureau of Statistics. The sample spans 13 national legislative constituencies. The survey and dataset are described in greater detail in Ahmed, Amjad, Habib, and Shah (2013) and Chaudhry and Vyborny (2013).

The social link which we refer to as "clan" in this paper is called *zaat* or *biradari* in Urdu. It is an endogamous grouping similar to the subcaste in India. In Punjab in particular, which shares a common cultural and social heritage with Indian Punjab, many of these groups are the same as those in India. Islamic teaching rejects all such social distinctions, which may have altered the social role of biradari. Unlike in modern India, these social groups have no official recognition, and no group receives any official preferential treatment or discrimination on the basis of clan. However, it is clear that biradari is important in social interactions in rural Pakistan. It is still part of a hierarchical system in which biradaris fall into historical categories of landowning, artisan or menial groups which still have different social status today (see for example Ahmad (1977), Mohmand and Gazdar (2006), Gazdar and Mallah (2013), Cheema, Mohmand, and Patnam (2009), Cheema and Mohmand (2006), Jacoby and Mansuri (2011), Karachiwalla (2014)). Biradari is also highly salient in politics; newspapers routinely report results based on which biradaris supported which candidates after elections. In the survey, households were asked to report their clan. In general they did so readily, as these kinship groups are salient and publicly known in villages in Punjab. This question was asked at the end of the household survey, to minimize any response bias to other questions. Figure 1 shows the distribution of clans in the sample. The largest clan is only 11% of the sample; clans with fewer than 1.5% of the sample are grouped together in the "other" category.

Households were also asked whether they knew anyone who held one of a list of positions, including

[1]Excluding areas affected by floods the previous year.

Figure 1: **Distribution of clans in sample**



elected offices (such as members of the national or provincial assembly), unelected offices (such as senior district bureaucrats) and non-government positions (such as major landlords). All households identified at least one contact from this list, and many identified two or more. For each of these individuals, the household was then asked about their connections to the individual, the frequency and means of contact, and a number of questions about assistance from the household to the influential and vice versa. Finally, respondents were asked to identify the clan of the influential person.

The "numberdar" was the most commonly mentioned influential person. The position of numberdar was historically a local official administrative role, appointed by British officials and then hereditary thereafter. These officials were also empowered much later during devolution to the local government in the Musharraf era. After the Musharraf era the local government system was not officially maintained. However, these officials still maintained an important role in communities, and they were among the most frequently identified officials in our survey. The same individuals have stayed in this now unofficial role. In many cases it is de facto still a hereditary position.

In focus groups we carried out as a part of this project, the numberdar was also mentioned as being an official of particular importance at the village level. The numberdars in our sample village are also politically active. Of the 378 households who said they knew the numberdar, 44% said that he

recommended a candidate in the last election. In addition, 49% said that he knew how they voted in the last national election, while 13% said they were not sure whether he knew or not. This could either mean that the secrecy of the ballot is compromised, or that he would know because they told him, to take advantage of an offer of clientelism (see Stokes, Dunning, Nazareno, and Brusco (2013)). For all these reasons, in this paper we focus on the numberdar's role as the local level patron. We have data on the local numberdar for 90% of the sample. We used the reports from all households in the village to determine the local patron's clan. For consistency, we use this subsample for all the estimates presented in this paper.

National politicians (Members of National Assembly or MNAs) were also named frequently; in every constituency at least one respondent mentioned knowing the politician. Each MNA represents a constituency of approximately 300,000 registered voters. There are 147 MNA constituencies in Punjab, of which our data cover thirteen. MNAs are elected in a first-past-the-post system. Multiple candidates contest every seat, but there are usually two or three major candidates. We focus on the winner and runner-up in this analysis. We used the reports from all households in the constituency to determine the politician's clan. In addition, research assistants at the Lahore School of Economics used current and historical news reports and informed contacts to identify the clan or caste of politicians. Because group and sub-group identities can overlap, we assume a household shares the politician's clan if any source indicates a match. For politicians' opponents, only these secondary sources were available, so we only have this variable for about half the sample.

Figure 2 shows the proportion of the sample that are politician co-clan members by constituency. There is a great deal of variation. In some constituencies, no sampled household shared the clan of the politician. In others, the politician's clan forms a substantial minority or even a majority. Figure 3 shows the same distribution for the local patron's clan within the village. This has a similarly large variation. (Note though that these are subject to significant sampling error, since only 16 households were surveyed in each village.)

As a second measure of social proximity for the politician only, we use the question on how the household could contact the politician. We define "access" as the ability to contact the politician by telephone or home visit. Table 1 shows the overlap between this and the shared clan variable.

There are two potential problems with this measure of access, as with any self-reported network links. First, households could report access differentially; if households who received assistance are more

Figure 2: **Concentration of clans in politicians' constituencies**

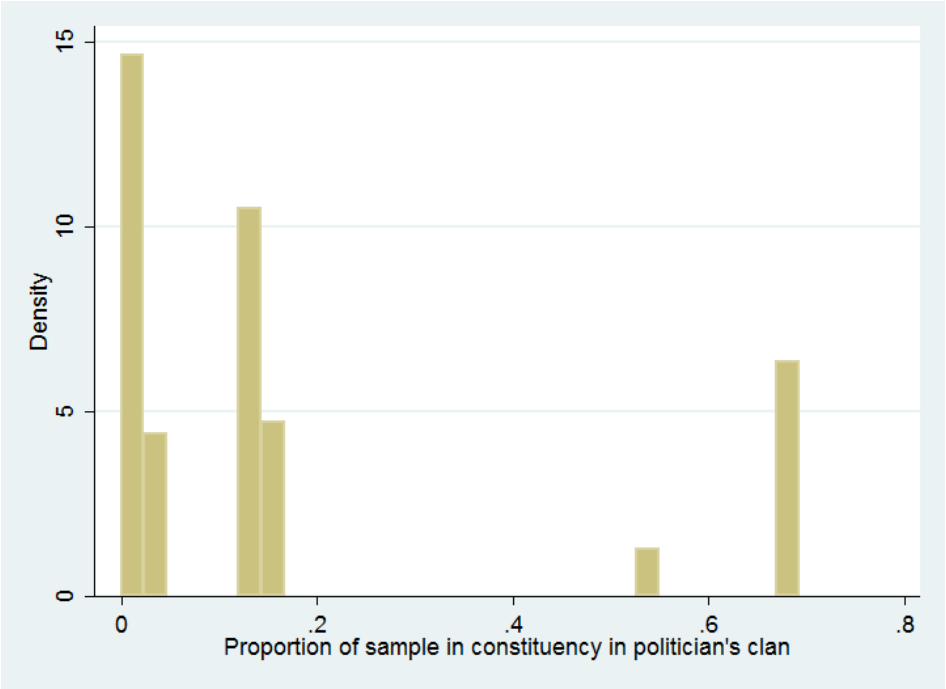


Figure 3: **Concentration of clans in patrons' villages**

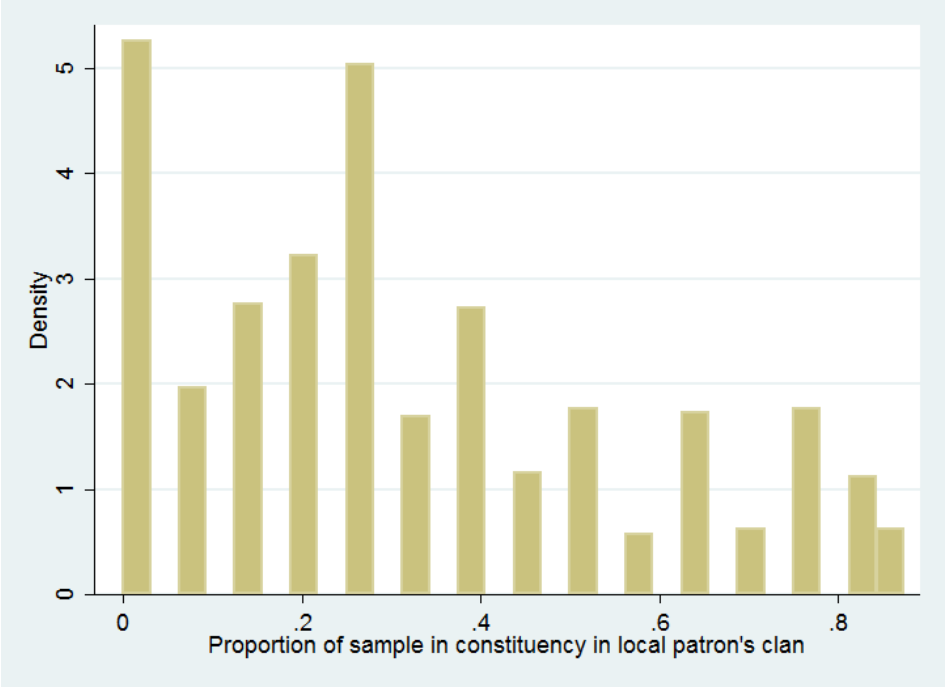


Table 1: Politician clan and access

HH has telephone, office or home access to politician	HH and politician same clan	
	No	Yes
No	691	142
	75%	15%
Yes	70	21
	8%	2%

likely to report a connection this could bias our estimates (Comola and Fafchamps, 2014). Second, access could occur endogenously, if households seeking assistance find ways to contact the politician. However, the survey question was designed to address the first problem by focusing on an objective measure: phone or home visits to the politician. This should be less subject to response bias than asking if the respondent could go to him for assistance, as in the network measures discussed by Comola and Fafchamps (2014). To address the second problem, we examine specific assistance variables that relate to these government assistance programs. In the whole sample of 924 households, only *two* households reported any assistance from the politician in getting access to either of these types of government assistance programs. So it seems very unlikely that households' strategic link formation explains our results.

We use two measures of assistance that households receive; both are proxies for g in the theoretical model. The first variable, t, is a dummy for receiving any of the targeted government benefits programs observed in our data. These include unconditional transfers targeted at needy households, and education-related transfers. The first category includes the national level Benazir Income Support Program and government zakat program, and the provincial Sasta Rashan ("Cheap Ration") program. The second category is education-related transfers. This includes the Punjab Girls' Stipend Program, the Punjab Free Textbook Program, and any other scholarship program the household reported. Chapters 3 and 4 discuss these programs in greater detail. The small sample size and low prevalance of many of these programs means that program-wise estimation with this data set yields unreliable results, with different results across specifications. Intead, in this paper we focus on whether households receive any targeted assistance program. Chapter 4 uses a different, larger dataset which allows estimation by different program categories.

The second measure is a dummy which equals one if the patron provided any assistance to the household in accessing public services (analogous to what some authors have called "constituency services"

(Stokes, Dunning, Nazareno, and Brusco, 2013)). This includes assistance in procuring a national identity card, getting children admission to a school, or getting access to a cash transfer program. Table 2 shows the full list of types of assistance that we asked about in the survey, and how often the local patron (numberdar) provided them. Assistance getting a national identity card is by far the most common. This card is used for many official purposes, including opening a bank account, registering property, getting government cash transfers, and voting. The importance of the card for voting suggests that these local patrons may be involved in organizing vote blocs.

These two measures complement each other, because the active assistance measures identify direct assistance from the patron to the client, while actual government program distribution allows us to test whether connections actually make a difference to where resources are spent, as well as avoiding potential reporting issues.

Table 2: Reported assistance from local patron

	Assisted with fee	Assisted without fee
Getting national ID card	-	21.24%
Resolving a problem at child's school	0.26%	1.28%
Paid child's school fee	-	1.04%
Getting a child admission to a school	0.26%	1.28%
Getting doctor's appointment	0.26%	2.86%
Resolving disagreements between HHs	0.25%	4.58%
Getting government transfer programs	0.00%	2.54%
When a HH member was victim of a crime	0.00%	1.53%
When a HH member was accused of a crime	0.00%	1.53%
Problems with the police	0.00%	2.04%
Lent money or goods such as agricultural inputs	-	0.82%
Helped to sell wheat to government procurement agency	0.00%	1.55%

To measure household work for patrons (e in the model), respondents were asked whether any household member had assisted each influential person mentioned through activities without being paid. Table 3 shows the frequency of these activities. Of all the respondents who knew the numberdar, 23% said they had assisted in weddings and festivals for the numberdar, and 9% mentioned engaging in political activities at his request.

Table 4 shows the overlap between these two categories. The proportions of households who only received assistance, only did unpaid work, or both are approximately equal.

We use the percentage vote difference between the winner and runner-up of the MNA seat as a measure of electoral competitiveness. Figure 4 shows the distribution of this variable in our sample. It is highly

Table 3: Unpaid work for the local patron

Weddings, funerals, religious festivals	22.65%
Emergencies (such as floods)	3.05%
Agricultural work without pay	2.54%
Building work without pay	1.78%
Canvassing for a political candidate	5.60%
Participating in political gatherings	2.80%

Table 4: Descriptive statistics: Assistance between local patrons and households

HH received any assistance from local patron	HH did any unpaid work for local patron	
	NO	YES
NO	740	70
	80%	8%
YES	54	60
	6%	6%

variable, including tight races with less than a 5% margin, as well as landslides with a 50 - 60% margin.

Table 5 shows descriptive statistics for all the variables in our empirical analysis.

Figure 4: **Electoral competitiveness in the PERI sample**

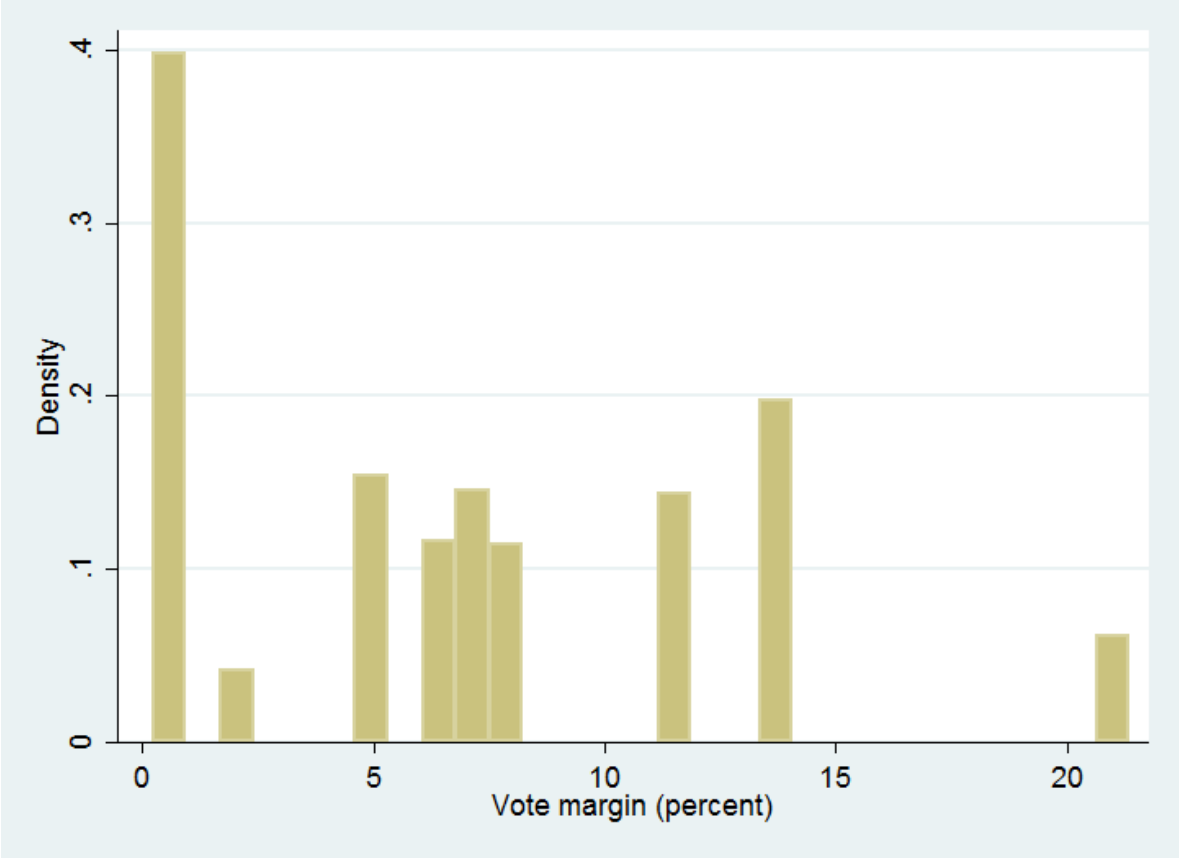


Table 5: Descriptive statistics

Variable	**N**	**Mean**	**SD**	**Min**	**Max**
Unconditional cash transfers	924	0.03	0.17	0.00	1.00
Education benefits	924	0.07	0.25	0.00	1.00
HH received either government assistance program	924	0.09	0.29	0.00	1.00
HH received assistance from local patron	924	0.12	0.33	0.00	1.00
HH did unpaid work for local patron	924	0.14	0.35	0.00	1.00
HH and winning politician same clan	924	0.18	0.38	0.00	1.00
HH and opponent politician same clan	439	0.05	0.22	0.00	1.00
HH and local patron same clan	833	0.33	0.47	0.00	1.00
HH is relative of local patron	924	0.02	0.16	0.00	1.00
Value of HH's dwelling (PKR)	924	178,432.95	341,500.80	0.00	5,000,000.00
Acres of land HH owns	922	2.19	5.37	0.00	80.00
Years HH has owned land in village	924	11.55	25.72	0.00	200.00
Landless	924	0.60	0.49	0.00	1.00
Years of education (most educated adult HH member)	908	6.62	4.91	0.00	21.00
HH head literate	924	0.64	0.48	0.00	1.00
Any child under 14 years ever enrolled	924	0.60	0.49	0.00	1.00
Vote margin (percent)	924	6.88	5.64	0.23	21.36

Number of observations is smaller for local patron clan because in a few villages no local patron was named by any respondent. Number of observations is smaller for opponents because this information was not collected directly in the survey, so it is only available for some constituencies through secondary sources. Data availability is discussed further in Section 3.

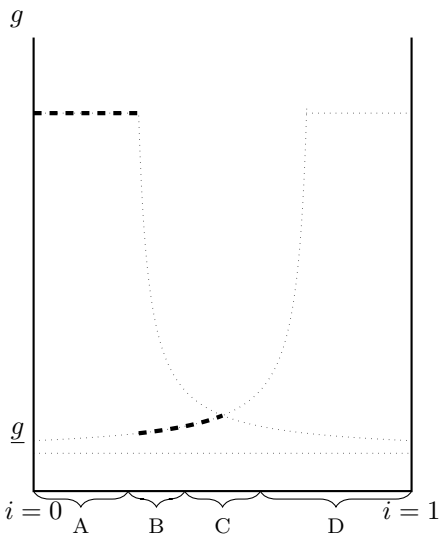


Figure 5: g observed if politician 0 wins; nepotistic warm glow; $\underline{g}$ non binding.

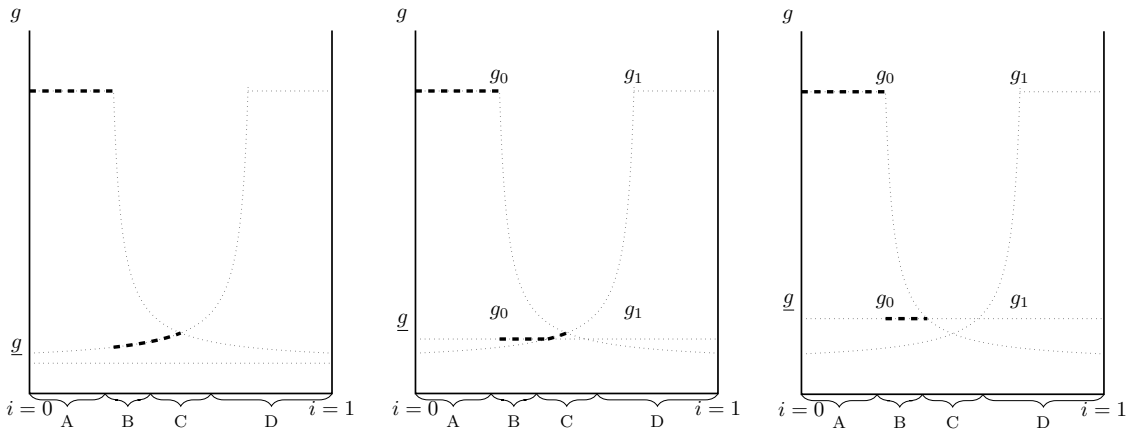
4 Theoretical and empirical framework

4.1 Politicians

Figures 5 - 7 show the distribution of assistance which should be observed empirically with different politician motivation and circumstances, per the theoretical model in Chapter 1. We divide the continuum of voters into groups A, B, C, and D according to their connections to the winning and losing politicians.

Figure 5 shows the case with warm glow and coordination costs. As shown in Chapter 1, this pattern of g can be produced if only warm glow depends on social proximity, or if both warm glow and coordination costs depend on it. Group A, who are closest to the politician, are the "inner circle": they receive the maximum amount of g possible, because the politician gets a net personal benefit from assisting them. They are the greatest beneficiaries of nepotistic warm glow. Group B and C benefit from clientelism: assisting them is costly. However, the politician's warm glow is less (and costs may be greater) the greater the social distance between him and the voter. As a result, the central voters (group C) are "within reach" of both the politician and his opponent. The two compete over them, and they are offered more as a result.

Figure 6 shows how the observed outcome changes if $\underline{g}$, the minimum amount of g that a voter will accept to form an alliance, is partially or fully binding. In the partially binding case (6b), the same



(a) $\underline{g} = 0.1$, not binding (b) $\underline{g} = 0.18$, partially binding. (c) $\underline{g} = 0.25$, fully binding

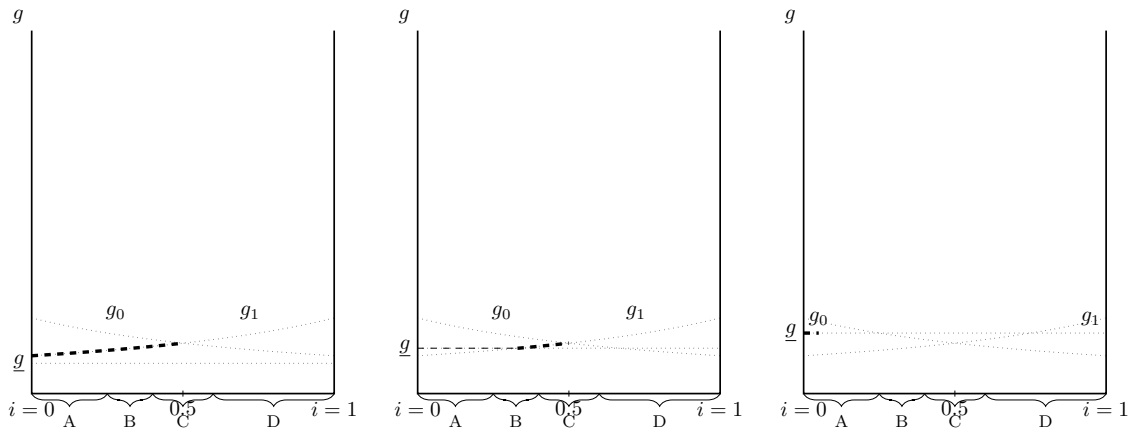
Figure 6: g offered in symmetric Nash equilibrium with $\underline{g}$ fully binding. Case with social connections affecting warm glow but not coordination costs. $\alpha = 0$, $\beta = .5$, $\omega = \frac{1}{8}$, $\gamma = 1$. Panels show equilibria with different levels of $\underline{g}$.

groups of voters receive assistance as in the non-binding case. However, the politician's closer contacts benefit more than before, because he must offer them at least $\underline{g}$. In the fully binding case (6c), the cost of reaching the central voters in group C is too great, and neither politician makes them an offer - so they get no g regardless of which politician wins.

Figures 7a - 7c show the case in which social distance determines the cost of coordination and commitment, but not warm glow. The pattern is similar to that in the previous three figures, but there is no special assistance to the "inner circle". Group A still receive assistance, but unless $\underline{g}$ is so high that making an alliance with more distant voters is prohibitive, Group A still receive less assistance than groups B and C.

The final possibility we consider is that the politician is unopposed, and has no need to engage in clientelism. If he is motivated by warm glow, only the inner circle (group A) will benefit; this is unaffected by the change in political competition. If he is not motivated by warm glow, no voters will receive g.

Note than in all the cases considered, Group D, who have a closer connection to the losing politician receive no g. In all cases except where $\underline{g}$ binds, they less than any other group, including unconnected voters. This provides a motivation for observed punishment effects (e.g. Fafchamps and Labonne (2013)).



(a) $\underline{g} = .1$ (b) $\underline{g} = .15$ (c) $\underline{g} = .2$

Figure 7: g offered in symmetric Nash equilibrium with coordination costs and no warm glow, $\underline{g}$ fully binding. $\omega = 0$, $\alpha = 0.2$, $\beta = 0.2$, $M = 0.05$. Panels show equilibria with different values of $\underline{g}$.

To test for these patterns in the data, we estimate:

$$t_i = \beta_0 + \beta_1 Clan_{ij} + \beta_2 Access_{ij} + \beta_3 ClanAccess_{ij} + \gamma X_i + \alpha_c + \epsilon_i \quad (1)$$

Where t_i is a measure of targeted government assistance to household i; $Clan_{ij}$ is a dummy for household i being in politician j's clan; $Access_{ij}$ is a dummy for whether the household reports that they can access the politician by telephone or home visit; and $ClanAccess_{ij}$ is the interaction of the two.

ClanAccess is used to proxy for the "inner circle" (group A in the figures). *Clan* and *Access* are proxies for group B. Those with no connection to the winning politician are the reference group in Equation 1, and proxy for group C and D.

X_i is a vector of control variables including whether the household is a co-clan member or relative of the local patron; the number of years the household has lived in the village; the value of the household's house; dummy and continuous measures of land ownership; a dummy for whether the household head is literate; a continuous measure of the maximum level of education of any adult household member; and a dummy for any child under 14 years of age in the household was ever enrolled in school (to help capture eligibility for scholarship programs that are offered to students in older grades).

α_c is a clan-specific error term, to allow for clans to have common unobserved characteristics. We present results both (1) assuming α is uncorrelated with the terms of interest, i.e. random effects; and (2) assuming it may be correlated, i.e. fixed effects. The random effects estimate is more efficient if the assumption is correct. However, the fixed effects estimate controls for all unobserved differences between clans that are constant between villages, which will address potential omitted variable biases. For example, some clans tend to be more wealthy or have higher status across Punjab; the fixed effects estimate will control for this.

For a subset of constituencies, we also have the politician's opponent's clan, so we can estimate separate coefficients for groups C and D:

$$t_i = \beta_0 + \beta_1 ClanAccess_{ij} + \beta_2 Clan_{ij} + \beta_3 Access_{ij} + \beta_4 Clan_{i,-j} + \gamma X_i + \alpha_c + \epsilon_i \quad (2)$$

Where $Clan_{i,-j}$ is a dummy for the household sharing the opponent's clan, and proxies Group D. Now the least connected households, i.e. Group C, are the reference group.

Finally, we test for whether assistance to Group B and C is more sensitive to electoral competition than Group A, the "inner circle". A completely unopposed politician would only assist Group A. We test this idea by adding interaction terms for electoral competitiveness to Equation 1. $Votemargin$ is the percentage difference between the votes received by the winner and runner-up in the constituency. A larger value indicates a less competitive election.

$$\begin{aligned} t_i =& \beta_0 + \beta_1 Votemargin_j + \beta_2 Clan_{ij} + \beta_3 Clan_{ij} Votemargin_j \\ &+ \beta_4 Access_{ij} + \beta_5 Access_{ij} Votemargin_j + \beta_6 ClanAccess_{ij} + \beta_7 ClanAccess_{ij} Votemargin_j \\ &+ \gamma X_i + \alpha_c + \epsilon_i \end{aligned} \tag{3}$$

The model predicts a negative sign for coefficients on interaction terms β_3 and β_5, because in less competitive constituencies, the politician does not need to use transfers of g to win the election. β_6 would be zero, because the "inner circle" receives assistance because of warm glow, regardless of electoral competition.

4.2 Vote blocs and patrons

For the local patrons, we have three dependent variables of interest:

- t_i is as defined above, i.e. a dummy which equals 1 if household i received any targeted government assistance program;
- a_i is a dummy which equals 1 if household i reported that they received assistance from the local patron;
- e_i is a dummy which equals 1 if household i reported that they did any unpaid work for the local patron.

The details of how these variables are constructed is discussed further in Section 3.

We test the following predictions from Chapter 1:

Prediction 1. *If the patron can act as a "gatekeeper" for the politician's assistance, assistance from the patron will be greater in communities where the patron has a closer social connection to the politician,* $G_k = \underline{G_k} + G_{jk}$.

We test this prediction using shared clan as a proxy for the social connection between the politician and the patron. We estimate:

$$t_i = \beta_0 + \beta_1 Clan_{jk} + \gamma X_i + \alpha_c + \epsilon_i \tag{4}$$

$$a_i = \beta_0 + \beta_1 Clan_{jk} + \gamma X_i + \alpha_c + \epsilon_i \tag{5}$$

Here we include the household's shared clan with the politician and local patron as part of the vector of controls X, in addition to the other control variables mentioned above.

Prediction 2. *If a patron is motivated by altruism towards his close contacts, he will offer his contacts more assistance in exchange for less effort.*

Prediction 3. *If a patron is motivated by easier interactions with his close contacts, he will demand greater effort from them.*

We test these two alternative predictions with the following specifications:

$$t_i = \beta_0 + \beta_1 Clan_{ik} + \beta_2 Relative_{ik} + \gamma X_i + \alpha_c + \epsilon_i \tag{6}$$

$$a_i = \beta_0 + \beta_1 Clan_{ik} + \beta_2 Relative_{ik} + \gamma X_i + \alpha_c + \epsilon_i \tag{7}$$

$$e_i = \beta_0 + \beta_1 Clan_{ik} + \beta_2 Relative_{ik} + \gamma X_i + \alpha_c + \epsilon_i \tag{8}$$

If in-group altruism is the mechanism, we expect β_1 and β_2 to be positive in Equations 6 and 7, but negative or zero in Equation 8. In contrast, if ease of interaction is the mechanism, we expect β_1 and β_2 to be positive in Equation 8.

Prediction 4. *An increase in the outside option of voters makes the patron more likely to encourage them to work on the market instead of for him. However, this only occurs if he is altruistic towards those voters.*

To test this prediction, we interact measures of the market option w with the social connection variables. We use two measures of the household's outside option. The first is the education level of the most educated household member. The second is the county (tehsil)-level mean income in private sector non-agricultural employment for our subsample in 2007-8.

$$e_i = \beta_0 + \beta_1 Ed_i + \beta_2 Clan_{ik} + \beta_3 Clan_{ik} Ed_i + \beta_4 Relative_{ik} + \beta_5 Relative_{ik} Ed_i + \gamma X_i + \alpha_c + \epsilon_i \quad (9)$$

$$e_i = \beta_0 + \beta_1 Inc_g + \beta_2 Clan_{ik} + \beta_3 Clan_{ik} Inc_g + \beta_4 Relative_{ik} + \beta_5 Relative_{ik} Inc_g + \gamma X_i + \delta_g + \alpha_c + \epsilon_i \quad (10)$$

Since the interaction in Equation 10 is computed from a county average, we include county dummies δ.

If this prediction holds, we expect β_3 and β_5 to be negative.

5 Empirical Results

5.1 Politicians

Table 6 shows the results of Equations 1 and 2. Columns 1-4 show the full sample, while Columns 5-8 show the subsample in which we obtained data on opponents. As shown in Columns 1-2 and 5-6, consistent with the model, the co-clan members of the winning politician receive the most assistance, followed by unconnected voters (the reference group), while the co-clan members of the losing politician receive the least. Columns 3-4 and 7-8 show a pattern consistent with the model of warm glow, as depicted in Figures 5 - 6b. Group A, the "inner circle" of the politician's closest contacts, receive the most assistance, while our two proxies for Group B (clan only, and access only) still receive positive

assistance, but less than Group A. The differences between Group A and B, and between Group A and C, are significant at the 1% level for the random effects model clustered at village level.

Table 7 shows the results of Equation 3. The results support the combination model of warm glow and clientelism with coordination costs. Group A, the "inner circle", who have both shared clan membership with and access to the politician, are far more likely to receive government assistance. The interaction term is insignificant: assistance to the inner circle is unrelated to electoral competitiveness. This confirms that this assistance is attributable to warm glow, not clientelism.

Of the two proxies for Group B, Clan only is again positive and significant. But as predicted by the model, these households receive less assistance when the election is less competitive. This suggests that this assistance is attributable to clientelism.

It is possible that vote margin may be proxying for other variables at the community or individual level. For example, if elections are more closely contested in more economically developed areas where voters are more educated, then a positive coefficient on the interaction term of clan with vote margin may in fact reflect not clientelism, but the fact that that more educated households are more likely to benefit from a politician of the same clan. To address this concern, we interact all the control variables with vote margin and include these as additional controls. Column 3 in Table 7 shows that the results are robust to this.

Table 6: Social proximity to politician and government assistance targeting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	HH received any government assistance program							
HH and winning politician same clan	0.11	0.16			0.26	0.38		
	(0.04)**	(0.07)**			(0.08)***	(0.11)***		
	(0.06)*	(0.06)**			(0.05)***	(0.08)***		
	(0.05)**	(0.06)**			(0.08)***	(0.14)**		
Clan and Access			0.32	0.35			0.44	0.56
			(0.08)***	(0.10)***			(0.09)***	(0.12)***
			(0.05)***	(0.07)***			(0.06)***	(0.07)***
			(0.04)***	(0.07)***			(0.10)***	(0.13)**
Access only			0.06	0.02			0.09	0.08
			(0.03)*	(0.04)			(0.04)**	(0.04)*
			(0.03)*	(0.04)			(0.05)**	(0.05)
			(0.04)	(0.03)			(0.04)**	(0.03)**
Clan only			0.09	0.15			0.22	0.34
			(0.04)**	(0.07)**			(0.07)***	(0.11)***
			(0.06)	(0.06)**			(0.05)***	(0.08)***
			(0.05)*	(0.06)**			(0.06)***	(0.13)*
HH and politician's opponent same clan					-0.26	-0.20	-0.24	-0.20
					(0.11)**	(0.14)	(0.10)**	(0.13)
					(0.05)***	(0.06)***	(0.05)***	(0.06)***
					(0.07)***	(0.07)**	(0.05)***	(0.06)**
Control variables	YES	YES	YES	YES	YES	YES	YES	YES
Clan FE		YES		YES		YES		YES
Control group mean	0.08		0.08		0.08		0.06	
Observations	816	816	816	816	351	351	351	351

Robust standard errors in parentheses, clustered by village level, clan x constituency, and constituency respectively. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Controls include shared clan with the local patron, vote margin between the winner and runner-up, the number of years the HH has lived in the village; value of the HH's house; dummy and continuous measures of land ownership; a dummy for whether the head is literate; continuous measure of maximum level of education of any adult HH member; and a dummy for any child under 14 in HH ever enrolled in school.

5.2 Voter blocs and patrons

Table 8 shows the results of Equation 4 and 5. The results are inconsistent with the model. When the local patron and the politician share the same clan, there is no difference in government assistance. These households are *less* likely to report active assistance from the patron. However, this result is not robust across specifications.

If correct, this pattern suggests possible extensions to the model. Patrons may act as substitutes for politician assistance. Politicians may either work directly with voters if they are close to them, or else have to work through local patrons for more distant groups to overcome the greater commitment costs with those groups.

In future work we will also exploit data from another sample, in which we have data on actual alliances between politicians and local patrons, to explore this relationship further.

Table 9 shows the results of Equations 6 - 8. The local patron's relatives receive much more assistance from him, while doing far less unpaid work for him. These results are more consistent with the model of social connections influencing the patron's actions via in-group altruism, rather than ease of exchange. It is possible that ease of exchange does motivate the assistance to relatives, if relatives provide other kinds of services to the patron that we do not measure. However, it is noteworthy that many of the forms of unpaid work we asked about in the survey are things that relatives might well be expected to do, such as supporting the patron in political canvassing, and helping during events such as festivals, weddings, and funerals. Yet controlling for their characteristics, they provide less of this kind of assistance to the patron than other households do.

Conditional on being a relative, shared clan with the patron is insignificant. The results are also insignificant for the government assistance programs.

Table 7: Political competition, social proximity and government assistance targeting

	(1)	(2)	(3)
	HH received any government assistance program		
Clan and access	0.33	0.34	0.15
	(0.09)***	(0.10)***	(0.64)
	[0.05]***	[0.07]***	[0.26]
	0.04***	0.05***	0.28
Clan and access x vote margin	0.00	0.02	0.05
	(0.04)	(0.04)	(0.05)
	[0.02]	[0.02]	[0.04]
	0.02	0.02	0.04
Access only	-0.00	-0.04	0.19
	(0.06)	(0.07)	(0.27)
	[0.06]	[0.07]	[0.26]
	0.06	0.07	0.28
Access only x vote margin	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)
	[0.01]	[0.01]	[0.01]
	0.01	0.01	0.01
Clan only	0.16	0.18	0.15
	(0.05)***	(0.07)***	(0.11)
	[0.05]***	[0.07]***	[0.11]
	0.04***	0.06***	0.09*
Clan only x vote margin	-0.02	-0.01	-0.02
	(0.01)***	(0.01)	(0.01)***
	[0.01]***	[0.01]	[0.01]***
	0.00***	0.01	0.00***
Control variables	YES	YES	YES
Clan FE		YES	
Control variables x group dummies			YES
Control group mean		0.08	
Observations	816	816	816

Robust standard errors, clustered by village level (parentheses), clan x constituency [brackets], and constituency braces. * $p < 0.1$, ** $p < 0.05$, ***$p < 0.01$. Controls include shared clan with the local patron, vote margin between the winner and runner-up, the number of years the HH has lived in the village; value of the HH's house; dummy and continuous measures of land ownership; a dummy for whether the head is literate; continuous measure of maximum level of education of any adult HH member; and a dummy for any child under 14 in HH ever enrolled in school. Controls x group dummies consist of the vector of controls multipled by the vector of group dummies (clan and access, clan only, access only).

Table 8: Politician-local patron interaction and assistance to households

	(1)	(2)	(3)	(4)
	HH received any government assistance program		HH reported any assistance from local patron	
Politician and local patron same clan	0.06	0.05	-0.08	-0.05
	(0.05)	(0.04)	(0.03)***	(0.04)
	(0.04)	(0.04)	(0.03)***	(0.04)
	(0.05)	(0.05)	(0.03)**	(0.04)
Control variables	YES	YES	YES	YES
Clan FE		YES		YES
Control group mean	0.08	0.08	0.08	0.08
Observations	784	784	784	784

Robust standard errors in parentheses, clustered by village level, clan x constituency, and constituency respectively. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Controls include shared clan with the politician, shared clan with the patron, vote margin between the winner and runner-up, the number of years the HH has lived in the village; value of the HH's house; dummy and continuous measures of land ownership; a dummy for whether the head is literate; continuous measure of maximum level of education of any adult HH member; and a dummy for any child under 14 in HH ever enrolled in school.

Table 9: Local patron - household interaction and social proximity

	(1)	(2)	(3)	(4)	(5)	(6)
	HH received any government assistance program		HH received active assistance from patron		HH did unpaid work for local patron	
HH is relative of local patron	0.06	0.06	0.18	0.18	-0.15	-0.15
	(0.07)	(0.08)	(0.09)**	(0.10)*	(0.03)***	(0.03)***
	(0.06)	(0.08)	(0.08)**	(0.09)**	(0.02)***	(0.03)***
	(0.05)	(0.06)	(0.10)*	(0.10)*	(0.02)***	(0.02)***
HH and local patron same clan	-0.00	-0.05	-0.02	0.02	-0.01	-0.00
	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)
	(0.03)	(0.03)*	(0.03)	(0.03)	(0.04)	(0.04)
	(0.02)	(0.02)*	(0.03)	(0.05)	(0.05)	(0.05)
Control variables	YES	YES	YES	YES	YES	YES
Clan FE		YES		YES		YES
Control group mean	0.07		0.14		0.18	
Observations	816	816	816	816	816	816

Robust standard errors in parentheses, clustered by village level, clan x constituency, and constituency respectively. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Controls include shared clan with the politician, vote margin between the winner and runner-up, the number of years the HH has lived in the village; value of the HH's house; dummy and continuous measures of land ownership; a dummy for whether the head is literate; continuous measure of maximum level of education of any adult HH member; and a dummy for any child under 14 in HH ever enrolled in school.

Table 10 shows the results of Equations 9 and 10. Neither the interaction with education, nor with county mean private income, is significant. This does not support prediction 4, that the outside option affects the work that households do for local patrons. However, the standard errors on these terms are very large, so the evidence is inconclusive.

6 Conclusions

We have provided a theoretically - grounded analysis of an original household survey dataset, shedding light on the interaction between politicians, local patrons, and households in rural Pakistan. To our knowledge this is the first empirical quantitative study that looks at both these levels and their interaction. We believe it is also the first to analyze quantitative data on the exchange of unpaid work for patron assistance.

Our results provide clear support for the model of politicians motivated by both nepotistic warm glow for their closest contacts, and clientelistic competition over less connected groups. They also support the model of patrons motivated by nepotistic altruism for their relatives, and extracting unpaid labor from other households in their community.

The results on politician-patron interaction are less clear. They suggest that assistance from the two may act as substitutes, or politicians may work with voters directly when they are well connected to them, and work through patrons when social distance makes the coordination costs too high. We also find no clear evidence on the relationship between market opportunities and interaction with patrons.

The results have implications for our understanding of the theoretical and empirical literature on clientelism and resource distribution. They support an alternative approach to reconciling the core-swing debate, as discussed in Chapter 1.

They also have implications for the interpretation of empirical literature on targeting of government assistance programs. For example, we may better understand an empirical finding on political influence on targeting if we distinguish between high value assistance to a very small group (the inner circle), which will not generalize beyond that group, and low-value assistance to a much larger group for clientelistic purposes. Our framework also formalizes an explanation for why political competition

Table 10: Local patron-household interaction and market labor opportunities

	(1)	(2)	(3)	(4)
	HH did any unpaid work for patron			
HH is relative of local patron	-0.22	-0.17	-0.15	-0.14
	(0.12)*	(0.14)	(0.06)***	(0.07)**
	(0.12)*	(0.14)	(0.05)***	(0.06)**
	(0.13)*	(0.12)	(0.05)***	(0.06)**
HH is relative of local patron X	0.12	0.05		
county mean private sector income	(0.22)	(0.24)		
	(0.23)	(0.24)		
	(0.21)	(0.20)		
HH is relative of local patron X			-0.00	-0.01
HH has at least one adult			(0.05)	(0.07)
who completed primary			(0.06)	(0.07)
			(0.06)	(0.07)
HH and local patron same clan	-0.13	-0.14	-0.07	-0.06
	(0.18)	(0.18)	(0.06)	(0.06)
	(0.14)	(0.14)	(0.05)	(0.05)
	(0.23)	(0.20)	(0.06)	(0.05)
HH and local patron same clan X	0.22	0.26		
county mean private sector income	(0.33)	(0.33)		
	(0.25)	(0.24)		
	(0.40)	(0.36)		
HH and local patron same clan X			0.09	0.09
HH has at least one adult			(0.07)	(0.07)
who completed primary			(0.06)	(0.06)
			(0.09)	(0.08)
HH has at least one adult			-0.07	-0.08
who completed primary			(0.04)*	(0.04)**
			(0.03)**	(0.04)**
			(0.05)	(0.04)*
Control variables	YES	YES	YES	YES
County FE	YES	YES		
Clan FE		YES		YES
Observations	816	816	819	819

Robust standard errors in parentheses, clustered by village level, clan x constituency, and constituency respectively. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Controls include shared clan with the politician, vote margin between the winner and runner-up, the number of years the HH has lived in the village; value of the HH's house; dummy and continuous measures of land ownership; a dummy for whether the head is literate; continuous measure of maximum level of education of any adult HH member; and a dummy for any child under 14 in HH ever enrolled in school.

plays a role in determining some types of assistance from politicians and not others.

Potential extensions to this work could use differences in community clan composition to explore how clan relates to the different groups of voters (A-D) in our data. Publicly available data on polling station-wise electoral results could also be incorporated into tests of the model. In addition, we plan to use a new dataset with data directly measuring politician-patron alliances to explore their interactions in greater detail. Finally, we may also consider extensions to the model that can help explain the politician-patron interaction better.

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Targeting one's own:

Politicians, clans, and cash transfers in Punjab, Pakistan

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Abstract

In this chapter, I estimate the extent of politicians' influence over targeting of government assistance to households. I use primary panel data from rural Punjab, Pakistan over a national election cycle to compare targeting in years when politicians of different clans were in office. I find that when a politician is elected, his co-clan members are 70% more likely than their peers to receive government assistance. I find a pattern of political interference in targeting that is consistent with the theoretical model in Chapter 1: the effect is greater overall in more competitive elections, but concentrated among the politician's "inner circle" in less competitive elections. A shared connection with a local official strengthens the effect, again consistent with the theoretical model. The advantage of a politician's clan members is reversed when he is replaced by a member of another clan. This suggests that the mechanism may be direct influence on targeting, rather than dissemination of information on how to apply for assistance. This effect occurs in households that are likely to be eligible and ineligible for these programs. This suggests that politicians' influence may both selectively correct exclusion errors in targeting and create inclusion errors.

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1 Introduction

Targeted assistance programs, such as cash transfers, are an important component of social spending in developing countries. They are potentially an effective tool for poverty alleviation and human capital investment. However, the targeting process itself may be subject to interference, whether through petty corruption by low-level functionaries (Niehaus, Atanassova, Bertrand, and Mullainathan, 2013), assistance by local committee members to their connections (Caeyers and Dercon, 2012), or the influence of high level politicians.

This chapter address this latter issue - the role of national politicians. I test whether politicians in rural Punjab, Pakistan influence the targeting of government assistance programs, including unconditional cash transfers and educational programs such as scholarships. I use a differences-in-differences approach with a primary panel dataset to estimate how much assistance a politician directs towards his own clan. This approach compares households at times when they are represented by a co-clan politician and when a politician from another clan is in office. This approach controls for all time-invariant unobservables determining whether a clan receives assistance, such as their long-run wealth, education, and social standing.

I find strong evidence that politicians direct government assistance programs towards their own clans. Households are more likely to receive assistance when a co-clan member comes into office. The size of the effect is large: households in the politician's clan are more than twice as likely as others to receive these transfers. The results are robust to clan fixed effects, household fixed effects, alternative specifications of clustering of standard errors, district-specific time trends, clan-specific time trends, and non-linear model estimation.

I test whether this political influence is due to clientelism or to inherent preferences for one's own group. Unlike previous literature, I allow for both of these mechanisms, following the theoretical model in Chapter 1. I find a pattern of political interference in targeting that is consistent with the model. Overall, the effect is stronger for politicians who won with a narrower vote margin. Based on the linear estimates, an increase of ten percentage points in the vote margin would halve the effect for unconditional cash transfers, and eliminate the effect for scholarships completely. However, the benefits are concentrated among the politician's "inner circle", particularly in less competitive elections. Consistent with the theoretical model, I find that the politician's opponent's clan also receives less assistance than other groups.

I also explore mechanisms for the influence. Consistent with the theoretical model, I find evidence that local officials may play a mediating role in directing assistance towards connections. The politician

effect is strongest where the local official is also a member of the same clan as the politician.

Households who received assistance when represented by a member of the same clan stop receiving unconditional cash transfers when the politician is replaced by a member of another clan, which suggests that the spread of information is not the main mechanism.

The results have implications for the overall quality of targeting of public assistance. Both eligible and ineligible members of the politician's clan appear to benefit. The impact of political connections on targeting appears to play a much larger role than the effect of observable poverty criteria.

The remainder of the paper proceeds as follows. Section 2 reviews relevant literature. Section 3 describes the political and institutional context. Section 4 presents the empirical strategy. Section 5 describes the data; Section 6 presents the results; and Section 7 concludes.

2 Literature

As outlined in the theoretical model in Chapter 1, I consider two possible motivations for politicians to direct assistance towards their own clans: clientelism and nepotism.

The objective of clientelism is to strengthen political support. It is a quid-pro-quo in which government assistance is exchanged for votes. Thus targeted assistance programs are particularly useful for clientelistic distribution. Nepotism, as I define it here, is any preference for benefiting one's own group or connections, regardless of whether it is needed for electoral success. This could include an altruistic or warm glow motivation, or social pressure to assist family members or close social connections.

There is empirical evidence from a number of contexts of politicians favoring their own group - village, ethnicity, or caste - in targeting public services or recommending individuals for jobs. Some literature places this in the context of clientelism, e.g. Besley, Pande, and Rao (2011). In this political economy framework, politicians may have personal preferences over policies, as in the citizen-candidate models of Besley and Coate (1997), Osborne and Slivinski (1996), Munshi and Rosenzweig (2013). However, papers in this literature typically do not address politicians' inherent personal preferences to assist particular individuals such as their relatives or friends. In some settings, such as in the case of elected village-level officials, this is a plausible alternative explanation to the political motivation.

In contrast, there is some empirical literature on the impact of social and family connections to officials on government assistance. Caeyers and Dercon (2012) find that households with a social link to officials in Ethiopia are more likely to get food aid immediately after a drought, although they cannot rule out strategic link formation by households. Fafchamps and Labonne (2013) find that politicians'

relatives in the Philippines get better jobs, both in the public and private sector. They find that this effect is weaker in highly politically contested areas. This suggests that the effect they find is due to nepotism, rather than clientelism.[1]

Observed patterns of distribution may reflect a combination of these two motivations - clientelism and in-group altruism. Most literature has not considered the two together.

This chapter contributes in three ways to this literature. First, I exploit the panel dimension of the data to provide credible causal identification of the effect of a household's connection to a politician - which has been limited in the literature (Fafchamps and Labonne, 2013). I find that households are much more likely to receive government assistance when they are represented by a politician of the same clan.

Second, I explore whether the benefit that connected households receive is due to clientelism or nepotism. I find different patterns of reward of a politician's inner circle and punishment of his opponent's connections in constituencies with different levels of political competition. These patterns are consistent with the theoretical model in Chapter 1. The results from the theoretical model and empirical analysis both show politicians targeting high levels of assistance towards a connected "inner circle" regardless of electoral pressure, but targeting assistance more broadly as a form of clientelism when electoral pressure is high. As I argued in Chapter 1, this also provides an alternative way of resolving the core-swing debate in the clientelism literature (Lindbeck and Weibull, 1987, Dixit and Londregan, 1996, Cox and McCubbins, 1986, Nichter, 2008, Stokes, Dunning, Nazareno, and Brusco, 2013, Vaishnav and Sircar, 2010).

Third, I explore possible mechanisms of the politician's influence. I find, again consistent with the model, that local officials seem to play an important role in targeting the politician's clan members. I also find some suggestive evidence that neither direct active intervention by the politician, nor the spread of information through his networks, is the key mechanism for this effect.

3 Context

Punjab is the largest province of Pakistan by population, with 80 million of the country's 180 million people. Approximately 50 million of Punjab's population live in rural areas.

The most recent elections took place in 2002, 2008 and 2013. The 2002 election was held under the military rule of Pervez Musharraf, but was still actively contested; the 2008 elections took place as a part of the transition back to civilian rule.

[1] A number of other papers also look at connections in the context of firms (Fisman, 2001, Khwaja and Mian, 2005).

The politicians we study are national legislators, Members of the National Assembly. Each MNA represents a constituency of approximately 300,000 registered voters. There are 147 MNA constituencies in Punjab, of which our data cover thirteen. MNAs are elected in a first-past-the-post system. Multiple candidates contest every seat, but there are usually two or three major candidates. I focus on the winner and runner-up in this analysis.

Two major parties have dominated national politics for the long term: the PML-N (Pakistan Muslim League - Nawaz) and the PPP (Pakistan People's Party). Each of these parties has been dominated by one family at the top for decades - the Sharif family and the Bhutto family, respectively. Individual constituencies also tend to be dominated by powerful local families, often landlord or "feudal" families, who have been involved in politics for decades. Some of these families are longtime supporters of one of the parties, while some have switched party opportunistically at various points.

In the 2002 election, the PML-Q (Pakistan Muslim League - Qaid e Azam) also played a major role. This party was aligned with the military ruler, Pervez Musharraf. Some of its candidates were from traditional political families who aligned with the government during this period, while many others were retired senior military officials. After the fall of Musharraf's government the PML-Q's base of support mostly disintegrated, with many of its politicians defecting back to the other parties.

Although the two elections were held in different institutional contexts, and both were subject to concerns about potential interference, both were also very actively contested, with many close outcomes in individual contituencies. This makes the change of incumbent and the comparison of winner and runner-up meaningful comparisons to assess the effects of connections to politicians.

The social link which I refer to as "clan" in this paper is called *zaat* or *biradari* in Urdu. It is an endogamous grouping similar to the subcaste in India. In Punjab in particular, which shares a common cultural and social heritage with Indian Punjab, many of these groups are the same as those in India. Islamic teaching rejects all such social distinctions, which may have altered the social role of biradari. Unlike in modern India, however, these social groups have no official recognition, and no group receives any official preferential treatment or discrimination on the basis of clan. However, whether biradari is in reality still hierarchical in Pakistan, like the caste system in India, is contested. It is clear, however, that biradari is highly salient in politics; newspapers routinely report results based on which biradaris supported which candidates after elections.

There is research to suggest that over time some groups of people in South Asia have successfully managed to change their caste in response to formal state restrictions or benefits reserved for particular groups - for example, Cassan (2012) finds that in colonial Punjab, after land ownership was reserved

for "agricultural castes", some groups were able to successfully manipulate their caste identity over a period of about four decades. This is unlikely to present a problem for our identification strategy for a number of reasons. First, the time frame over which caste may be manipulated is likely to be much longer than the five-year period covered in our study. Second, the benefits we observe are reversed when a politician leaves office. Thus if they were attributable to opportunistic caste changes these would have to take place over the electoral cycle, to take advantage of the connection to a specific politician during his term in office. This seems highly unlikely. Third, since caste and clan are not recognized in any formal way by the state in Pakistan, and there are no benefits reserved for any caste or clan, unlike in the colonial period and in modern India. Therefore, to benefit from such a change, a household would have to change its status according to the perception of the local social network, not in the classification of the state. While it might be possible to successfully change one's caste identity in the eyes of the colonial state authorities, doing so in the eyes of the social network of the clan itself may be much more difficult. This is especially likely to be true in rural villages with close social networks.

The targeted assistance programs I study are all the unconditional transfers targeted at needy households, and education-related transfers. The first category includes the national level Benazir Income Support Program and government zakat program, and the provincial Sasta Rashan ("Cheap Ration") program.

The national zakat program institutionalizes religious giving to the poor in Islam through taxation and an unconditional cash transfer. It is targeted at the community level by a local committee. This program was founded in the 1980s, but its funding has been reduced in subsequent years. It has no specific official criteria, other than to target the needy, widows and orphans. The exact targeting is left to the discretion of the local committee.

The Benazir Income Support Program (BISP) was founded in 2008. It is an unconditional cash transfer program. Recipients are supposed to receive 1000 PKR per month (USD 10) indefinitely. Initially MNAs were actually asked to select recipients from their constituents. However, after pressure from the World Bank, the government adopted a proxy means test based mechanism of targeting (a poverty scorecard). The scorecard gives points for female-headed households as well as for lack of assets and the number of dependents. This was carried out through a massive survey exercise by many NGOs in partnership with the government; initial sign-up does not depend on the recipient making any application, although the recipient must get a National Identity Card issued if he/she does not already have one.

The Punjab Sasta Rashan program was started in 2006. In this program, the senior civil service officer in each district was responsible for identifying needy families, who received staple food products below the market price.

The second category is education-related transfers. This includes the Punjab Girls' Stipend Program, the Punjab Free Textbook Program, and any other scholarship program the household reported.

The Punjab Girls' Stipend Program and Punjab Free Textbook Program were initiated as a part of the World Bank-supported Punjab Education Sector Reform Program.

The Punjab Girls' Stipend Program began in (year). It is a provincial conditional cash transfer targeted at girls in grades 6-10 in 14 selected low-literacy districts of Punjab. It is worth 2400 PKR annually (USD 24). It is paid out quarterly, conditional on 80% attendance, which is monitored and reported by school officials. The Free Textbook Program began in (year). Free textbooks are supposed to be distributed to all students in government schools from kindergarten to grade 10.

Even though these education programs are not supposed to have a discretionary component to their targeting, there are a number of potential avenues for interference with delivery, such as in the certification of attendance, or payment system through postal transfers in the girls' stipend program.

The small sample size and low prevalance of many of these programs means that program-wise estimation with this data set yields unreliable results, with very different results across specifications. Intead, in this paper I focus on whether households receive any targeted assistance program. However, in Chapter 4 (Vyborny and Haseeb, 2014) I use a different, larger dataset which allows estimation by different program categories.

4 Empirical strategy

The relationship of interest is:

$$g_{it} = \gamma L_{it} + \beta X_{it} + \epsilon_{it} \tag{1}$$

Where g_{it} is government assistance received by household i at time t and L_{it} is a dummy for household i having a link with a politician in office. γ is the parameter of interest.

However, there are several reasons why the estimate of γ in Equation 1 may be biased.

First, households may form links strategically, in order to attempt to access assistance or for other reasons. This would cause simultaneity bias. In this paper, I focus on a fixed link, shared clan between the household and politician, which eliminates this possibility.

Second, many important unobserved variables may be correlated with L. Well-connected households are less likely to be eligible for assistance programs targeted at the poorest. This would cause a downward bias. Conversely, these households may be more educated and more likely to know how to apply for assistance. Since they are also more likely to have children in school, they may also be more likely to be eligible for education benefits. These factors would result in an upward bias.

To address this, I exploit the change of politicians in the 2008 election. A household's clan does not change, but the politician in office changes over this period. I compare the same clans when they are represented by a co-clan member, and when they are not.

My preferred specification is an adaptation of the difference-in-differences specification for a "treatment" that occurs in some groups at time 1, and other groups at time 2:

$$g_{ict} = \beta_1 L_c + \gamma L_{ct} + \beta_2 Round_t + \beta_3 X_{ict} + \alpha_{ic} + \epsilon_{ict} \tag{2}$$

Where g_{ict} is government assistance received by household i in clan c at time t. L_c is a dummy for a household in the "treatment" group, i.e. a household which was ever represented by a co-clan member. L_{ct} is a dummy for being represented by a co-clan member at time t. I use the random effects estimator for improved efficiency. so α is an individual specific, time-invariant error term and ϵ is a time-variant error term. Both are assumed to be uncorrelated with L_{ct}.

In this case the "treatment" is at the level of a group (a clan within a constituency), rather than at the household level. Therefore, any omitted variable bias is due to an unobserved variable at the group level, i.e. correlated with clan and government assistance. So this specification solves omitted variable bias to the same extent that the household fixed effects specification would. It is also more efficient than the household fixed effects specification. However, I also show clan fixed effects and household fixed effects estimates as a robustness check.

This specification controls for all time-invariant unobservables determining whether a particular clan would be more likely to receive assistance, such as long-run wealth, education, and social standing. Any time-invariant geographic differences are also addressed by this strategy. Our estimate of γ will only be biased if an omitted variable correlated with the change in politician in office also determines changes in assistance to his clan. To address this possibility, we also show robustness to district- and clan-specific time trends.

5 Data

I test the predictions of our model using primary data from the PERI panel household survey. I collaborated with a research team from the Lahore School of Economics, who created this panel by re-visiting 1,024 of the households surveyed in a large province-wide cross-sectional survey (the UNICEF-Punjab government Multiple Indicator Cluster Survey). The survey sampled clusters in tehsils that are primarily rural and was stratified to cover all regions of Punjab except selected areas of West Punjab which had been hit by major floods in 2010. The survey and sampling strategy are also described in greater detail in Ahmed, Amjad, Habib, and Shah (2013) and Chaudhry and Vyborny (2013).

The first round of the survey took place in late 2007-early 2008, spanning 6 months before the 2008 national election, and the second round took place in April 2011, 18 months before the 2013 national election. The sample includes 13 national legislative constituencies; of these, the incumbent was re-elected in seven.

Table 1 shows basic characteristics of the households who were surveyed in the followup round. The first two panels include the two categories of targeted transfer programs used in this paper as dependent variables. Because of the small sample size and low coverage of these programs, as well as concerns about measurement error, I use a binary measure of whether a household received either of these two forms of assistance as the dependent variable in the analysis.

The sample mean for these two categories changes between rounds as the national and provincial governments scaled back some assistance programs and rolled out others.

The first two panels of Table 1 also shows summary statistics for the shared clan variable used as an independent variable. Households were asked to report their own clan / caste ("zaat" or "biradari"). In general they did so readily, as these kinship groups are salient and publicly known in villages in Punjab. I use two sources of information to determine whether a household shares the clan of a politician.

First, households were asked about a set list of potential influentials. If the respondent knew an influential person, he/she was asked to report the clan of that individual. National politicians (Members of National Assembly or MNAs) were named frequently; in every constituency at least one respondent mentioned the politician. We used the reports from all households in the constituency to determine the politician's clan. In addition, research assistants at the Lahore School of Economics worked through current and historical news reports and informed contacts to identify the clan or caste of politicians. Because group and sub-group identities can overlap, I assume a household shares the

politician's clan if any source indicates a match. For politicians' opponents, only these secondary sources were available, so we only have this variable for about half the sample.

The third panel of Table 1 shows basic household characteristics for the whole panel sample. The wealth index used here is a standardized z-score based on household asset ownership.[2]

Of the 1,024 selected households, 924 were successfully surveyed (an attrition rate of 10%). Table 2 shows that attriters have less land and assets. It also shows that attrition is higher for areas and households which receive more government education transfers. This effect is likely driven by geographic factors, since the biggest education transfer program is targeted at low-literacy districts, where economic opportunities are lower and outmigration higher as a result.

Unfortunately I cannot test whether attrition is correlated with our independent variable of interest (clan), because this information was not asked in the baseline survey. Based on other observables, it is possible that households who are not the politician's co-clan members are more likely to attrit. However, with the fixed effects estimator, this would only bias results if both (a) households are more likely to attrit when their co-clan members leave office, and (b) these attriters receive more government transfers than the non-attriters, which seems unlikely.

Table 3 shows descriptive regressions identifying the correlates of program targeting. Both landlessness and illiteracy are correlated with the unconditional cash transfer program, which is consistent with program guidelines. Conditional on these, other wealth measures are not significant. The education programs are targeted as expected, with target districts and households with children ever enrolled more likely to report these programs. Surprisingly, households with literate heads are significantly less likely to receive educational assistance (even conditional on target district status for the girls' CCT).

Figure 1 shows the distribution of clans in the sample. The largest clan is only 11% of the sample; clans with fewer than 1.5% of the sample are grouped together in the "other" category. Comparing this to Table 1, which shows that on average 18% of the sample in a constituency shares the politician's clan, we can tell that politicians tend to come from areas where their clan is more concentrated. Figure 2 shows the proportion of the sample that are politician co-clan members by constituency. There is a great deal of variation; in some constituencies, no sampled household shared the clan of the politician, in others the majority of the sample are of the same clan as the politician, while most are in between.

We use the percentage vote difference between the winner and runner-up of the MNA seat as a measure of electoral competitiveness. Figure 3 shows the distribution of this variable. It is highly

[2]This was calculated by the UNICEF - Punjab government MICS survey office with a standard methodology used across countries. The mean is not zero in our sample because it is standardized across the whole MICS sample, of which this survey is a sub-sample.

Table 1: **Descriptive Statistics on Panel Households**

		Round 1 (2007)			
Need-based cash transfer	924.00	0.01	0.10	0.00	1.00
Scholarship / education benefits	924.00	0.23	0.42	0.00	1.00
Any targeted assistance program (cash transfer or education benefits)	924.00	0.24	0.43	0.00	1.00
Shares national politician clan	924.00	0.18	0.39	0.00	1.00
		Round 2 (2011)			
Needs-based cash transfer	924.00	0.03	0.17	0.00	1.00
Scholarship / education benefits	924.00	0.07	0.25	0.00	1.00
Any targeted assistance program (cash transfer or education benefits)	924.00	0.09	0.29	0.00	1.00
Shares national politician clan	924.00	0.18	0.38	0.00	1.00
		HH characteristics			
Female head	924.00	0.07	0.26	0.00	1.00
Years owned land in village	924.00	11.55	25.72	0.00	200.00
Land owned (acres)	922.00	2.19	5.37	0.00	80.00
Landless	924.00	0.60	0.49	0.00	1.00
House value (PKR)	924.00	1,784.32	3,415.01	0.00	50,000.00
HH head literate	924.00	0.64	0.48	0.00	1.00
Any child below 14 years old ever enrolled in school	924.00	0.60	0.49	0.00	1.00
Wealth score	924.00	-0.46	0.73	-1.92	1.41
		District and constituency characteristics			
Scholarship district	924.00	0.53	0.50	0.00	1.00
Vote margin	924.00	6.88	5.64	0.23	21.36

variable, although the second election, in 2008, was noticeably more competitive.

Table 4 shows the characteristics of households who are ever represented by a politician of the same clan (in either round of the panel). These households are significantly wealthier, more educated, and longer established in the community. As discussed in Section 4, this could suggest either an upward or downward bias on the OLS estimate of γ. As Table 4 shows, elite clans are significantly less likely to receive government education benefits in the first round of the panel, despite having more children in school. In the second round this effect has disappeared. If anything they appear to be more likely to receive both types of assistance programs in the second round, although this difference is not significant.

Table 2: **Correlates of Attrition**

	Mean (Non-Attriters)	Mean (Attriters)	Difference
Needs-based cash transfer	0.01	0.01	-0.00
Scholarship or educational benefits	0.23	0.14	0.09 *
House value (USD)	2590.82	2039.23	551.59
Years HH has owned land in village	11.55	0.00	11.55 ***
Acres of land HH owns	3.43	2.16	1.27
Landless	0.54	0.67	-0.14 **
HH head literate	0.58	0.50	0.07
First (poorest) wealth quartile	0.24	0.38	-0.14 **
Second wealth quartile	0.26	0.19	0.07
Third wealth quartile	0.25	0.27	-0.02
Fourth (richest) wealth quartile	0.26	0.16	0.10 *
Female headed HH	0.05	0.03	0.02
District with girls' stipend CCT program	0.53	0.71	-0.18 ***
Observations	924	100	

Figure 1: **Distribution of clans in sample**

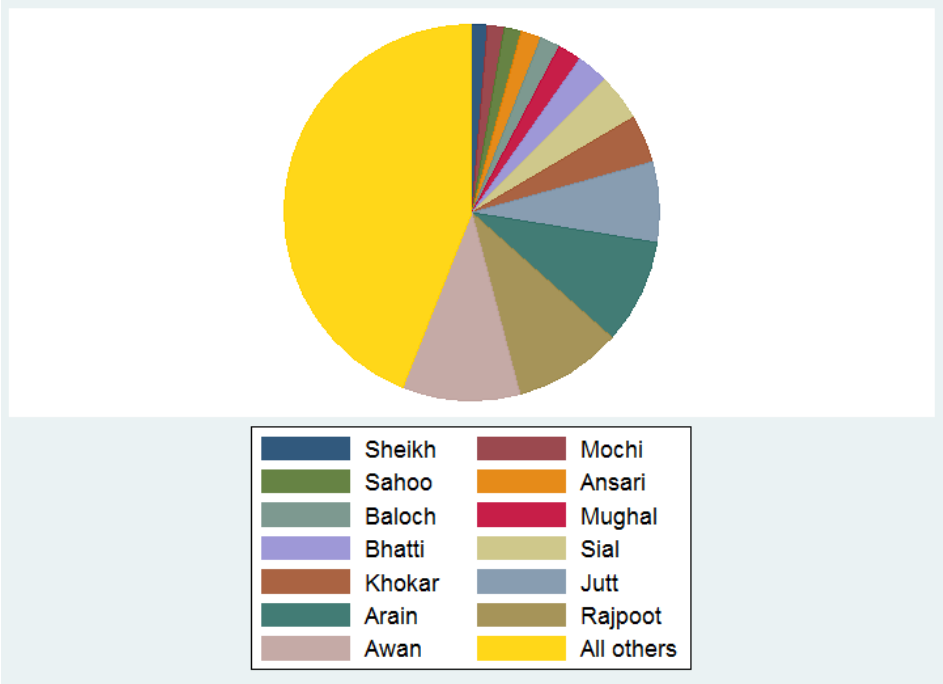


Table 3: **Correlates of assistance program targeting**

	(1)	(2)	(3)	(4)
	Needs-based cash transfer		Scholarship or educational benefits	
	Full sample	Non-politician clans	Full sample	Non-politician clans
Female headed HH	0.03	0.02	0.04	0.02
	(0.02)	(0.02)	(0.04)	(0.04)
Years HH has owned land in village	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Acres of land HH owns	-0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Landless	0.01	0.02**	0.00	-0.01
	(0.01)	(0.01)	(0.02)	(0.03)
House value (USD)	0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
First (poorest) wealth quartile	0.01	0.00	-0.01	0.01
	(0.01)	(0.01)	(0.02)	(0.03)
Second wealth quartile	0.01	0.00	0.00	0.02
	(0.01)	(0.01)	(0.02)	(0.03)
Third wealth quartile	0.01	0.01	0.03	0.02
	(0.01)	(0.01)	(0.02)	(0.02)
HH head literate	-0.01**	-0.01	-0.05**	-0.05**
	(0.01)	(0.01)	(0.02)	(0.03)
Any child in HH ever attended school	0.00	0.00	0.12***	0.13***
	(0.01)	(0.01)	(0.01)	(0.02)
District with girls' stipend CCT	-0.00	0.00	0.09***	0.08**
	(0.01)	(0.01)	(0.03)	(0.03)
Vote margin (percent)	-0.00	0.00	0.00	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)
Constant	0.01	-0.00	0.02	-0.01
	(0.01)	(0.01)	(0.03)	(0.04)
Sample mean	0.02	0.02	0.15	0.16
Observations	1821	1399	1821	1399

Dependent variable is a dummy for whether the household received the program (needs-based transfer in columns 1-2, education benefit in columns 3-4). Columns 1 and 3 show linear probability model results for the full sample; columns 2 and 4 show linear probability model for the subset of households who are of a different clan than either their 2002 or 2008 elected politician. Standard errors in parentheses, clustered by clan x constituency. * $p < .1$, ** $p < .05$, *** $p < .01$

Figure 2: **Concentration of clans in politicians' constituencies**

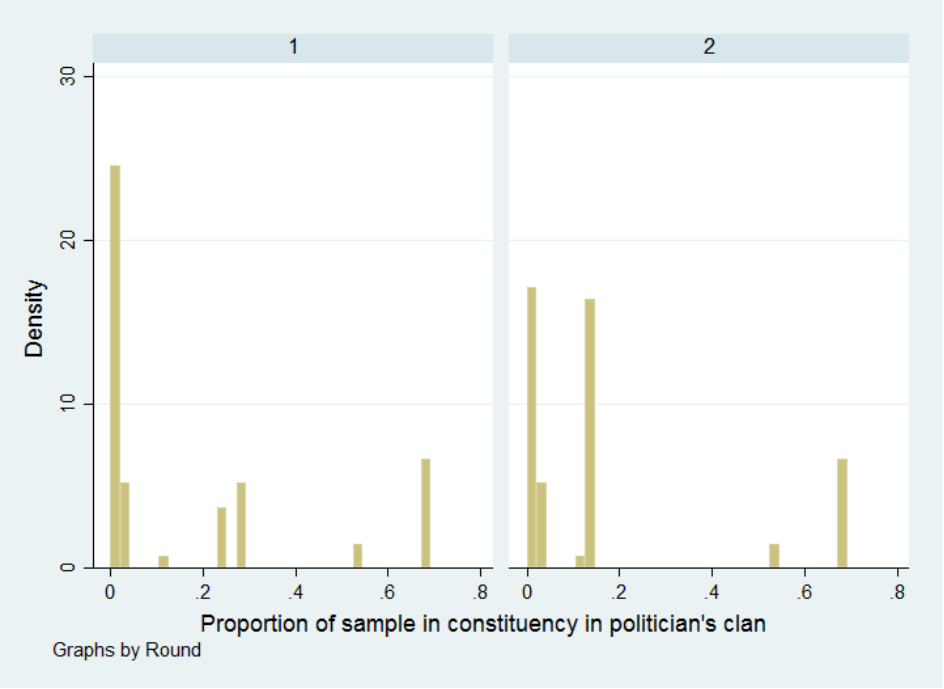


Table 4: **Clan and HH characteristics**

	(1)		
	Mean (Other clans)	Mean (Politician clans)	Difference
	Round 1 (2007)		
Needs-based cash transfer	0.01	0.01	0.00
Scholarship or educational benefits	0.25	0.16	0.09 **
Household received either assistance program	0.17	0.14	0.04
Observations	684	240	
	Round 2 (2011)		
Needs-based cash transfer	0.02	0.04	-0.02
Scholarship or educational benefits	0.06	0.09	-0.03
Observations	684	240	
	HH Characteristics		
House value (USD)	2339.73	3267.72	-927.99 ***
Years HH has owned land in village	10.12	15.64	-5.52 **
Acres of land HH owns	3.14	4.26	-1.12 *
Landless	0.57	0.45	0.12 **
HH head literate	0.55	0.65	-0.11 **
First (poorest) wealth quartile	0.28	0.12	0.16 ***
Second wealth quartile	0.25	0.26	-0.01
Third wealth quartile	0.24	0.27	-0.03
Fourth (richest) wealth quartile	0.23	0.35	-0.12 ***
Female headed HH	0.05	0.06	-0.01
District with girls' stipend CCT program	0.65	0.18	0.47 ***
Years of education (most educated HH member)	6.27	7.58	-1.32 ***
Any child below 14 years in HH ever attended school	0.75	0.86	-0.11 **
Wealth index score	-0.54	-0.23	-0.31 ***
Observations	684	240	

6 Results

6.1 Effect of Clan Connection

Table 5 shows the results of a "naive" random effects estimate, with only household level controls. As discussed in Section 4, the direction of bias on this estimate is ex ante ambiguous. The point estimate of γ is zero.

Table 6 shows the results of the panel estimation. Column 1 shows my preferred specification, i.e. Equation 2. Columns 2 and 3 show the estimates with clan and household fixed effects, respectively. I also report standard errors clustered by alternative groups: village (64 clusters), clan x constituency (312 clusters), and constituency (13 clusters).

The results are striking. The estimate of γ is large, significant, and stable across specifications. It corresponds to a 70% increase over the mean for the "control group," i.e. households who are not represented by a co-clan member. This means that households represented by a co-clan member are at least twice as likely to receive assistance.

Table 5: **Results: "Naive" specification**

	(1)
	HH received any assistance program
HH represented by a co-clan politician	-0.00
	(0.04)
Round	-0.16***
	(0.04)
Controls	YES
Observations	1846

Standard errors in parentheses, clustered by clan x constituency.
* $p < .1$, ** $p < .05$, *** $p < .01$

6.2 Robustness checks

6.2.1 Close election subset

As a robustness check, I test the same specification with the subset of constituencies where the vote margin between winner and runner-up was less than ten percent. This adds another element of identification: by using close elections only, the results are similar to the combined regression discontinuity and difference-in-difference approach in Chapter 4. Table 7 shows the results, which are significant and similar in magnitude to the main specification.

Figure 3: **Electoral competitiveness in the PERI sample**

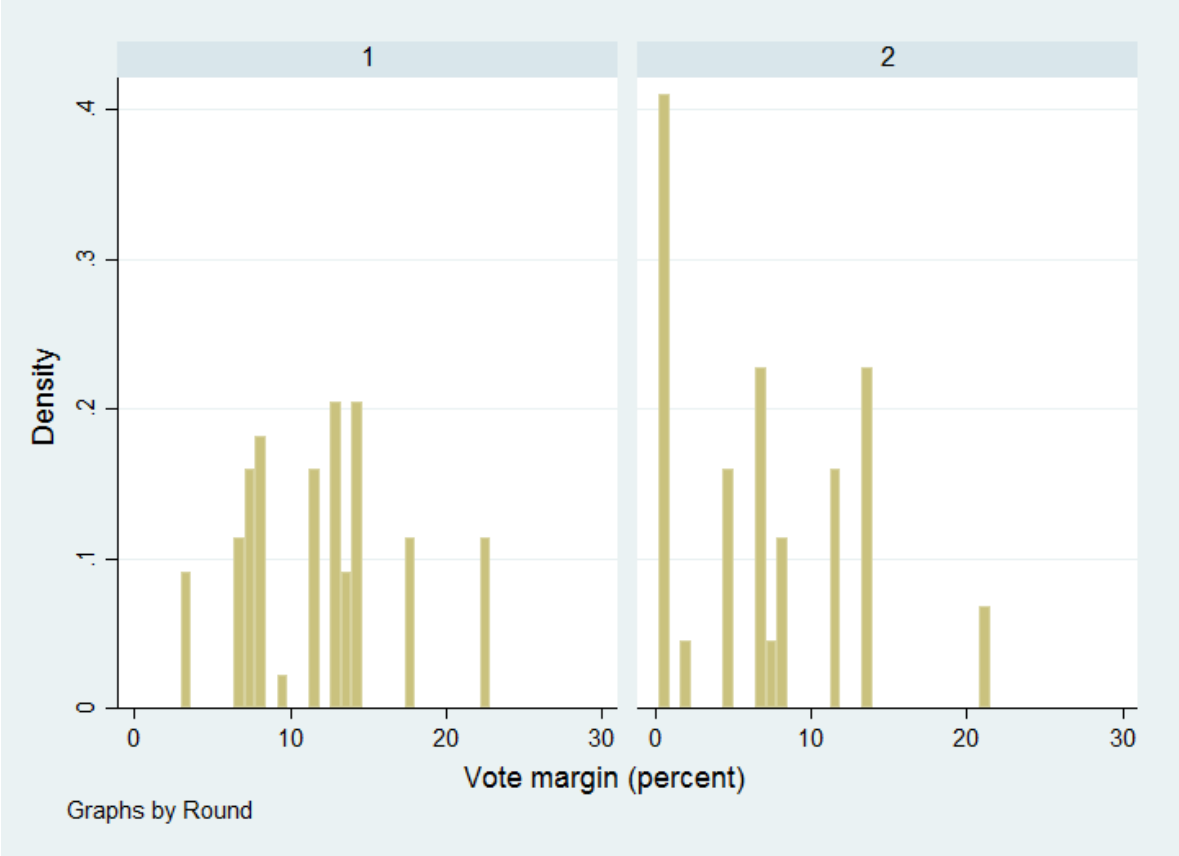


Table 6: **Results: Politician clan and government assistance**

	(1)	(2)	(3)
	HH received any assistance program		
HH represented by a co-clan politician	0.12	0.12	0.11
SE clustered by village	(0.07)*	(0.07)*	(0.06)*
SE clustered by clan x constituency	(0.09)	(0.09)	(0.08)
SE clustered by constituency	(0.06)*	(0.07)*	(0.06)*
Round	-0.16	-0.15	-0.15
	(0.04)***	(0.05)***	(0.04)***
Ever represented by a co-clan politician	-0.01	-0.02	
	(0.04)	(0.05)	
HH represented by a co-clan politician in both rounds	-0.16	-0.14	
	(0.05)***	(0.07)**	
Controls	YES	YES	
Clan FE		YES	
HH FE			YES
"Control group" mean		0.17	
Observations	1846	1818	1848

Standard errors in parentheses, clustered at village level except where indicated.
* $p < .1$, ** $p < .05$, *** $p < .01$

Table 7: **Results: Close election subset**

	(1)
	HH received any assistance program
HH represented by a co-clan politician	0.14
SE clustered by village	(0.08)*
SE clustered by clan x constituency	(0.10)
SE clustered by constituency	(0.09)
Round	-0.18
	(0.06)***
Ever represented by a co-clan politician	-0.02
	(0.04)
HH represented by a co-clan politician in both rounds	0.01
	(0.08)
Controls	YES
Observations	1003

Standard errors in parentheses, clustered at village level except where indicated.
* $p < .1$, ** $p < .05$, *** $p < .01$

6.2.2 Group time trends

The main specification addresses any time-invariant characteristics of a clan that may cause a higher or lower probability of receiving targeted assistance. However, it is possible that time-variant characteristics could bias the estimate of γ.

Given that there were significant changes in the overall funding and distribution of targeted assistance in the 2000's, it is possible that changes in overall geographic distribution and targeting could be increase assistance to clans prevalant in particular areas.

Alternatively, if a particular clan becomes more economically or socially powerful over time, and this affects their ability to get assistance directly, as well as their ability to elect a co-clan member to office, this could bias the estimate of γ upwards.

To address these possibilities, I estimate Equation 2 with district-specific and clan-specific time trends. Table 8 shows that the results are robust to both types of time trends.

Table 8: **Robustness check: District and Clan Time Trends**

	(1)	(2)	(3)
	HH received any assistance program		
HH represented by a co-clan politician	0.12	0.10	0.19
SE clustered by village	(0.07)*	(0.06)	(0.10)*
SE clustered by clan x constituency	(0.09)	(0.08)	(0.11)*
SE clustered by constituency	(0.06)*	(0.06)*	(0.13)
Round	-0.16	-0.02	-0.97
	(0.04)***	(0.07)	(0.05)***
Ever represented by a co-clan politician	-0.01	0.01	-0.06
	(0.04)	(0.05)	(0.06)
HH represented by a co-clan politician in both rounds	-0.16	-0.20	-0.17
	(0.05)***	(0.05)***	(0.08)**
Controls	YES	YES	YES
District time trends		YES	
Clan time trends			YES
Observations	1846	1846	1818

Standard errors in parentheses, clustered at village level except where indicated.
* $p < .1$, ** $p < .05$, *** $p < .01$

Table 9: **Robustness to logit model**

	(1)	(2)
	HH received any assistance program	
	Coefficients	Marginal effects
HH represented by a co-clan politician	0.95	0.14
SE clustered by village	(0.56)*	(0.09)
SE clustered by clan x constituency	(0.76)	(0.13)
SE clustered by constituency	(0.57)*	(0.09)
Round	-1.27	-0.15
	(0.32)***	(0.04)***
Ever represented by a co-clan politician	-0.14	-0.02
	(0.43)	(0.05)
HH represented by a co-clan politician in both rounds	-1.27	-0.10
	(0.34)***	(0.02)***
Controls	YES	

Standard errors in parentheses, clustered at village level except where indicated.
* $p < .1$, ** $p < .05$, *** $p < .01$

6.2.3 Robustness to logit model

The specifications above are all adaptations of the linear probability model to panel data. Table 9 shows the preferred specification estimated with a logit model instead. The coefficients are significant; marginal effects are insignificant but similar in magnitude to the linear coefficients.

6.3 Testing the Predictions of the Model

The theoretical model in Chapter 1 allows social connections to affect both the strength of a politician's altruism towards a voter, and the ease of making a clientelistic bargain with him. This results in a nepotistic "inner circle" of family and friends to whom the politician gives intense assistance, regardless of electoral pressure.

If a politician faces electoral pressure, he will give out more assistance overall. In addition to his inner circle, his more distant connections benefit, from clientelism - only because he needs their votes. If the politician's clan is too small to make up a winning bloc, then unaligned clans - those closer to the center of the spectrum, with no close connection to either the politician or his opponent - will receive clientelistic payments. How much of the electorate receive these payments - and how distant

the social connections - depends on the cost of making an alliance with an individual group. However, in all cases, those connected to the opponent will still receive nothing.

6.3.1 Clientelism and Electoral Competitiveness

We first test whether politicians distribute more assistance in areas where they face greater electoral pressure. Table 10 shows the results. Households in the politician's clan are an estimated 4.5 percentage points more likely to receive unconditional cash transfers. However, a ten percentage point increase in the vote margin would reduce this effect by two percentage points - almost half. Similarly, these households are an estimated 21.4 percentage points more likely to receive educational benefits; but a ten percentage point increase in the vote margin would eliminate this effect entirely. This is strong evidence that a large part of the clan effect was due to clientelism.

6.3.2 The politician's inner circle

I next test whether an "inner circle" of politician's connections receive greater assistance, as predicted by the model of in-group altruism. The theoretical model predicts that the "inner circle" will always benefit, while those outside the inner circle will only benefit in competitive election settings.

To test this, I construct a dummy for whether the household reports having telephone, home or office access to meet the politician. Figure 4 shows that co-clan members appear to be slightly more likely to have this access, but the difference is not significant.

Because this data is only available for the second round, I split the connection term (L) into two, one for politicians in office in the first round only, and one for those in the second round only. The interaction terms shown here are for the second round only. Tables 11 shows the results of interaction terms,

The results are consistent with the model. The politician's closest connections, households who are of the same clan and have access to him, receive by far the most assistance, and this does not vary at all with electoral competition. Members of other clans who have access benefit primarily when the election is a landslide, perhaps suggesting an expansion of the "inner circle"[3] Members of the same clan who do not have access benefit primarily when the election is close; they appear to be receiving clientelism, only when their votes are needed.

There are two potential problems with this measure of access, as with any self-reported network links. First, households could report access differentially; if households who received assistance are

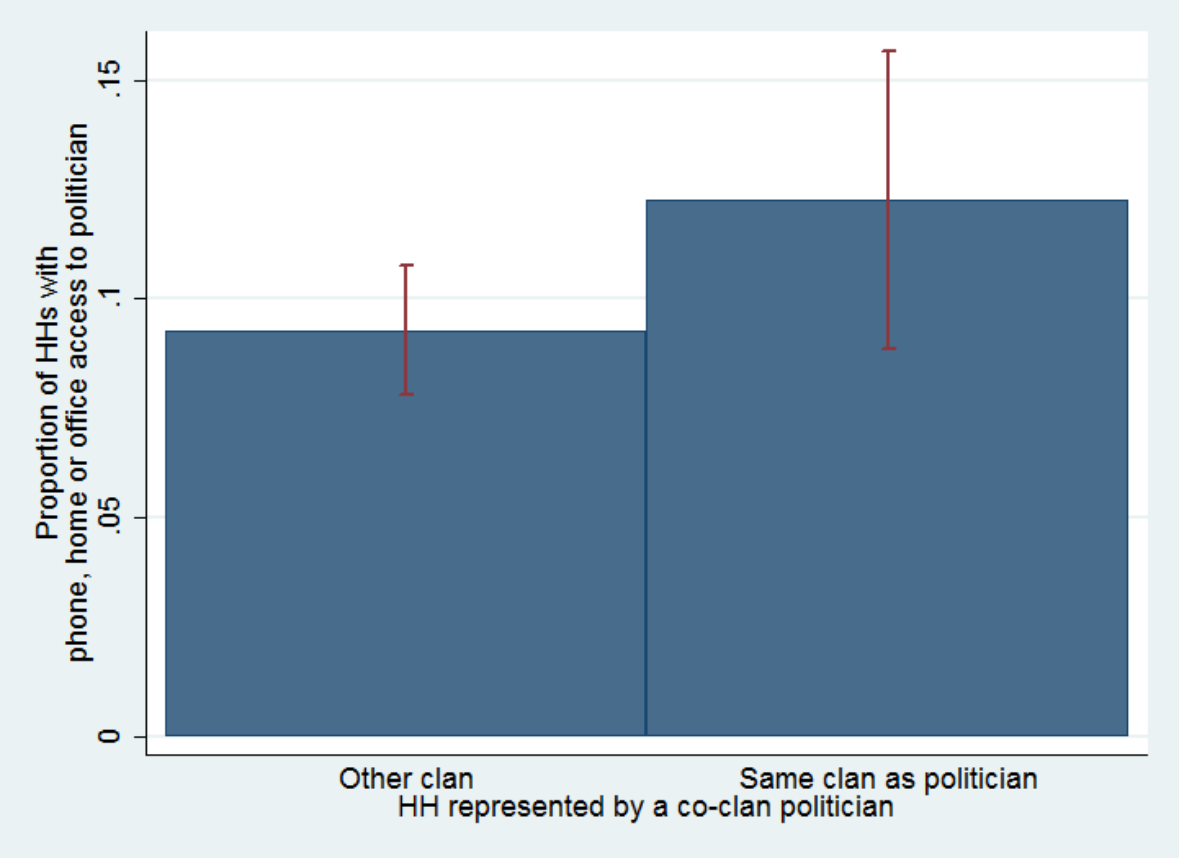
[3]Although this is not predicted in the model, it could be incorporated in an extension to the model.

Table 10: **Mechanisms: Electoral Competitiveness**

	(1)	(2)	(3)
	HH received any assistance program		
HH represented by a co-clan politician	0.31	0.32	0.36
SE clustered by village	(0.07)***	(0.08)***	(0.07)***
SE clustered by clan x constituency	(0.10)***	(0.10)***	(0.10)***
SE clustered by constituency	(0.08)***	(0.07)***	(0.08)***
Vote margin (percent)	0.00	0.00	0.01
SE clustered by village	(0.00)	(0.00)	(0.00)*
SE clustered by clan x constituency	(0.00)	(0.00)	(0.00)***
SE clustered by constituency	(0.01)	(0.01)	(0.01)
HH represented by a co-clan politician x Vote margin	-0.02	-0.02	-0.03
SE clustered by village	(0.00)***	(0.00)***	(0.01)***
SE clustered by clan x constituency	(0.00)***	(0.00)***	(0.00)***
SE clustered by constituency	(0.01)***	(0.01)***	(0.01)***
Round	-0.18	-0.17	-0.15
	(0.04)***	(0.04)***	(0.04)***
Ever represented by a co-clan politician	-0.01	-0.03	
	(0.04)	(0.05)	
HH represented by a co-clan politician in both rounds	-0.17	-0.10	
	(0.04)***	(0.06)*	
Controls	YES	YES	
Clan FE		YES	
HH FE			YES
Observations	1846	1818	1848

Standard errors in parentheses, clustered at village level except where indicated.
* $p < .1$, ** $p < .05$, *** $p < .01$

Figure 4: **Clan and access to politician**



more likely to report a connection this could bias our estimates (Comola and Fafchamps, 2014). Second, access could occur endogenously, if households seeking assistance find ways to contact the politician.

The survey question was designed to address the first problem by asking an objective question on phone or home access to the politician. This should be less subject to response bias than asking if the respondent could go to him for assistance, as in the network measures discussed by Comola and Fafchamps (2014).

To address the second problem, I examine further questions from the survey. Households who said they knew the politician were asked whether they received assistance from him for a range of possible purposes, including assistance with children's education and assistance to get a government assistance program such as BISP or zakat. In the whole sample of 924 panel households, only *one* household reported any assistance from the politician in each of these categories. So it seems very unlikely that households' strategic link formation completely explains this pattern of results.

Table 11: **Results: Clan and access to politician**

	(1)	(2)
	HH received any assistance program	
Same clan and access to politician	0.38	0.38
SE clustered by village	(0.12)***	(0.15)***
SE clustered by clan x constituency	(0.13)***	(0.15)**
SE clustered by constituency	(0.12)***	(0.14)***
Same clan and access to politician X vote margin		0.00
SE clustered by village		(0.03)
SE clustered by clan x constituency		(0.01)
SE clustered by constituency		(0.01)
Access to politician different clan	0.18	0.07
SE clustered by village	(0.08)**	(0.09)
SE clustered by clan x constituency	(0.07)***	(0.08)
SE clustered by constituency	(0.09)*	(0.10)
Access to politician different clan X vote margin		0.01
SE clustered by village		(0.01)**
SE clustered by clan x constituency		(0.01)**
SE clustered by constituency		(0.01)**
Same clan no access	0.24	0.28
SE clustered by village	(0.06)***	(0.06)***
SE clustered by clan x constituency	(0.08)***	(0.07)***
SE clustered by constituency	(0.08)***	(0.08)***
Same clan no access x vote margin		-0.01
SE clustered by village		(0.01)*
SE clustered by clan x constituency		(0.00)**
SE clustered by constituency		(0.01)
Controls	YES	YES
Group dummies	YES	YES
Observations	1852	1852

Standard errors in parentheses, clustered at village level except where indicated. * $p < .1$, ** $p < .05$, *** $p < .01$ Coefficients shown are for the connection during round 2. Group dummies include time-invariant dummies for each group listed, for households connected by clan during round 1, and a round dummy.

6.3.3 Local patrons

The theoretical model in Chapter 1 allows for politicians to work through local patrons. These patrons organize their bloc to vote for the politician, and in exchange, they play a role in selecting recipients for public assistance. The patrons then distribute the assistance to their own connections within the bloc.

I apply this idea to the *numberdar*, a local position which was historically appointed to a local official administrative role under the British, and subsequently during local government appointments in the Musharraf era. After the Musharraf era the local government system was not officially maintained. However, these officials still maintained an important role in communities, and they were among the most frequently identified officials in our survey. During the period covered by the panel dataset these officials would not have changed or been re-appointed. Their role is explored more in Chapter 2.

Here I test whether a shared link with both the politician and the local official leads to greater assistance. Table 12 shows the results. The same-clan effect is driven completely by households who are co-clan members of both the politician and the local official. Note that this cannot be due to the effect of the local official on his own: the term for the official has zero effect. The interaction effect for relatives is large in magnitude but not statistically significant. These results are consistent with the theoretical model: local officials seem to play an important role as gatekeepers.

6.4 Mechanisms and implications

The results shown in the preceding section shed some light on the mechanisms of the connection effect.

It is possible that the politician directly contacts program implementers to ensure that his connection get access. Alternatively, a politician may assist his connections in addressing problems in implementation, such as problems with payments not being delivered. However, almost no households attribute assistance to the politician in receiving these programs, which makes these explanations seem unlikely.

A second possible mechanism is that information passes through the politicians' networks - including local officials - on how to apply for programs. Another is that the local officials interfere with local administration of programs. This could occur in programs that rely on reporting of information from local institutions, such as the attendance reports from local schools. In the next subsection we test this mechanism.

Another possibility is that program implementers, such as bureaucrats, may know that households

Table 12: Mechanisms: Local Official

	(1)	(2)	(3)	(4)
	HH received any assistance program			
HH represented by a co-clan politician	0.12	0.13	-0.01	-0.02
SE clustered by village	(0.07)*	(0.07)**	(0.08)	(0.08)
SE clustered by clan x constituency	(0.09)	(0.09)	(0.10)	(0.10)
SE clustered by constituency	(0.06)*	(0.07)*	(0.07)	(0.07)
HH in village with co-clan local official		-0.01	-0.01	-0.01
SE clustered by village		(0.03)	(0.03)	(0.03)
SE clustered by clan x constituency		(0.03)	(0.03)	(0.03)
SE clustered by constituency		(0.02)	(0.02)	(0.02)
HH shares clan of politician and local official			0.31	0.31
SE clustered by village			(0.10)***	(0.10)***
SE clustered by clan x constituency			(0.11)***	(0.11)***
SE clustered by constituency			(0.09)***	(0.09)***
HH related to local official				0.01
SE clustered by village				(0.07)
SE clustered by clan x constituency				(0.07)
SE clustered by constituency				(0.07)
HH represented by co-clan politician and related to local official				0.16
SE clustered by village				(0.25)
SE clustered by clan x constituency				(0.24)
SE clustered by constituency				(0.24)
Round	-0.16	-0.17	-0.16	-0.16
	(0.04)***	(0.05)***	(0.05)***	(0.05)***
Controls	YES	YES	YES	YES
Group dummies	YES	YES	YES	YES
Observations	1846	1664	1664	1664

Standard errors in parentheses, clustered at village level except where indicated.
* $p < .1$, ** $p < .05$, *** $p < .01$

are well-connected, and make sure they get preferential access. We do not have data to test this mechanism directly. However, this may be the most probable mechanism given the evidence against other possible channels.

6.4.1 Information and persistence

As shown in Table 13, the effect of being represented by a member of the same clan does not outlast the politician's term in office. This table shows the basic specification, with the coefficient of interest split to allow estimation of heterogenous effects between households who were represented by a member of the same clan in period 1 but not in period 2, and vice versa. If households were enrolled in an assistance program with the assistance of a politician, and then remained enrolled after that politician left office, we would expect the incoming politician effect to be larger. In fact the results are the opposite. We cannot reject that the two are equal ($p = 0.22$ in HH FE specification).

If politicians only shared information with their networks on how to get enrolled, but did not actively assist their own clan members (or block others) from receiving them, we would expect to see persistent effects and no impact for households whose clan member leaves office - contrary to these results.

Since there is a change over time in the overall availability and spread of different assistance programs, however, this is not a perfect test of this mechanism. Ideally, I would conduct this test with only one program which was continuously available throughout. Unfortunately, this sample is too small to reliably test the effects on individual programs, and even the programs which have run continuously during this period have not been equally well funded at all points in time.

6.4.2 Who benefits?

As Figures 5 and 6 show, the clan effect appears not to be driven primarily by selection or rationing within an eligible pool of recipients. There is no clear pattern of poorer or richer same-clan households being selected: it appears that both eligible and ineligible households are receiving the programs.

6.4.3 Winners, opponents and punishment

In addition to rewarding their connections, winning politicians might punish their opponents' connections, as in Fafchamps and Labonne (2013) and Hsieh, Miguel, Ortega, and Rodriguez (2011).

The model in Chapter 1 also predicts that the connections of opponents would receive less assistance than unconnected voters, who may be contested as swing voters. I obtained data on the opponent's

Table 13: **Results: Persistence of effects**

	(1)	(2)	(3)
	HH received any assistance program		
HH represented by a co-clan politician - round 1 only	0.18***	0.17***	0.22***
	(0.06)	(0.06)	(0.06)
HH represented by a co-clan politician - round 2 only	0.06	0.06	0.00
	(0.11)	(0.12)	(0.16)
HH represented by a co-clan politician in both rounds	-0.05	-0.02	
	(0.06)	(0.09)	
Ever represented by a co-clan politician	-0.01	-0.03	
	(0.06)	(0.07)	
Round	-0.15***	-0.15***	-0.12***
	(0.04)	(0.04)	(0.04)
Controls	YES	YES	
Clan FE		YES	
HH FE			YES
Observations	1824	1824	1824

Standard errors in parentheses, clustered at clan x constituency level.
* $p < .1$, ** $p < .05$, *** $p < .01$

Figure 5: **Connections and targeting: HH received any assistance program**

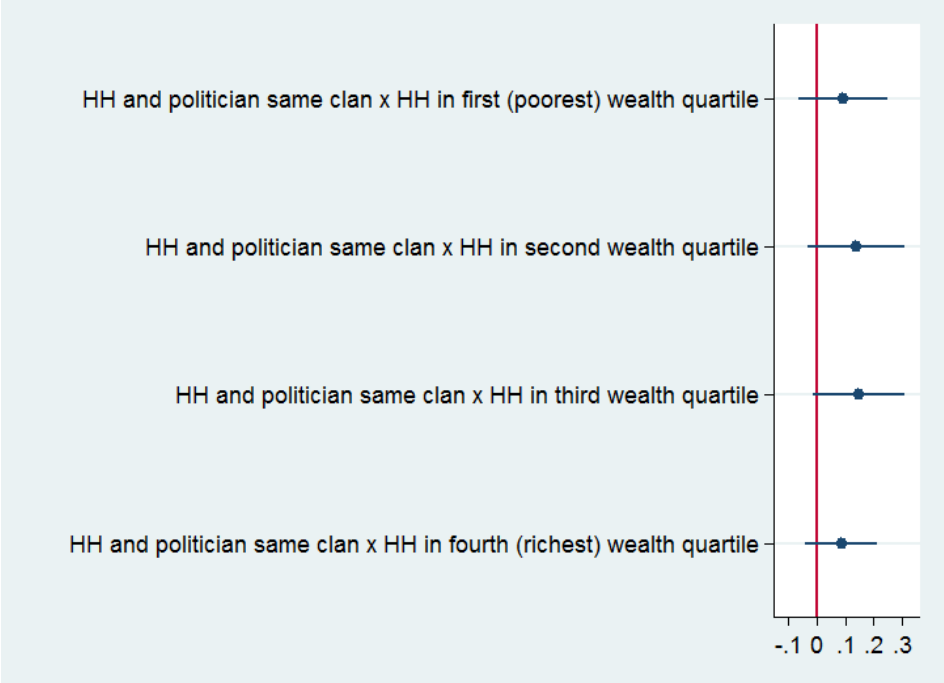
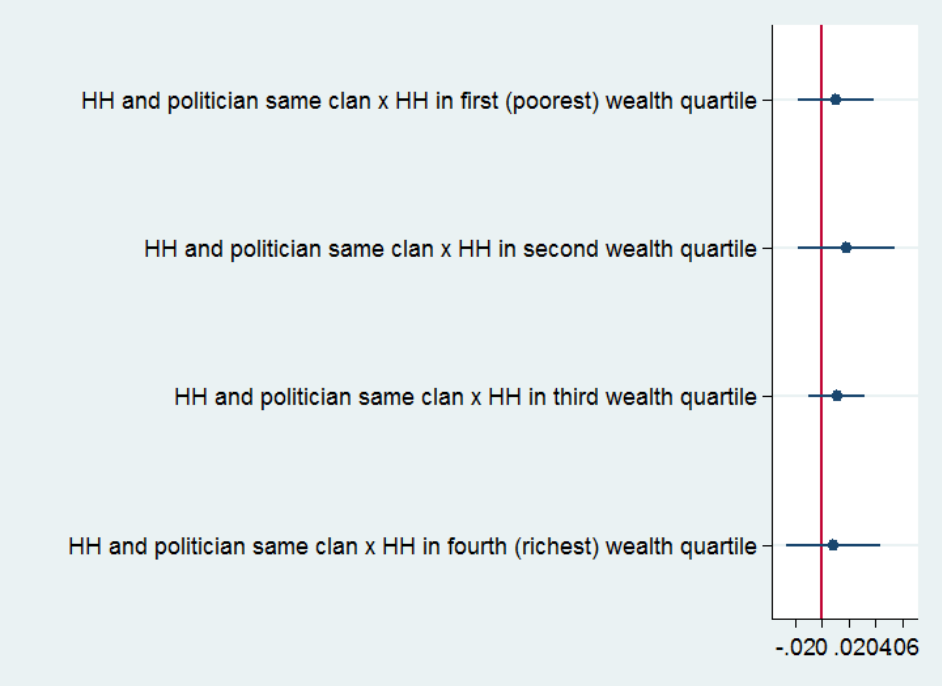


Figure 6: **Connections and targeting: HH received unconditional cash transfer**



clan for some constituencies (seven constituencies for the 2002 election, five constituencies for the 2008 election). Table 14 shows results for the households in these constituencies. The results are consistent with the model and with the earlier empirical literature on punishment: the opponents' connections do worse than unconnected households.

7 Conclusions

The panel estimates in this paper identify the effect for households in constituencies where the legislator changed in the 2008 election, not those where the incumbent was re-elected. This may imply selection for legislators who are politically weaker or less adept, who may be less able to deliver government goods to their constituents and connections. This would imply that the estimate of γ is lower in the panel model than the average effect for the population. On the other hand, it may be that constituencies where the incumbent lost tend to be more tightly contested, and therefore politicians give out more transfers to their potential supporters, leading to an estimate of β_2 that higher than the average in the population. Overall, however, 75% of constituencies were in this category in the 2008 election. So the effect of connections in contested constituencies is also arguably an effect of interest in its own right.

Table 14: **Winners and opponents**

	(1)	(2)
	HH received any assistance program	
HH represented by a co-clan politician	0.12	0.11
SE clustered by village	(0.07)*	(0.10)
SE clustered by clan x constituency	(0.09)	(0.08)
SE clustered by constituency	(0.06)*	(0.09)
HH of runner-up clan		-0.26
SE clustered by village		(0.23)
SE clustered by clan x constituency		(0.04)***
SE clustered by constituency		(0.12)**
Round	-0.16	-0.14
	(0.04)***	(0.07)**
Ever represented by a co-clan politician	-0.01	0.04
	(0.04)	(0.10)
HH represented by a co-clan politician in both rounds	-0.16	-0.18
	(0.05)***	(0.05)***
Ever shares opponent clan		0.15
		(0.21)
Constant	0.32	0.26
	(0.10)***	(0.16)*
Observations	1846	949

Standard errors in parentheses, clustered at village level except where indicated.
* $p < .1$, ** $p < .05$, *** $p < .01$

The empirical findings both give a well-identified estimate of the causal effect of social connections to politicians on targeted assistance. In conjunction with the theoretical model, they also yield a better understanding of the patterns of assistance within connected groups, and the motivations for politicians' influence - both nepotistic and clientelistic.

These results help advance the theoretical and academic literature on nepotism and clientelism by looking at them in conjunction. They are also of use for policymakers in making public service delivery decisions, to take into account how local political and social relationships affects the targeting and delivery of these programs.

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Bringing home the benefits:

Politicians' influence on government assistance and the effect of targeting reform

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Abstract

Assistance programs for poor communities and individuals are an important component of public sector activities in developing countries. Yet funders of these programs, whether donors or national taxpayers, may be concerned about potential misuse of the money. Donor agencies often try to influence program design to reduce mistargeting. But there is limited evidence on whether this is effective. In this paper, we investigate whether politicians direct more assistance to households in their home villages. We focus on targeted government assistance programs, including social safety net and education programs. We also test whether that effect differs in programs with different targeting mechanisms. We carried out a unique household survey in politicians' home villages in rural Punjab, Pakistan. We exploit close elections to compare households in the home villages of close winner and close runner-up politicians. We use a combination Regression Discontinuity - Difference-in-Differences approach, comparing the connections of just-winners and just-losers, before and after an election. This provides stronger identification than either the RD or DD strategy used separately. We find that having a politician from one's village elected more than doubles the amount of government assistance received. We explore the distribution of assistance within the village and find that it is broad-based, not limited to the politician's own clan. We then exploit a major reform to the targeting mechanism of a cash transfer program as a natural

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experiment. After the reform, households in winners' home villages are still significantly more likely to receive the transfer; however, many more households receive it in the losers' home villages as well. We explore both how and why politicians influence targeting. We also document three methodological measures we have taken to ensure our results are robust against potential concerns about data mining. Our results have implications for donor agencies and civil society organizations who aim to engage with governments to reduce corruption and improve public spending.

1 Introduction

Assistance programs for poor communities and individuals are an important component of public sector activities in developing countries. Yet funders of these programs, whether donors or national taxpayers, may be concerned about potential misuse of the money. Donor agencies often try to influence program design to reduce mistargeting.

For example, in 2009 the World Bank became closely involved in assisting the Pakistani government to re-design its targeting mechanism for the Benazir Income Support Program, a major unconditional cash transfer program for poor households, with an annual budget of 34 billion PKR ($340 million USD). The government originally called on politicians to nominate recipients. Within a year, apparently under pressure from the World Bank, the government discarded the existing targeting system and instead adopted a proxy means test poverty scorecard.

However, there is limited research on whether these effects differ when donors and the government make different choices of programs or design the programs differently. Conventional wisdom in Pakistan - and many other contexts with less than perfect institutions - suggests that many government benefits are given disproportionately to those who are well-connected, regardless of the official rules.

Recent research explores how targeting mechanisms affect local elite capture or corruption by the implementers. But to our knowledge, ours is the first paper to test whether program design can mitigate political influences on targeting.

In this chapter, we study political influences on targeted government assistance programs in rural Punjab, Pakistan. We analyze an original dataset we collected in politicians' home villages - their villages of birth or historic family landholdings - in rural Punjab, Pakistan. We test whether politicians direct more government assistance to their home villages. We then assess whether different types of programs are more or less subject to this type of interference.

We use a combination of a Regression Discontinuity and Difference-in-differences approach. This approach compares households connected to the winner and the loser, before and after the election. This approach uses very comparable "treatment" and "control groups", which standard DD does not. Households connected to just-winners and just-losers should be similar. It also controls for differences in levels of assistance between the two groups, which cross-section RD does not. Hence our approach provides stronger identification than either the RD or DD strategy used separately. The identifying assumption is that these two groups have similar time trends.

We find that politicians in competitive constituencies direct significantly more government resources to households in their own home villages. Households in these villages receive more than twice as much

government assistance as the rivals' home villages.

Households also benefit even when the politician from their home village wins in another constituency, which does not include the home village. This supports the theory that the home village effect is due to politician preferences for assisting their own village, rather than as part of an electoral strategy. In the theoretical framework in Chapter 1, the home villages can be thought of as the politician's "inner circle".

We then exploit a major reform to the targeting mechanism of a cash transfer program, the Benazir Income Support Program, as a natural experiment. This program was originally officially targeted by politicians, but the government agreed to switch to a proxy means test targeting mechanism instead. After the reform, households in winners' home villages are still significantly more likely to receive the transfer. However, many more households receive it in the losers' home villages as well; the proportion of recipients who are in the politician's home village was dramatically reduced.

Home village households benefit even in programs in which there is no official scope for the politician to influence targeting - both the post-reform BISP and a girls' Conditional Cash Transfer program. In addition, home villages benefit even when the politician represents a different constituency. This shows that politicians can exert influence on targeting in an unofficial capacity.

We explore the distribution of assistance within the village. We find that it is broad-based, not limited to the politician's own clan. The effect also appears to be concentrated on poorer households. This may be because it is easier for politicians to direct assistance to their preferred households among those who are actually eligible. It could also be because poor households value the transfer programs more.

We explore possible mechanisms for the home village effect. We find no evidence that knowledge of programs or personal interaction with the politician drives the results. This suggests that program administrators make sure that households from these villages receive assistance.

Our empirical strategy identifies a Local Average Treatment Effect: the effect of a connection for households in competitive political environments, where elections are close and multiple clans are serious contenders. Because we compare the home villages of winners and runners up, our estimate is also specific to households in politically well-connected locations. We discuss how the effect might differ in a more general setting. The theoretical model in Chapter 1 and the empirical evidence from a representative sample in Chapters 2 and 3 provides insight which suggests that the effect we estimate is not unique to this setting.

We take three methodological measures to address potential concerns about data mining. First, a

pre-analysis plan was finalized and registered before data analysis. Second, we use a summary index and False Discovery Rate p-value corrections to account for testing of multiple public service outcomes. Finally, data were split into two parts, one of which is reserved and will not be analyzed until the paper is finalized for publication, to ensure that results are robust to testing the same specifications on out-of-sample data. For this data split methodology, we adapt the method used by Fafchamps and Labonne (2013). We introduce stratification into this methodology, which makes the data split appropriate for a much smaller sample than they use, while minimizing loss of statistical power.

The chapter proceeds as follows. Section 2 discusses some of the literature that is most relevant to this chapter. Section 3 outlines our empirical strategy. Section 4 discusses the data. Section 5 presents results, including a discussion of motivations, mechanisms and external validity; Section 6 shows a range of robustness tests, and 7 concludes.

2 Related literature

Chapters 2 and 3 review the literature on the value of a caste or ethnic connection to a politician. Here, we focus on two aspects of the literature which are important for this paper: home village favoritism, and program design.

Just as with other dimensions of social distance considered in the earlier chapters, there are two possible reasons that an official might direct assistance to his home village. He may have an inherent preference for assisting his home village. Alternatively, it may play a part in an electoral strategy. These motivations, and how they interact, are explored in the theoretical model in Chapter 1. However, if targeted assistance is a part of an electoral strategy, the area targeted needs to be large enough to play a significant part in the outcome of the election. Unless the home village is proxying for a larger geographic area, a single village can only be significant in the context of elections to a small local body.

Besley, Pande, and Rao (2011) present empirical evidence from village councils (Gram Panchayats) in India. They find that the village of the council head receives significantly more public goods provision. They frame this as a part of an electoral strategy. They show that the population share of the village, and therefore its potential to play a role in a winning coalition of villages, is positively correlated with election of the head of the council, as predicted by their model. Although they only have cross-section data, they do use data from an earlier data source to show that these areas did not receive higher levels of assistance before the village councils were formed.

Cheema and Mohmand (2006) study home village bias during the decentralized local government

system in Faisalabad district in Pakistani Punjab.[1] They study construction of sewer drains in a small sample of villages. They use a difference-in-differences specification to test whether the home village of an elected local official (the union council nazim) receives more spending. They find that the official's home village does receive significantly more construction. Unlike the other studies in this literature, they use household survey data to study the distribution of the construction within the village. They find that households in vote blocs allied with a local patron are more likely to receive drain construction in front of their houses. However, individual voters are not targeted, suggesting bloc voting and patronage system of the type we explore in Chapters 1 and 2. This suggests the motivations are electoral, which is plausible in the local government system - the nazim's constituency would include an average of six villages, so one village could play an important role. This chapter therefore analyzes quite a different phenomenon, since the home village is only a tiny proportion of a national politician's constituency. While their small, geographically concentrated sample allowed them to do valuable qualitative analysis of the electoral dynamics, the small number of clusters (eight villages) is a potentially important limitation to the quantitative results (see Cameron, Gelbach, and Miller (2008)) as well as the study's external validity.

Carozzi and Repetto (2014) use a village fixed effects approach to study home town favoritism in Italy. Since the "home town" in this case is often a large city, it is plausible that favoritism could form part of an electoral strategy. Like us, they also examine the difference between politicians representing their home town in a constituency and those representing another constituency. Surprisingly, they find that only the politicians who represent a *different* constituency seem to direct assistance towards their home towns. They seem to do so at the direct expense of their own constituency. The data suggest that these "external" politicians tend to be more senior and important on the national stage. The authors argue that these politicians may therefore not need to spend for re-election purposes in their electoral district. They argue that national politicians in Italy may assist their home villages as a part of a long-run strategy to run for local mayor after retiring from parliament. These two arguments (the high-profile "external" politicians do not need to spend in their electoral district, but do need to establish a reputation in order to win a mayoral race in their home town later on) seem to be somewhat at odds with each other. Arguably inherent preferences would explain the results better. Senior politicians may be more able to exercise these preferences due to their greater power. However, the possibility of a post-retirement electoral incentive, and the lack of data on politician's careers after retirement, makes it difficult to distinguish between this incentive and inherent preferences. This

[1] This system was initiated during the early 2000's under President Pervez Musharraf, and was effectively disempowered after his government ended.

post-retirement incentive is of course context-specific, and would not occur in our case. In Pakistan, there has been no consistently functioning local government system, and national politicians typically remain on the national scene - they do not retire and seek lower level elected office.

Do, Nguyen, and Tran (2013) study ancestral home village favoritism among politicians and bureaucrats in Vietnam. They argue that any favoritism in this setting must be based on inherent preferences, because it is the central party that determines an official's rise to power, and home villages are not important in this power structure. They study official - village dyads, and regress village-level infrastructure on the seniority of the official from that village at a given point in time, with dyad fixed effects. They find evidence that bureaucrats direct infrastructure spending to their home villages, but politicians do not. This difference is likely due to the context, in which the legislature has little de facto power compared to the bureaucracy. But it is noteworthy that bureaucrats exert influence despite having no formal budget authority. They investigate mechanisms by studying several interaction effects. Where provincial rules allow for more budget discretion, the home village effect is stronger. In addition, it is stronger when the local commune head and the bureaucrat are of a similar age, suggesting very personalized connections.

Mu and Zhang (2011) study the behavior of appointed and elected village officials in rural China. These officials are elected at a local level, but their results are more consistent with inherent preferences than with electoral motivation. Using a village fixed effects specification, they find that the home villages of both elected and appointed officials receive more public goods spending. They also find that the elected officials who spend more on their home villages are significantly *less* likely to be re-elected in the next election. This all suggests preference-based favoritism.

This chapter builds on this literature in several ways. First, our RD-DD identification strategy requires weaker assumptions than the fixed-effects approaches taken in the papers discussed above. This addresses the concern that the political or economic rise of certain geographic areas might cause both election of politicians from those areas, and the increase in assistance to them.

Second, the existing literature focuses on block-level transfers, infrastructure and local public goods. To our knowledge, ours is the first study of home village favoritism in household-level transfers. Aside from Cheema and Mohmand (2006), we believe it is the first to examine any kind of distributive effects within the home village. This distinction is important because it is possible that politician influence on individual benefits follows different patterns. For example, assistance may be targeted much more narrowly at the politician's relatives, or it might instead be used as a quid-pro-quo for individual votes. Politicians might get "warm glow" from building visible infrastructure in their home village, but not

as much from assisting individuals with cash transfers. Our results can therefore help shed light on the motivations for favoritism.

Third, by collecting original household survey data on these targeted transfers, we can examine within-village distribution. This allows us to assess how favoritism affects targeting quality. Again, this may be different with individual transfer programs than with local public goods, even when spending on local public goods does have targeting requirements (such as the program for poor municipalities in Do, Nguyen, and Tran (2013)). Our data allow us to assess favoritism within the village for the politician's relatives or particular clans. We can also test directly for mechanisms such as personal interaction and knowledge of program application procedures.

Finally, we contribute in two ways to understanding how program design may mitigate favoritism in government assistance and spending. First, because we collected a large sample, we can test for differences between programs. Second, we exploit a natural experiment in which one program had a major targeting reform during the period we study. This allows us to test whether the reform reduced favoritism.

This last feature allows us to contribute to the literature on which types of assistance programs are more subject to politician influence and favoritism. As Stokes, Dunning, Nazareno, and Brusco (2013) point out, many authors in political science seem to assume that clientelism is synonymous with individual transfer payments. This is unsatisfactory, both because local public goods may also be used for clientelistic purposes, and because individually targeted transfers have many legitimate purposes. To inform better policy, it is helpful to understand what may make transfer programs are vulnerable to interference.

A larger body of literature looks at how program design, in particular targeting procedures, affect corruption or elite capture: see for example Niehaus, Atanassova, Bertrand, and Mullainathan (2013), Alatas, Banerjee, Hanna, Olken, and Tobias (2012). This literature focuses on how the behavior of low-level administrators or local community elites may affect targeting outcomes. It does not address how vulnerable these programs are to the influence of high-level politicians, who might be able to bypass such checks.

Bardhan, Mitra, and Mookherjee (2011), in their study of clientelism in West Bengal, India, find that receiving government assistance is correlated with voting for the incumbent. But they only observe this pattern with recurring short-term benefits such as credit, agricultural input kits, and relief programs. They do not observe it with major one-time benefits, such as land titling, or local infrastructure improvements. This could suggest that targeted short-term programs are more vulnerable

to use for clientelistic purposes. But there are a number of limitations to this interpretation. First, these estimates are not causal, and they do not isolate particular features of government programs for comparison. Second, as the authors acknowledge, they cannot rule out that the results reflect support among the poor for a broad-based policy, rather than clientelism.

We contribute to this literature by providing a clean comparison of a program before and after a targeting reform. We estimate the effect of the home village connection before and after this reform. We are thus able to contribute a causal estimate of the impact of program design on politician influence on program targeting.

3 Empirical strategy

Our basic relationship of interest is:

$$g_{it} = \beta_0 + \delta V_{it} + \epsilon_{it} \tag{1}$$

g_{it} is a measure of government assistance that household i receives at time t. L_{it} is a dummy for household i having a politician from its home village who is in office at time t. δ is the coefficient of interest: the causal effect of a connection on government assistance.

However, it is possible that households who need or want more government assistance strategically form links with politicians when they come into office. This will lead to simultaneity bias in Equation 1. In this paper we avoid this problem by studying a time-invariant link: the politician's birth or ancestral home village.

There are also many possible reasons that households who are connected to politicians may differ on unobservables from other households. Since politicians are likely to come from more socially and economically privileged groups, households connected to them are likely to be privileged overall, beyond what we observe in the data. This could bias δ upwards, because advantaged households are more likely to know how to "work the system" and access government assistance effectively. Alternatively, δ could be biased downwards, because these households are less likely to need certain programs, or would be socially stigmatized if they receive these programs and so choose not to apply for them (as in Gille (2013)).

To identify δ, we use a combination of a Regression Discontinuity and Difference-in-Differences approach. This approach compares households connected to the winner and the loser, before and after the election. Standard DD requires the assumption that, without the treatment, the treatment and

Table 1: Subsamples for RD estimation

	Close-election winner	Close-election runner-up
Home village	A	B
	2002-2007	2002-2007
	2008-2013	2008-2013
Neighboring village	C	D
	2002-2007	2002-2007
	2008-2013	2008-2013

control groups have the same time trend in g. Our approach uses very comparable "treatment" and "control groups", which standard DD does not. Households connected to just-winners and just-losers should be similar. Standard RD requires the assumption that, without the treatment, the treatment (close winners) and control (close losers) groups would receive the same level of g. Our approach controls for differences in levels of assistance between the two groups, which cross-section RD does not. Hence our approach provides stronger identification than either the RD or DD strategy used separately. Our identifying assumption is that, absent the influence of the elected politician, the home villages of just-winners and just-losers would have similar time trends in g.

We sampled the home villages of winner and runner-up politicians in competitive constituencies in the past two elections. Hence the selection of the RD sample, which would be achieved by dropping observations outside a set bandwidth in other settings, is built into our dataset. Table 1 shows the breakdown of our working subsample for the estimations in this paper.

To estimate the home village effect, we restrict the sample to only the home villages of winners and runners up. We include all households who were in category A or category B in Table 1, in either period. We then estimate the following equation on this subsample:

$$g_{it} = \beta_0 + \beta_1 Round_2 + \beta_2 V_i + \delta V_{it}^* + \gamma X_{it} + \epsilon_{it} \tag{2}$$

Where:

- g is a measure of government assistance. The variables for government services and assistance, g, include social safety net programs, education programs, and identity cards. They are discussed in greater detail in Section 4.
- $Round_2$ is a dummy for the second period in the panel;
- V_i is a dummy for a household in a village which has ever fielded winning politician, i.e. households who have ever been in group A (the "treatment group" in DD terminology). Note that

this allows us to control for level differences in the treatment and control group, which is not possible in the cross section RD approach.

- V_{it}^* is a dummy for a household from the home village of a winning politician during the period that politician is in office, i.e. "treated at time t";
- δ is our coefficient of interest: the effect of a politician from i's village holding office on g_{it};
- X_{it} is a vector of controls, including:
  - Vote margin (the RD running variable), defined as the percentage difference in vote count between the winner and runner-up of the election;
  - Political party dummies interacted with round dummies;
  - Household characteristics: a dummy for female-headed household, a dummy for no household assets, a dummy for dwelling quality (mud house), a dummy for no formal education, and the number of girls within an age range targeted for a scholarship program.
  - Each of the household characteristics above, interacted with a round 2 dummy. This controls for eligibility for programs, even if those programs were scaled up or scaled down over the period of the panel.
  - Dummies for the household's clan connection to the politician, to ensure that village clan composition effects do not drive the results, including a dummy for household i having the same clan as the winner in either year, and a dummy for household i sharing the clan of the winner at time t;
  - Dummies for other types of political connections, including sharing the clan of the winning or runner-up politician, or being in the home village of a politician who won in an election with a greater than ten percent margin.[2]

Given our quasi-experimental identification strategy, we do not need to rely on these control variables for identification. However, including them will improve precision. They will also help to address any imbalances between "treatment" and "control" groups, especially since some of these variables are determined at the constituency level, which creates greater potential for lack of balance.

To estimate the effect on neighboring villages, we estimate the analogous specification with the subsample of groups C and D in Table 1:

[2]For example, if a constituency was competitive in the 2008 election but not in the 2002 election, connections to the 2002 winner are not included in our terms of interest but instead are captured with these variables.

$$g_{it} = \beta_0 + \beta_1 Round_2 + \beta_2 NV_i + \delta NV_{it}^* + \gamma X_{it} + \epsilon_{it} \quad (3)$$

Where NV is a dummy for a neighboring village in the same "patwari circle" (local administrative unit) to the politician's home village.

To estimate interaction effects, we fully interact Equation 2 with a vector of characteristics W_i, such as household clan dummis or eligibility proxies:

$$g_{it} = \beta_0 + \beta_1 Round_2 + \beta_2 W_i + \beta_3 V_i + \beta_4 V_i W_i + \delta_1 V_{it}^* + \delta_2 V_{it}^* W_i + \gamma X_{it} + \epsilon_{it} \quad (4)$$

3.1 Control for multiple inference

Specification search and testing of multiple outcomes are of growing concern in the literature, and have the potential to cause important misinterpretation of results (see e.g. Casey, Glennerster, and Miguel (2012)). We take four measures to prevent these problems:

- We registered a comprehensive pre-analysis plan on the Experiments in Governance and Politics database. The plan is reproduced in Appendix C. Modifications from the plan are reported in Appendix D.

- A third party researcher ran our code to select a random sample of half of our 13,000 observations. These data are archived until our specifications are finalized. At that point, we will test whether our results are replicated in the archived subsample. This is a check against specification search, following Fafchamps and Labonne (2013). We build on their method to make it more applicable to a much smaller sample while minimizing loss of statistical power, by stratifying on the independent variables of interest (village and clan). This is a methodological contribution which can allow the use of the data split technique in a much broader range of applications.

- We aggregate the assistance programs analyzed into a single variable of the approximate total monetary value of assistance. Our main specifications test the impact on that single variable.

- For analysis of patterns across programs, we report False Discovery Rate-adjusted p-value corrections (Benjamini and Hochberg, 1995). This is a correction for multiple outcomes testing.

The background and detailed methodology of each of these measures is described in greater detail in Appendices A - C.

4 Data

Our data set is a unique household survey we collected with the support of grants from the British Academy International Partnerships program and the Lahore School of Economics, and in collaboration with Azam Chaudhry and other researchers at the Lahore School. Field work was carried out in February 2013, and covered a total of 13,000 households in 18 National Assembly constituencies across Punjab. The political and institutional context of rural Punjab is discussed in detail in Chapters 2 and 3.

To select the sample, we first selected National Assembly constituencies with competitive elections (less than a 10% margin between the winner and the runner up) in either 2008 or both 2002 and 2008, in which the clan of the incumbent also changed at the 2008 election. Once these constituencies were selected, we identified the home villages of winner and runner-up politicians in the 2002 and 2008 elections. We define the "home village" as a rural village in which the politician was born or where his family are long-time landlords (since many politicians in Pakistan are from landlord families with a historical base in the area where they have large landholdings). We used public secondary data sources to identify the home villages, and the survey team also confirmed this information with a local key informant in the field.

The mean adult population of the villages in our sample was 2,600 in the 1998 census. Since a constituency comprises about 300,000 registered voters, the village population comprises less than 1% of the whole constituency.

For a subset of politicians, we also selected neighboring villages. These are in the same *patwari* circle, a local administrative unit which comprises between 2-10 villages.

Table 2 shows the distribution of the sample within these categories. In some cases, we could not identify a politician's home village. This happened more frequently for runners-up because of the dearth of public information about them. This could bias our estimates. If higher-profile losing politicians are still able to bring resources to their constituency, our estimate of δ will be biased *downwards* towards zero. However, we do a robustness check on the subset of constituencies in which we have matched pairs of home villages of both winners and runners-up. As shown in Section 6, the results are similar to our main results.

The sampling is discussed in greater detail in Appendix E.[3]

[3]Some note on sample sizes: First, in this chapter, we analyze data from the subsample of villages which have ever fielded a winner or runner-up candidate, as described in Section 3. Therefore we omit the data we collected from home villages of politicians who were winners or runners-up in a landslide election in 2002, but who were not winners or runners-up in 2008. Second, we also collected data from some constituencies which were selected according to an alternative definition of competitive elections. These are not used in our main estimates, but are presented as a robustness check

Table 2: Subsamples for RD estimation

	Close-election winner	Close-election runner-up
Home village	A	B
2002-2007	1,027	639
2008-2013	1,700	835
Neighboring village	C	D
2002-2007	158	268
2008-2013	805	310

Cells show the number of households which fall into each category in each period. Totals for 2002-2007 are lower because the sample was selected for constituencies which had a close election in 2008; some constituencies had a close election in both periods, but others did not, so those home villages are not included in the treatment or control group. Instead, a control variable is included to control for the effect of their political connections in 2002. Other politicians' home villages were also sampled but are not included in this table or the estimations in the paper.

Households were asked about their receipt of a range of government assistance programs and services over the past eleven years. Details of the programs are listed in Table 3. The programs are also all described in greater detail in Chapters 2 and 3.

We asked about when programs were received during the entire 11-year period, in order to construct a retrospective panel using recall data on our dependent variable, g_{it}. Because this is retrospective data, recall bias is a concern. In order to improve the accuracy of respondent recall, enumerators probed using follow-up questions on whether respondents had received each program before, during or after a list of key national events over that time period, including natural disasters and major political or news events. This was found in pilots to be successful and respondents did not appear to have difficulty responding in this framework; for this reason, we chose to collect the discrete, memorable variable of whether a program was received and when it started and stopped, as opposed to asking for the amount received directly.

in Section 6. The results are very similar to our main results. Finally, we are working with half the data from each village, because of our "data split" approach, discussed in Section 3.1 and Appendix B. Thus the final sample sizes in our main estimations are approximately 3,000 households or 6,000 panel observations. When the paper is finalized and we estimate our specifications on the full sample, this will be 6,000 households or 12,000 panel observations.

Table 3: **Public programs**

Program / Service	Sample	Description	Eligibility variables	Delivery level	Discretionary	Cash or in-kind
Social safety net transfers						
BISP pre 2009	Full	Unconditional cash transfer for needy HHs	Female head; rudimentary housing; no land	HH	Yes	Cash
BISP post 2009	Full	Unconditional cash transfer for needy HHs	Female head; rudimentary housing; no land	HH	No	Cash
Zakat	Full	Unconditional cash transfer for needy HHs	Female head; rudimentary housing; no land	HH	Yes	Cash
Sasta rashan / Ramzan rashan	Full	Free or reduced-price food supplies	Female head; rudimentary housing; no land	HH	Yes	In-kind
Education - transfers						
Girls' Stipend Program	Full	Conditional cash transfer to HHs with girls in school	School aged girls	HH	No	Cash
Free textbooks	Full	Free textbooks to children in government schools	Children in government school (full); children of school age (subsample)	Sub-village	No	In-kind
Identity cards (newly issued in year t)						

Table 3: **Public programs**

Program / Service	Sample	Description	Eligibility variables	Delivery level	Discretionary	Cash or in-kind
ID card	Full		None	HH	N/A	N/A

All surveyed households were asked about their clan (zaat/quom) and caste/subcaste (biradari); this was then matched with the clan and caste/subcaste of the politicians reported in a questionnaire administered to two local officials in each village. Given the overlap between definition of caste and clan in Pakistani Punjab, if either the clan or caste/subcaste variable matched, we consider the household and politician to be of the same clan. For the purpose of this paper, we refer to "shared clan" using this definition.

All households were also asked some basic household characteristics including education, major assets, and the number of girls who grew up in the household over a certain time frame, to capture proxies for eligiblity for government programs.

4.1 Variable construction

We have observations of a binary for whether a household received each program g for each of the last eleven years: 2002 to early 2013. The politicians we study stood for election in the 2002 and 2008 elections. The two aggregate time periods we consider are T_1, 2002 to 2007, inclusive, and T_2, 2008 to 2013, inclusive.

Certain programs may provide a recurring benefit. For example, once a student has been signed up for a CCT program, she may continue to receive it by default until she finishes school. If a politician arranges for a household to be inducted into a program, and the household continues to benefit into the next politician's term, this would introduce an important source of measurement error into our estimates. To address this, we construct the dependent variables differently for programs that appear in the data to continue for a long period by default. If in 50% or more of our observations, where a household received a program at least once in 2003 or 2004, they continued to receive it for three or more years subsequently, we consider that a long-running program. The programs classified as long-running are the free textbooks and the girls' stipend program.[4]

Continuous: Program value We estimate Equation 2 using a single estimate of total value received as a dependent variable. To minimize recall bias, we collected data on the years in which a household received a program, not on the exact amount received. To construct an approximate amount, we use estimates of the value of each program from administrative sources and government survey data. In the case of programs with benefits that are hard to quantify, such as receiving a new ID card, we use the approximate marginal cost of producing an ID card. We adjust the values of cash programs for

[4]BISP would be a long-running program. However, since it only started in the second term, this problem does not arise.

inflation such that all values are expressed in 2013 Pakistani Rupees (PKR); 100 PKR = $1 US.

This is analogous to a summary index using weights based on values. However, the weights used here (approximate values) have an economic meaning. As Bardhan and Mookherjee (2012) point out, a simple sum of programs may mask patterns of high-value programs, which may be directed towards the elite, and low-value programs, which may be directed towards a larger group of voters through clientelism.[5]

We also test these specifications in program-specific regressions. Here we use the approximate total value received in an individual program over T as a dependent variable. P-value correction for multiple inference is used in the individual program regressions.

For long-running programs, we define g_{iT} as the approximate amount received, if the household started to receive the program started during aggregate period T.[6]

Discrete: programs received In addition, we estimate Equation 2 on a dummy variable for each program.

We construct a binary for whether household i received the program in *any year* t in aggregate period T. For long running programs, we define g as "new receipt," i.e. binary for household i *started* receiving the program at any year t in aggregate period T (that is, it received the program at any time t in T, and not having received it at $t-1$).

We use multiple inference p-value correction using the False Discovery Rate control method described in Section 3.1.

Number of programs Separately, we test the specifications on a simple sum: the number of *distinct* programs the household received. It is possible that recipients of patronage receive many small tokens of government support, but that they do not receive much more in value terms (cf Bardhan and Mookherjee (2012)). The test on an (unweighted) sum of the number of government programs allow us to check for this. For this purpose, we adapt 2 using a negative binomial specification.

Table 4 shows descriptive statistics on this two-round retrospective panel. Because of policy changes in which some programs were rolled out and some were scaled back, the total amount of government assistance increased dramatically in value in the second round. This makes controlling for the overall time trend important. In 6 we show that our results are also robust to region-specific time trends.

[5] This argument relates to the model of patterns of assistance we formalize in Chapter 1.
[6] If a household received a program intermittently, restarts are also included here.

Table 4: Summary statistics for two round panel

	count	mean	sd	min	max
			Round 1		
Total value of government services received (PKR)	3500	349	677	0	15720
Number of different programs received	3472	0.678	0.62	0	3
Free textbooks	3500	0.147	0.35	0	1
Zakat cash transfer	3500	0.001	0.02	0	1
In-kind food	3500	0.001	0.03	0	1
BISP cash transfer	3500	0.000	0.00	0	0
Girls' stipend CCT	3500	0.009	0.09	0	1
New ID card	3472	0.521	0.50	0	1
Number of girls eligible for stipend x Round 1	3499	0.617	0.96	0	10
Household head never attended school x Round 1	3500	0.477	0.50	0	1
No household assets x Round 1	3500	0.152	0.36	0	1
Female head x Round 1	3500	0.029	0.17	0	1
Rudimentary housing x Round 1	3500	0.337	0.47	0	1
Home village politician in office	3500	0.293	0.46	0	1
Home village politician ever won	3500	0.779	0.41	0	1
Same-clan politician in office	3500	0.036	0.19	0	1
Same-clan politician ever won	3500	0.128	0.33	0	1
Home village politician in office - non competitive election	3500	0.200	0.40	0	1
Same clan politician in office - non competitive election	3500	0.040	0.20	0	1
			Round 2		
Total value of government services received (PKR)	3500	2277	7703	0	93491
Number of different programs received	3472	1.005	0.75	0	4
Free textbooks	3500	0.224	0.42	0	1
Zakat cash transfer	3500	0.003	0.05	0	1
In-kind food	3500	0.007	0.08	0	1
BISP cash transfer	3500	0.067	0.25	0	1
Girls' stipend CCT	3500	0.040	0.20	0	1
New ID card	3472	0.664	0.47	0	1
Number of girls eligible for stipend x Round 2	3499	0.617	0.96	0	10
Household head never attended school x Round 2	3500	0.477	0.50	0	1
No household assets x Round 2	3500	0.148	0.35	0	1
Female head x Round 2	3500	0.029	0.17	0	1
Rudimentary housing x Round 2	3500	0.263	0.44	0	1
Home village politician in office	3500	0.486	0.50	0	1
Home village politician ever won	3500	0.779	0.41	0	1
Same-clan politician in office	3500	0.111	0.31	0	1
Same-clan politician ever won	3500	0.128	0.33	0	1
Home village politician in office - non competitive election	3500	0.197	0.40	0	1
Same clan politician in office - non competitive election	3500	0.021	0.14	0	1

Round 1 and Round 2 panels show summary statistics for the same (panel) households; Round 1 is 2002-2007, and Round 2 is 2008 - 2013. Total value of government services received is approximate total value of government assistance programs received in PKR ($1 US = 100 PKR), calculated based on years received as described in section 4.

Table 5: **Impact of a home village politician on government assistance**

	(1) OLS Total value	(2) Negative binomial Number of programs	(3) OLS Conditional value
Home village politician in office (Treated)	907.58	0.09	1222.84
	(245.60)***	(0.06)	(357.56)***
	[264.73]***	[0.05]*	[404.82]***
Home village politician ever won (Treatment group)	-122.17	0.03	-416.70
	(251.87)	(0.07)	(361.42)
	[230.18]	[0.09]	[361.42]
Observations	6910	6854	4482
Control variables	YES	YES	YES
"Control group" mean	686.84	0.77	1110.92
Observations	6910	6854	4482

Dependent variable in column (1) is approximate total value of government assistance programs received in PKR ($1 US = 100 PKR), calculated based on years received as described in section 4. Dependent variable in column (2) is number of distinct government assistance programs received. Column (2) shows marginal effects after negative binomial regression. Dependent variable in column (3) is total value, conditional on any program received. Control variables are as specified in Equation 2. "Control group" is defined as any household in either Group A or B in Table 1, when that household's home village politician is *not* in office. Robust standard errors, clustered at village level (parentheses) and constituency level [brackets]. *** p<0.01, ** p<0.05, * p<0.1.

5 Results

Table 5 shows the results from estimating equation 2 on the summary index of government assistance. They show that politicians direct significantly more government assistance to their own home villages; the magnitude of the difference is approximately 900 rupees per household over the 5 year period (US $9). This corresponds to approximately a *130 percent increase* over the "control" group: the home villages of the winners receive more than double what the home villages of the losers do.

Note that this specification nests a test for the identifying assumption of the competitive-elections RD identification strategy. By exploiting the panel nature of the data, we can test directly whether ever-winners are systematically different. In other words, this tests whether g is higher for a household which was ever in Group A in Table 1, as opposed to a household which was always in Group B (runners-up). A significant coefficient would indicate a violation of the RD assumption. This estimate is negative and insignificant, which supports the assumptions of cross-section RD.

5.1 Scope and motivations for politician influence

We carry out two tests to define the scope of politician influence in targeting, and thus understand the motivations.

It is possible that the home village could represent part of a larger constituency which is not geographically contiguous. For example, a politician might represent the interests of the poor, of more urbanized areas, or of a particular clan. These groups could be better represented in his home area. However, all our specifications control for poverty proxies interacted with a round dummy. This controls for differences in characteristics as well as in changes in national or provincial policies that target different groups. In addition, in Table 6 we compare observable characteristics of the households in villages of winners (treatment) and runners-up (control).[7] The results show that the two groups of villages are generally comparable; if anything, the winners' villages actually have *fewer* households who appear to be eligible for need-based cash transfers. They are comparable in terms of their political party representation; the main results already include controls for political party interacted with round. Home village is also not proxying for clan (shown later in Table 10).

Home village could be proxying for a larger geographic area. To address this, we first test whether the home-village politician effect extends beyond the politician's immediate home village. Column (2) in Table 8 shows the results of Equation 3. This specification compares a treatment group of villages in the same local area[8] as the winner's home village to a control group of villages in area of the rival's home village (groups C and D in Table 1). The home villages themselves are excluded. The results show that the politician's neighboring villages also benefit from assistance. Because our sample is selected from home and neighboring villages, we cannot test with this dataset whether the benefits go further than the immediately neighboring villages. In a future extension, we will use secondary data to test this.

We test whether favoritism takes place equally when there is an electoral incentive and when there is not. Table 9 shows the results with the treatment group divided into two groups. The first group correspond to politicians whose home villages fall within their constituencies. The second group are those whose home villages are outside their constituencies. Both sets of home villages receive significant assistance, and we cannot reject that the two coefficients are equal ($p = .55$). Again, this is inconsistent with a clientelism explanation, unless the politician is planning to switch constituencies in a future

[7] For the 2008 winners, we compare the "pre-treatment" observables, i.e. their 2002 characteristics. For 2002 winners, we do not have data on pre-treatment outcomes. However, we compare observable characteristics in the year 2002, and most of these outcomes are unlikely to have been affected by government assistance or connections, especially in such a short time period.

[8] *Patwari* circle.

race.

Taken together, these results support the idea that politicians have a direct preference for supporting their home villages, rather than assisting them as a part of a larger electoral strategy. In the theoretical framework of Chapter 1, home villages fall into the "inner circle". The politician has an inherent preference to assist this group of his closest contacts.

Table 6: Differences in characteristics by treatment group

	Home village politician ever won (Ever treatment group)	Home villages of runners-up who never won (Always control group)
Girls in eligible age range	-0.163**	(0.0709)
HH head no education	-0.0401	(0.0644)
No HH assets	-0.0788	(0.134)
Female head	0.0152	(0.00902)
Rudimentary housing, round 1	-0.295*	(0.157)
Party = PPP	-0.185	(0.250)
Party = PMLQ	0.212	(0.271)

Table 7: Robustness to geographic time trends

	Total value of government assistance		
	(1)	(2)	(3)
Home village politician in office (Treated)	907.58	725.40	928.87
	(245.60)***	(208.38)***	(234.09)***
	[264.73]***	[219.62]***	[234.09]***
Home village politician ever won (Treatment group)	-122.17	10.53	-82.63
	(251.87)	(234.73)	(186.74)
	[230.18]	[242.57]	[186.74]
Control variables	YES	YES	YES
Region x time dummies		YES	
District x time dummies			YES
Observations	6910	6910	6910

Dependent variable is approximate total value of government assistance programs received in PKR ($1 US = 100 PKR), calculated based on years received as described in section 4. Control variables are as specified in Equation 2. "Control group" is defined as any household in either Group A or B in Table 1, when that household's home village politician is *not* in office. Robust standard errors, clustered at village level (parentheses) and constituency level [brackets]. *** p<0.01, ** p<0.05, * p<0.1.

If the politician's home village is his "inner circle," who is close enough to be included? Table 10 shows the results from estimating Equation 4, with clans as the interaction term. The home village effect is decomposed into the effect on the politician's own clan, clans of his rivals (the other winner

Table 8: Neighboring villages

	(1)	(2)	(3)	(4)
	Total value of government services received			
Home village politician in office (Treated)	907.58 (245.60)*** [264.73]***	928.87 (234.09)*** [246.08]***		
Home village politician ever won (Treatment group)	-122.17 (251.87) [230.18]	-82.63 (186.74) [196.23]		
Neighboring village politician in office (Treated)			1097.11 (741.63) [717.85]	1686.80 (737.39)** [732.65]**
Neighboring village politician ever won (Treatment group)			-121.91 (323.00) [331.37]	-753.25 (428.95) [429.29]
Control variables	YES	YES	YES	YES
District dummies x round dummies		YES		YES
"Control group" mean				
Observations	6910	6910	2692	2692

Dependent variable is approximate total value of government assistance programs received in PKR ($1 US = 100 PKR), calculated based on years received as described in section 4. Control variables are as specified in Equation 2. "Control group" in columns 1-2 (3-4) is defined as any household in either Group A or B (C or D) in Table 1, when that household's home village politician is *not* in office. Robust standard errors, clustered at village level (parentheses) and constituency level [brackets]. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Home villages in and out of the politician's constituency

	(1)	(2)
	Total value of government services received	
Home village politician in office (Treated)	907.58 (245.60)*** [264.73]***	
Treated - HV is out of constituency		780.06 (394.70)* [417.66]*
Treated - HV is in constituency		969.66 (314.45)*** [293.86]***
Observations	6910	6910
Proportion of sample - "in constituency treatment group"	28%	
Proportion of sample - "out of constituency treatment group"	16%	

Dependent variable is approximate total value of government assistance programs received in PKR ($1 US = 100 PKR), calculated based on years received as described in section 4. Control variables are as specified in Equation 2. Robust standard errors, clustered at village level (parentheses) and constituency level [brackets]. *** p<0.01, ** p<0.05, * p<0.1.

and the runners-up from the two elections), and all other clans. The coefficients on the home village effect for the politician's own clan and all other clans are large and similar in magnitude to the basic estimate; only the latter is significantly different from zero. The point estimate on the home village effect for rival clan is much smaller, although we cannot reject that the three coefficients are equal. [9]

These interaction effects do not identify the causal effect of clan, but rather the causal effect of a home village politician on different clans. So these results could be driven by poorer clans having more households who are eligible for assistance. We have controlled for several indicators of poverty, but this will not fully control for differences in eligibility.

However, the results clearly indicate that other clans benefit in the politician's home village - not just his own clan. The benefits of a local politician are widespread.

As with any application of RD, our empirical strategy identifies a Local Average Treatment Effect: the effect of a connection for households in competitive political environments, where elections are close and multiple clans are serious contenders.

In the context of the analysis in Chapters 1-3, however, it is clear that if politicians assist an "inner

[9] It is possible that with the estimate on the full sample we will achieve greater precision with these estimates. See Section 3.1 for details on our data split methodology.

Table 10: **Impact of a home village politician on government assistance by clan**

	(1)
	Total value of government services received
Home-village politician in office (treated) x same clan	619.08 (782.97) [798.46]
Home village politician in office (treated) x rival clan	276.00 (532.82) [405.48]
Home village politician in office (treated) x other clan	994.87 (290.32)*** [324.81]***
Observations	6910

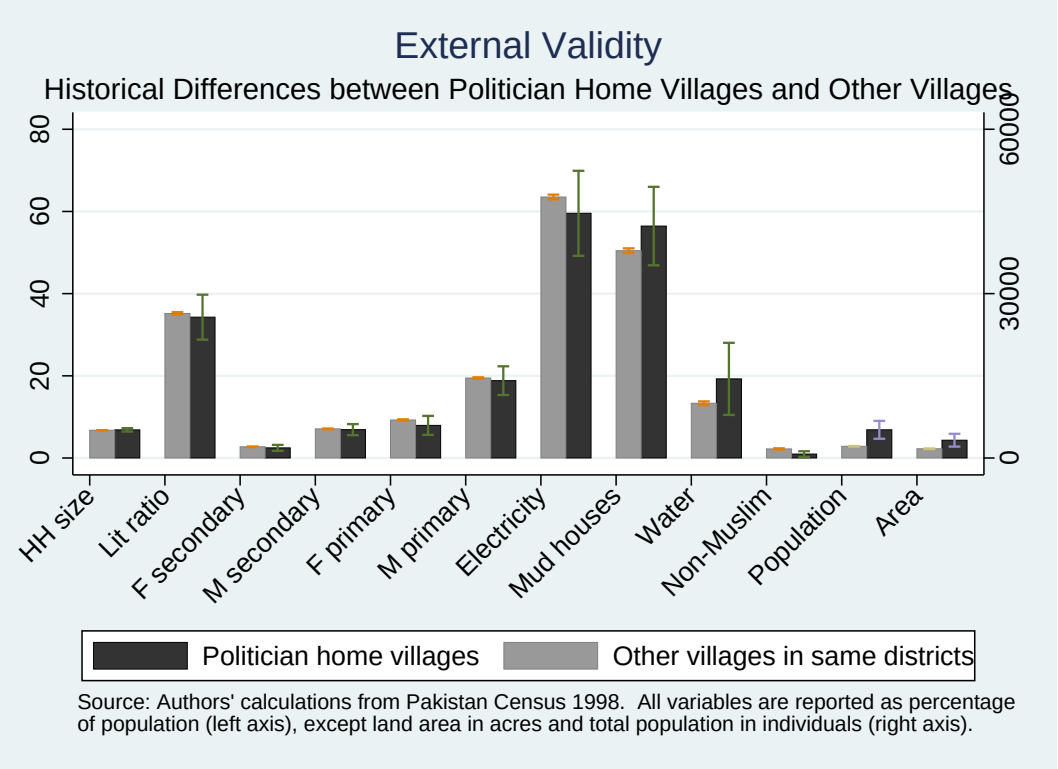
Dependent variable is approximate total value of government assistance programs received in PKR ($1 US = 100 PKR), calculated based on years received as described in section 4. Control variables are as specified in Equation 2. Robust standard errors, clustered at village level (parentheses) and constituency level [brackets]. *** p<0.01, ** p<0.05, * p<0.1.

circle" of voters out of nepotistic preferences, this will not vary with the level of political competition. The evidence supports the idea the home village effect is a highly localized and is based on these nepotistic preferences, and is not consistent with a programmatic or clientelistic electoral strategy. Therefore, the effect we estimate is like to generalize well to other types of constituencies.

Because our control group are the home villages of runner-up politicians, the effect we estimate is also specific to politically well-connected locations. This may suggest that the effect would be greater in other areas, since politicians who lose can still provide information about how to apply for a program and likely still have some useful connections for negotiating the bureaucracy, even when they are not in office. Figure 1 shows characteristics of the politician home villages in our sample (winners and losers of close and landslide races), contrasted with the other villages in their districts, from the 1998 census, well before the period we study. The observations are at the village level, so $N = 61$ for our sample, but $N = 12,000$ for the rest of the district. These villages are not significantly different from others on public services or any of the other variables except population: politicians tend to come from more populous villages.

The only possible reason that our estimated effect might be larger than the effect in the general population is punishment. If politicians specifically want to punish their opponents' villages, the RD would measure the combined effect of reward and punishment (Fafchamps and Labonne, 2013).

Figure 1: External Validity: Politician Villages and Others in their Districts



5.2 Program design

Can these results tell us anything about program design, and whether it can mitigate political capture? To answer this, we take two approaches. First, we compare results between different categories of programs. Second, we exploit a policy experiment: a targeting reform to one of the programs we study.

5.2.1 Program comparisons

Program characteristics are not randomly assigned, and the programs we analyze differ in multiple ways - funding, size, purpose, and so on. Therefore comparisons between programs in this setting cannot be interpreted as causal estimates of the effect of any program design feature. This is a necessary drawback given the setting; however, comparing estimates between programs give extremely useful information about the extent of capture in different programs, and suggestive evidence about whether program design is related to these differences. We also note that there is a major advantage of a non-experimental setting to answer this research question, as compared to an experimental approach in which alternative mechanisms are tested systematically: it has high external validity for understanding

how well programs are implemented by a government in a realistic institutional and political setting (cf. Bold, Kimenyi, and Mwabu (2012)).

Tables 11 shows the results of estimating equation 2 on individual assistance programs. The positive effect of having a politician from one's home village in office is concentrated in a few programs. The coefficient on the village effect for the BISP cash transfer program and the girls' stipend CCT program are both significant. Both of these results are robust to using a probit specification (table available on request). After False Discovery Rate correction of the p-value (see Section 3.1, both of these programs retain significance at the ten percent level in the binary specification, and BISP is still significant at the ten percent level in the value specification.

Table 12 shows the results for different programs in comparable units. Here we convert the value of the program to control group standard deviations, i.e. we subtract the control group mean and divide by the control group standard deviation. We then carry out the estimation as before. This allows us to examine relative political influence between programs. We use Seemingly Unrelated Estimation to carry out cross-equation tests of the magnitude of δ, as specified in the Pre-Analysis Plan (Table 26). As shown in Table 13, we examine differences in δ by three categories: programs with discretionary targeting procedures; programs in cash versus in-kind; and national programs (at the same level of the politician we study) versus provincial programs.

The only cross-equation test that is rejected is the difference between national and provincial programs. This may suggest that politicians have more ability to influence the distribution of programs at their own political level. However, these results are driven by one program, the BISP. This program has other features which differ from the provincial programs (such as the much larger value of the program to each recipient). These features may be other reasons for the greater influence over this program than the others.

Table 11: **Impact of home village politician on individual government assistance programs received: Binary**

	(1) Free textbooks	(2) Zakat cash transfer	(3) Food transfer	(4) BISP cash transfer	(5) Girls' stipend CCT	(6) ID card
Home village politician in office (Treated)	-0.00	0.00	0.00	0.03	0.01	0.06
P-values, clustered at village level	(0.874)	(0.457)	(0.525)	(0.001)***	(0.082)*	(0.161)
Q-values, clustered at village level	{0.874}	{0.63}	{0.63}	{0.006}***	{0.246}	{0.322}
P-values, clustered at constituency level	[0.854]	[0.423]	[0.523]	[0.002]***	[0.065]*	[0.166]
Q-values, clustered at constituency level	⟨ 0.854 ⟩	⟨ 0.628 ⟩	⟨ 0.628 ⟩	⟨ 0.012 ⟩**	⟨ 0.195 ⟩	⟨ 0.332 ⟩
Home village politician ever won (Treatment group)	0.02	0.00	0.01	-0.01	-0.01	0.03
	(0.524)	(0.773)	(0.011)**	(0.157)	(0.498)	(0.570)
	[0.555]	[0.823]	[0.012]**	[0.130]	[0.366]	[0.635]
Control variables	YES	YES	YES	YES	YES	YES
"Control group" mean						
Observations	6910	6910	6910	6910	6910	6854

Dependent variable is an indicator for receiving the government program at any time during period t. Control variables are as specified in Equation 2. Robust p-values in parentheses, clustered at the village level (in parentheses) or constituency level [in brackets]. Q-values are the Benjamini and Hochberg (1995) False Discovery Rate-adjusted analog of p-values, adjusting for all six hypotheses tested; see A. Q-values are clustered at the village level {in braces} or constituency level ⟨ in pointed brackets ⟩. *** p<0.01, ** p<0.05, * p<0.1

Table 12: Program wise results: standard deviations

	(1) Free textbooks	(2) Zakat cash transfer	(3) Food transfer	(4) BISP cash transfer	(5) Girls' stipend CCT	(6) ID card
Home village politician in office (Treated)	-0.01	0.00	-0.00	0.22	0.02	0.11
P-values, clustered at village level	(0.880)	(0.789)	(0.935)	(0.001)***	(0.603)	(0.236)
Q-values, clustered at village level	{0.935}	{0.935}	{0.935}	{0.006}	{0.935}	{0.708}
P-values, clustered at constituency level	[0.862]	[0.780]	[0.936]	[0.003]***	[0.588]	[0.238]
Q-values, clustered at constituency level	⟨ 0.936 ⟩	⟨0.936⟩	⟨0.936⟩	⟨0.018⟩	⟨0.936⟩	⟨0.714⟩
Home village politician ever won (Treatment group)	0.03	-0.01	0.15	-0.04	-0.01	0.05
	(0.749)	(0.782)	(0.009)***	(0.486)	(0.774)	(0.612)
	[0.765]	[0.821]	[0.013]**	[0.486]	[0.662]	[0.673]
Control variables	YES	YES	YES	YES	YES	YES
Observations	6910	6910	6910	6910	6910	6910

Dependent variable is value of government program received, expressed in standard deviations of the "control" group. Control variables are as specified in Equation 2. Robust p-values in parentheses, clustered at the village level (in parentheses) or constituency level [in brackets]. Q-values are the Benjamini and Hochberg (1995) False Discovery Rate-adjusted analog of p-values, adjusting for all six hypotheses tested; see A. Q-values are clustered at the village level {in braces} or constituency level ⟨ in pointed brackets ⟩. *** p<0.01, ** p<0.05, * p<0.1

Table 13: Cross-equation test results

Hypothesis	Group 1	Group 2	P-value
Discretionary programs (1) are more affected than non-discretionary programs (2).	BISP pre reform, Zakat cash transfer, Sasta rashan food transfer	BISP after reform, free textbooks, girls' stipend	0.54
Cash programs (1) are more affected than in-kind programs distributed to households (2).	BISP pre reform, BISP post reform, Girls' stipend	Sasta rashan food transfer, free textbooks	0.13
Politicians are able to influence who receives programs that are administered at their own level (1) more than those administered at a different level (2).	BISP pre reform, BISP post reform, zakat	Girls' stipend, Sasta rashan food transfer, free textbooks	0.05

5.2.2 BISP natural experiment

The government of Pakistan made a major reform to the BISP cash transfer program, after the first year of the program. When it was initiated in 2008, the politicians themselves were directly responsible for selecting recipients in their constituency. However, in 2009, with extensive involvement from the World Bank, the government of Pakistan ended that system and replaced with a World Bank-designed proxy means test poverty scorecard system (see Schreiner (2009)). Then president Zardari "said that the poverty score card is a transparent and impartial survey of the poor through an internationally recognized procedure...[and that] he was personally monitoring the progress of the Poverty Scorecard Survey."[10] The same scorecard has since been adopted by many other agencies in the country for assistance targeting.

Other features of the program stayed the same before and after the policy change, so comparing effects on this program before and after the policy change provides a natural experiment on targeting reform.

We split up the BISP program into two components, as though it were two separate programs: cash transers to households who were enrolled and started receiving the program before the policy change in 2008, and those to households who were enrolled in 2009 or later, after the policy change. Because BISP is an ongoing transfer program, households qualify for it once and continue to receive it without any specified end date. Therefore, if a household started receiving the program in 2008, all the transfers they receive under the program are considered part of the pre-change program, and vice versa. We drop pre-reform recipients from the sample for estimating the post-reform years. Since BISP

[10] *Business Recorder*, Pakistan, August 2012.

was only initiated in 2008, i.e. in the second round of our panel, these are cross-section RD results rather than panel estimates. In addition, because dividing the periods up in this way reduces the sample, multicollinearity arises with our standard set of regressors. Therefore we use a parsimonious subset of controls here.[11]

We also use a later cutoff to check that measurement error in respondent recall, or any uneven implementation of the policy change, does not drive the results. This would be likely to bias our estimate of δ upwards for the post-reform period. To address this, we repeat the same test comparing BISP in 2008 and 2011.

Tables 14 shows the results. Households in the home villages of politicians continued to receive significantly more funds through the BISP program, not only immediately after but even two years after the program targeting was completely reformed (significant in the continuous model, insignificant but similar magnitude in the binary model). Again, this result is not driven by households who were "grandfathered in" under the old system, because we define the variables here to include only households who start receiving the program for the first time after the specified date.

However, the size of political influence δ is decreasing, particularly once we use the standardized value of BISP, which accounts for the fact that the overall size of the program was growing (columns 4-8). We test the cross-equation restriction that δ is equal across each of these specifications. The p-value of this test is shown at the bottom of table 14. Examining the program immediately before and after the reform is the cleanest comparison, because the size and scope; this is shown in Columns 4 and 5. The cross-equation test indicates that politician influcence, δ, dropped significantly after the reform (p = .06). This corresponds to a two-thirds reduction in our measure of politician influence over the program. There is a slight increase in δ after 2009, but the change is not significant between 2009 and 2011-2013.

Since the scorecard reform, the official procedure for selecting BISP recipients is through a proxy means test carried out through a household survey in the field. This might suggest that the reduction in δ is purely due to the fact that the survey had already taken place in the home villages, or saturation of the potential recipient pool there. But this is not the case. In fact, the number of new recipients of BISP increased in the home villages after the reform as well, and continued to increase in the later years. We include the numbers of new recipients for each time period in the home village (treatment) and rival village (control) at the bottom of Table 14.

[11] We drop V and the clan and other politician controls in this specification. Robustness checks using alternative subsets of controls yield similar patterns of results.

Table 14: BISP cash transfer: Home village effects before and after reform

	(1)	(2)	(3)	(4)
	Normalized value			
	2008	2009	2009-2013	2011-2013
Home village politician in office (Treated)	0.39	0.15	0.19	0.06
	(0.13)***	(0.04)***	(0.07)***	(0.07)
	[0.13]***	[0.05]***	[0.08]**	[0.07]
Observations	3411	3385	3385	3358
Control variables	YES	YES	YES	YES
P-value for cross-equation test $\delta_{before} = \delta_{after}$		0.03**	.14	.03**

Dependent variable is value of BISP received, as described in section 4, expressed in standard deviations of the "control" group for the same time period. Control variables are as specified in Equation 2, except for the omission of V and other clan control variables to address multicollinearity due to too many regressors in subsamples. Robust standard errors, clustered at the village level (in parentheses) or constituency level [in brakcets].

5.3 Political influence and targeting quality

What are the consequences of political influence? Does it result in gross mistargeting? To explore this, we focus on the BISP cash transfer program. This program makes a good case study both because it shows the strongest evidence of political influence, and because it has defined targeting criteria. We do not have all the proxies used as a part of the proxy means test for BISP targeting, but we have a subset of them: indicator variables for a female headed household, a household with no major assets, and a household living in a rudimentary (*kacha*) dwelling.

Table 15 shows the interaction specification 4 with the eligibility variables. The results show that much of the effect appears to be concentrated among households that are likely to be eligible for the program. Comparing the columns of 15, we can see that after the targeting policy reform and as time went on, the home village effect seems to be concentrated less and less on the eligible households; the basic village treatment term becomes significant, while the female head interaction is actually negative and significant. This could be due to a saturation effect: once eligible households in the home village were already signed up, any further new recipients would be less likely to be eligible.

Table 16 shows the characteristics of BISP recipients in home villages and rival villages. The two groups have similar levels of assets and education. The home village recipients are less likely to be female-headed households, but actually more likely to live in rudimentary houses, so no clear picture of differential targeting quality from these data.

Since there is widespread knowledge of the use of the proxy means test survey to target BISP, it is possible that well-connected respondents misreported household characteristics to our survey team in the belief that this could help them receive the program, or to avoid possible discontinuation of bene-

fits. To prevent this, our enumerators explained to respondents that the survey was not connected with the government and would have no consequences for any benefits for them, but we cannot rule out that respondents may not have always been convinced. This would only affect our main results if respondents under-reported BISP receipts in the hope of receiving more assistance, and more respondents in home villages did so. In that case, our main estimate of δ would be biased downwards, understating the true effect. If respondents under-report their assets more in home villages, it would not affect the main results (which are similar with and without control variables - not shown). However, it would make the eligibility results in Table 15 unreliable. In that case, it is possible that ineligible households receive the program, but appear to be eligible based on their misreported information.

Table 15: Politician influence and eligibility

	(1)	(2)	(3)	(4)	(5)
	Value of BISP cash transfer				
	Any year	2008	2009	2009-2013	2011 - 2013
Home village politician in office (Treated)	0.41	0.14	0.15	0.20	0.08
	(0.16)**	(0.09)	(0.05)***	(0.09)**	(0.07)
	[0.17]**	[0.08]	[0.05]**	[0.09]*	[0.06]
Treated x no major HH assets	0.81	1.21	0.08	0.06	-0.01
	(0.43)*	(0.60)*	(0.10)	(0.15)	(0.12)
	[0.43]*	[0.61]*	[0.13]	[0.17]	[0.12]
Treated x female head	-0.24	0.23	0.18	-0.23	-0.49
	(1.26)	(1.39)	(0.66)	(0.49)	(0.32)
	[1.27]	[1.36]	[0.65]	[0.46]	[0.33]
Treated x rudimentary housing	0.37	0.24	-0.04	-0.04	-0.02
	(0.13)***	(0.17)	(0.12)	(0.17)	(0.16)
	[0.12]***	[0.19]	[0.13]	[0.17]	[0.16]
Observations	3411	3411	3385	3385	3385

Specification shown is the same as in previous tables, but with all village terms (treated and treatment group) interacted with the vector of three eligibility characteristics. Dependent variable is value of BISP received, as described in section 4, expressed in standard deviations of the "control" group for the same time period. Control variables are as specified in Equation 2, except for the omission of V and other clan control variables in columns 2-5 to address multicollinearity due to too many regressors in subsamples. Robust standard errors, clustered at the village level (in parentheses) or constituency level [in brakcets]. *** p<0.01, ** p<0.05, * p<0.1

Table 16: Characteristics of BISP recipients in home villages and rival villages

	Mean - rival villages	Mean - home villages	Difference
HH head has no formal education	0.60	0.61	-0.01
HH has no major assets	0.17	0.21	-0.04
Female headed HH	0.20	0.08	0.12 **
Rudimentary housing	0.23	0.40	-0.16 *
Observations	30	144	

5.4 Mechanisms

How do politicians actually exert influence over targeting? Apart from the BISP cash transfer program pre-reform, they do not play any official role in selecting recipients for any of the benefits we have studied. Yet households in politicians' home villages still received significantly more from the BISP program after the reform, and they received more in the girls' stipend program as well. We consider a number of possible mechanisms.

First, politicians could ensure households are informed about how to sign up for a program or collect its benefits. In BISP post reform, households do not need to sign up for the program, because a team carries out the survey in each village to identify households. It is possible that politicians make sure that the survey team visits their village, and visit as soon as possible to start the process. However, as shown in Table 14, home villages continued to have many more new recipients in the years after the BISP program started, so they may have found ways to add recipients after the initial survey.

However, recipients do need National Identity Cards to collect the benefits. They also need to understand how to collect the money. Initially BISP recipients had to visit post offices to collect funds, and much later (around the end of the period we study), the program was switched to a debit card system. These may be significant barriers for poor households; in particular, encouraging female recipients to apply for ID cards has been a problem for the program in some parts of the country. We find no home village effect on newly issued ID cards (see Table 11). However, politicians might have helped recipients collect their transfers.

Second, politicians could help households resolve issues with the program, such as petty corruption or negligence by local implementers. For example, there were many issues reported with the post office delivery system of BISP transfers. In the case of the girls' stipend program, school officials must submit paperwork certifying the girl's attendance at school. They might not take this step, or might require a bribe from the girl's parents. Politicians could use their power over the bureaucracy to resolve these problems.

These first two possibilities fall in the category of "constituent services" (Stokes, Dunning, Nazareno,

and Brusco, 2013), used for resolving exclusion errors in the whole targeting process. In this light, politician influence may seem benign or even positive. Of course, it is still highly inequitable: problems are only resolved for well-connected households.

Alternatively, politicians may use their influence to create "inclusion errors" in the sense of getting ineligible or marginally eligible recipients to receive programs.

This could happen at the initial targeting stage. With the BISP program, politicians could pressure the survey teams to change the data in the poverty scorecard, to get more households in their villages included in the list. With the girls' stipend program, politicians could pressure school officials to add more names to the list despite the fact that their attendance is insufficient. Politicians could offer the implementers protection against consequences if these inclusions were discovered.

Another possibility is that politicians directly use their influence with the national or provincial bureaucracy to have names added to the central lists used for these programs, after initial targeting takes place.

It is important to note that with many of these mechanisms, the politician may not actively intervene. If implementers at each stage are aware of the politician's home village, they may give these households special treatment without needing to be asked, in the hope of currying favor or avoiding punishment.

We use two variables to shed light on some of these possible mechanisms. To assess a mechanism variable, we include it as a control. If the variable is the mechanism for the effect of x on y, this should reduce the magnitude of the coefficient of interest.

First, a subsample of households, who were interviewed for a longer survey, were asked about whether they knew the procedure to apply for each of several programs. In Table 17, we show our main specification for BISP, with this variable included as a mechanism. The results are unchanged. This suggests that information on how to collect the benefits was not the key mechanism for the effect on BISP.

Second, all households surveyed were asked about their relationship with the politician. In Table 18, we use this relationship as a mechanism variable. Again, the results are unchanged. This suggests three possibilities. Politicians may intervene in ways that provide assistance to the entire village (consistent with the clan results in Table 10). In this case, intervention in the sense of having names added to a list seems less likely. This is also consistent with the idea that implementers offer the home village households assistance without active intervention. Alternatively, politicians may assist individual households through intermediaries, such as a local *numberdar*. In future work we will explore

this with data we collected on local influentials.

This last result also differs from the results in the representative sample analyzed in Chapters 2 and 3. This difference is consistent with the idea that the entire home village acts as the politician's "inner circle", whereas in the larger constituency a personal relationship is an indicator of being in the inner circle.

Table 17: Information as a mechanism

	(1)	(2)	(3)
	Value BISP cash transfer	Knows how to get BISP program	Value BISP cash transfer
Home village politician in office (Treated)	4101.94	0.04	4003.38
	(1263.73)***	(0.10)	(1320.68)***
	(1475.48)**	(0.08)	(1546.45)**
Knows how to get BISP program			2298.80
			(1427.95)
			(1421.36)
Observations	181	181	181

Subsample with long questionnaire used to allow for inclusion of "knows how to get BISP" variable. Dependent variable in columns 1 and 3 is value of BISP received, as described in section 4, expressed in Pakistani Rupees (100 PKR = 1 USD). Dependent variable in column 2 is binary for whether respondent indicates he/she knows how a household can get the BISP program. Control variables are as specified in Equation 2, except for the omission of V and other clan control variables in columns 2-5 to address multicollinearity due to too many regressors in subsamples. Robust standard errors, clustered at the village level (in parentheses) or constituency level [in brackets]. *** p<0.01, ** p<0.05, * p<0.1

6 Robustness checks

6.1 Identification through close elections

A large literature has discussed the validity of the close-elections identification strategy. In situations where candidates or parties are able to make strategic choices to ensure that the vote total is just enough that they win, the assumption that just-winners and just-losers are fully comparable is brought into question. Recent work from the U.S. House of Representatives, such as Sekhon and Caughey (2011), Grimmer, Hersh, Feinstein, and Carpenter (2011) shows that in that context, just-winners are more likely to be bigger campaign spenders and are more likely to control key state administrative bodies.

Note that this does not necessarily imply rigging of the election, nor does rigging necessarily create

Table 18: Personal interaction

	(1)	(2)
	Total value of government services received	
Home village politician in office (Treated)	907.58 (245.60)*** [264.73]***	975.34 (268.12)*** [297.72]***
Home village politician ever won (Treatment group)	-122.17 (251.87) [230.18]	-177.56 (239.07) [233.78]
Knows politician in office		-383.85 (272.49) [291.82]
Knows politician who ever won		-417.28 (462.03) [467.38]
Observations	6910	6910

Dependent variable is value of services received in PKR (100 PKR = 1 USD). Control variables are as specified in Equation 2. Robust standard errors clustered at the village level (in parentheses) or constituency level [in brackets]. *** p<0.01, ** p<0.05, * p<0.1.

this kind of correlation (cf. Simpser (2013), Gehlbach and Madison (2013)). Rather, these patterns suggest sophisticated strategic behavior in which a candidate closely monitors the projected voting outcomes and allocates resources to achieve a vote count just above that of his competitors. This could be achieved through legitimate campaigning or rigging. This is likely to make use of the extensive, systematic and frequent pre-election and election day polling which is routine in U.S. elections, which can be used even for targeting the locations of bring-out-the-vote campaigns even on election day.

In addition, a recent systematic study of elections from a range of other developed and developing countries by Eggers, Hall, and Snyder (2013) shows no such pattern in any other context studied apart from the United States.

In Pakistan, the information available about likely voting outcomes before elections take place is far more limited than in the U.S. context. National media predictions about which electoral outcomes have been highly inaccurate in recent elections. This makes the sort of targeted, precise campaigning of the type observed in U.S. House elections extremely unlikely.

Election rigging is certainly a serious issue in Pakistan. However, election results in Pakistan are announced at the polling station level. Therefore, to rig the elections to a level of precision that would cause problems for our RD strategy, politicians would have to do one of two things. They would either have to pressure or bribe the returning officers in individual polling stations to misreport the numbers, or they would have to deploy voters to vote multiple times or stuff the ballot boxes strategically, only when they face a close race and only enough to just win the race. Again, it seems unlikely given the information available that this could take place to the level of precision required.

However, we conduct a diagnostic check using the full sample of elections in Punjab in 2008. Table 19 shows the split between governing party winners and incumbent winners in close elections within different bandwidths. In fact, both groups won slightly less than half the seats in these close elections; for incumbents the effect becomes significant at an eight percent bandwidth. This does not suggest strategic manipulation, since the sign is the opposite from what would be expected in that case. Our sampling strategy sampled some constituencies within a ten percent margin, but over-sampled constituencies with less than a five percent margin between winners and runners-up; we present the five-percent margin subsample as a robustness check on our results in Table 20, and the results are unchanged.

Finally, we note again that by combining this strategy with the difference-in-differences strategy, we substantially weaken the assumptions necessary for identification: only the time trend, rather than the level, of the dependent variable need be similar between our quasi-experimental "treatment" and

Table 19: Diagnostic test for manipulation at cutoff

Bandwidth	N	Mean (Gov't party won)	t-stat	Mean (Incumbent won)	t-stat
1%	8	.75	1.53	0.5	0.00
2%	13	.62	0.82	0.41	-0.71
3%	16	.56	0.49	0.40	-0.89
4%	21	.52	0.21	0.41	-0.96
5%	24	.46	-0.40	0.35	-1.76
6%	28	.38	-1.31	0.36	-1.81
7%	31	.39	-1.27	0.38	-1.53
8%	33	.42	-0.87	0.38	-1.77*
9%	37	.43	-0.81	0.35	-2.30**
10%	42	.43	-0.92	0.33	-2.64**

Source: Election Commission of Pakistan and authors' calculations.

"control" groups.

Table 20 also shows that the results are robust to a narrower RD bandwidth, i.e. a more conservative definition of a "close election".

Table 20: Robustness to alternative RD bandwidth

	(1)	(2)
	Total value of government assistance	
	10% bandwidth	5% bandwidth
Home village politician in office (Treated)	907.58	861.02
	(245.60)***	(326.02)**
	[264.73]***	[304.87]**
Home village politician ever won (Treatment group)	-122.17	-72.50
	(251.87)	(289.73)
	[230.18]	[243.89]

Dependent variable is value of services received in PKR (100 PKR = 1 USD). Control variables are as specified in Equation 2. Robust standard errors clustered at the village level (in parentheses) or constituency level [in brackets]. *** p<0.01, ** p<0.05, * p<0.1.

6.2 Migration

It is possible that households migrate selectively to politicians' home villages to try to benefit from better public services. We test empirically whether migration into a village differs between our "treatment" and "control" villages, i.e. those with winning and just-losing politicians. Table 21 shows the results. The results show that winners do attract greater migration into their home village, and only among members of other clans (both those of their rivals and others).

Table 21: Main specification on $Y = Migrated$

VARIABLES	(1) HH migrated to village in period T	(2)
Home village politician in office (Treated)	0.01	
	(0.01)	
	[0.01]	
Home-village politician in office (treated) x same clan		-0.03
		(0.02)
		[0.02]
Home village politician in office (treated) x rival clan		0.05
		(0.03)*
		[0.03]*
Home village politician in office (treated) x other clan		0.02
		(0.01)
		[0.01]
Control variables	YES	YES
Observations	6910	6910

Dependent variable is dummy for whether the household migrated into the community in period t. Control variables are as specified in Equation 2. Robust standard clustered at the village level (in parentheses) or constituency level [in brackets]. *** p<0.01, ** p<0.05, * p<0.1.

This migration may indeed be strategic to take advantage of the assistance offered. However, we can check that this effect does not drive our results; we gathered data on households' length of time living in the village in order to perform a robustness check for this. We repeat the main analysis after dropping data for households who report having lived in the village for less than the relevant panel period (11 years). Table 22 shows the results, which are very similar in magnitude and significance to the main results.

6.3 Endogeneity of clan variable

In this paper, we do not analyze friendship or other similarly endogenously formed social network relationships as a link variable, because they are likely to be endogenously formed - people may approach and form relationships with politicians in power in order to benefit from the connection.

Neither of the connection variables (village and clan) that we analyze in this paper are based on on self-reporting in response to a question about the politician, which helps to minimize this issue.

Table 22: Main specification excluding migrants

VARIABLES	(1)	(2)
	Total value	
Home village politician in office (Treated)	892.71	
	(266.09)***	
	[296.26]***	
Home-village politician in office (treated) x same clan		401.78
		(809.76)
		[849.52]
Home village politician in office (treated) x rival clan		479.10
		(530.04)
		[407.85]
Home village politician in office (treated) x other clan		970.93
		(313.93)***
		[367.41]**
Control variables	YES	YES
Observations	6434	6434

Dependent variable is value of government services received. Sample excludes all households who migrated within the panel period. Controls include a vector of household characteristics interacted with round dummies, and a constant. Robust standard errors in parentheses, clustered at the constituency level. *** p<0.01, ** p<0.05, * p<0.1

The politicians' home villages were identified using media and similar sources before the survey was carried out, and confirmed in the field. The clan variable is based on the household's reported clan in the initial component of the questionnaire, in which households are only asked about their basic characteristics, before the politician is even mentioned.

There is research to suggest that over time some groups of people in South Asia have successfully managed to change their caste in response to formal state restrictions or benefits reserved for particular groups - for example, Cassan (2011) finds that in colonial Punjab, after land ownership was reserved for "agricultural castes", some groups were able to successfully manipulate their caste identity over a period of about four decades. This is unlikely to present a problem for our identification strategy for a number of reasons.

First, the time frame over which caste may be manipulated is likely to be much longer than the ten-year period covered in our study, especially in the rural villages which we cover in our study, where social networks are close.

Second, caste and clan are not recognized in any formal way by the state in Pakistan, and there are no benefits reserved for any caste or clan, unlike in British India and modern India (where reservations or "affirmative action" in education are offered to particular caste groups). According to the findings in our preliminary work on the pilot dataset, politicians do seem to direct assistance to their own caste or clan - but this benefit is relatively shortlived, just lasting while that politician is in office, making periodic changes of caste to take advantage of the connection to a specific politician during his term in office seems highly unlikely. In addition, while it might be possible to successfully change one's caste identity in the eyes of the colonial state authorities, doing so in the eyes of the social network of the clan and the politician who is a member of it may be much more difficult.

6.4 Subsets of treatment and control groups

As discussed in Section 4, there were a few constituencies in which we did not identify the home village of one of the politicians. This could bias our estimates. If higher-profile losing politicians are still able to bring resources to their constituency, our estimate of δ will be biased *downwards* towards zero. However, we do a robustness check on the subset of constituencies in which we have matched pairs of home villages of both winners and runners-up. As shown in Table 23, the results are similar in magnitude to our main results, although the coefficient loses significance with the smaller sample size.

As a robustness check, we estimate our main specification, using only the 2008 winners. Table

Table 23: Robustness check: matched pairs

		Total value
Home village politician in office (Treated)	907.58***	674.78**
	(245.60)	(316.48)
	[264.73]	[282.59]
Observations	6910	4654

Dependent variable is value of services received, standardized by the control group mean and standard deviation. Control variables are as specified in Equation 2. Robust standard errors clustered at the village level (parentheses) or constituency level [brackets]. *** p<0.01, ** p<0.05, * p<0.1.

Table 24: Robustness to 2008 winner subset

Home village politician in office (treated)	1691.66**
	(788.35)
	[753.79]
Home village politician won (treatment group)	64.41
	(395.11)
	[362.42]
Observations	5068

Treatment and control groups in this table are 2008 winners and runners-up, respectively. Home villages of 2002 winners and runners-up are excluded from the sample. Dependent variable is value of services received in PKR (100 PKR = 1 USD). Control variables are as specified in Equation 2. Robust standard errors clustered at village level (parentheses) or constituency level [in brackets]. *** p<0.01, ** p<0.05, * p<0.1.

24 shows the results. The coefficient for value is larger than in the full sample, although marginally insignificant (p = 0.11). The marginal effect in the count model is now insignificant, and the point estimate is negative. This is consistent with the fact that in the second round, there was an expansion of one high value program (BISP), while some provincial programs were scaled back (see Table 4).

6.5 Household fixed effects

Tables 25 shows household fixed effects as a robustness check; the results are unchanged.

7 Discussion and conclusions

In this paper, we have shown that politicians in competitive constituencies in Punjab, Pakistan direct substantial amounts of government assistance programs towards their connections, in particular towards households in their home villages. We find that this effect is concentrated on households who are likely to be eligible for these programs, suggesting that politicians work primarily within the parameters of the program, influencing rationing within the eligible rather than directing benefits to

Table 25: Impact of home village politician on Government Assistance: Robustness to Household Fixed Effects

	(1) HH FE	(2) HH FE
	Total value	
Home village politician in office (Treated)	960.70	
	(278.57)***	
	[311.97]***	
Home-village politician in office (treated) x same clan		293.18
		(715.04)
		[669.52]
Home village politician in office (treated) x rival clan		1080.36
		(550.86)*
		[388.33]**
Home village politician in office (treated) x other clan		1020.88
		(338.08)***
		[379.97]**
Control variables	YES	YES
Observations	6910	6910

Dependent variable is value of services received in PKR (100 PKR = 1 USD). Control variables are as specified in Equation 2. Robust standard errors clustered at village level (in parentheses) or constituency level [in brackets]. *** p<0.01, ** p<0.05, * p<0.1.

completely ineligible households. We also find that this effect takes place is concentrated in a few assistance programs despite their formalized targeting processes, suggesting that efforts on the part of governments and donors to reduce political mistargeting through these design features are insufficient and may need to be complemented with other mechanisms to reduce this effect.

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A Multiple inference

Multiple inference, or testing multiple hypotheses or multiple left hand side variables, implies that the probability of getting some spurious effects (rejections of the null) increases much higher than the selected critical value α, the probability of rejection on any one test.

However, the problems associated with testing impact on multiple outcomes have to be weighed against the advantages of exploring a range of possible services where the impact of interest could be found - in our case, different government services and assistance programs, where unexplained variation in targeting may differ dramatically from program to program (based on our observations from our pilot dataset).

The various solutions for this problem include combining outcome variables into summary indices; adjusting p-values using techniques such as the Bonferroni correction, Westfall and Young free step-down resampling method for control of family-wise error rate (FWER) or Benjamini and Hochberg method of False Discovery Rate control (Anderson, 2008, Benjamini and Hochberg, 1995); or the mean effects approach used in Kling, Liebman, and Katz (2007) and Casey, Glennerster, and Miguel (2012), in which all outcome variables are tested separately but the cross-equation restriction that the effect size is zero for a family of outcomes is also tested.

The summary index approach may improve power, but may also be subject to the same sort of effect; in addition, it obscures informative differences between outcome variables, and makes effect sizes difficult to interpret informatively.

The Anderson (2008) approach implies two things: (a) it weights outcome measures that vary little more heavily, reflecting an implicit assumption that their lower variance reflects that they are more difficult to change or that a change in them is more meaningful than in an indicator with higher variance, and (b) it assumes that outcomes that vary together are simply providing redundant information. This makes sense in the context of Anderson (2008) and Casey, Glennerster, and Miguel (2012), in which the outcome variables are really measures of some underlying variable, such as quality of institutions or a child's human capital.

We argue that in our context this does not make sense, because the outcomes are actually distinct programs whose value to recipients is additive. For example, if a single targeting process or list is used to qualify individuals for a scholarship program and a free textbook program, they will have a high covariance. However, the individuals who receive both are still benefiting from both programs, and it does not make sense to assign a lower weight to these programs. In contrast, total value is a meaningful variable, which allows us to estimate meaningful coefficients in terms of additional value

of government services received by the household.

Therefore, we will use a single summary index as a dependent variable, using weights based on an estimate of the value of each government assistance program provided.

The monetary values approach uses weights that have an a priori basis, unlike the approach outlined in Anderson (2008) of weighting outcomes by the inverse of the covariance matrix, or the approach of standardizing outcomes to have the same mean and variance, as in Kling and Liebman (2004) and Casey, Glennerster, and Miguel (2012), which are based on how much the outcomes vary in the data.

We argue the advantages of this approach outlined above outweigh the challenge of approximating the value of the services.

Testing program characteristics and individual programs We also estimate Equation 2 on each of the individual programs listed in Table 3. To test hypotheses relating to groups of programs, we will use Seemingly Unrelated Estimation to test these hypotheses as cross-equation restrictions. [12]

We present the results of regressions on individual programs, both on value and on a binary of receiving the program, with their original p-values as well as with p-values adjusted for multiple inference using the False Discovery Rate control p-value correction of Benjamini and Hochberg (1995). This approach controls the total proportion of false rejections within a set of hypothesis tests; we report the FDR "q-values," analagous to p-values, i.e. for each hypothesis, the lowest false rejection rate at which the null would be rejected.

B Data splitting and archival procedure

An additional innovation designed to prevent any possibility of data mining is the use of archiving a subset of the data, building on and modifying the data splitting and archival procedure used in Fafchamps and Labonne (2013).

For the census data set (N = 13,000), the data were split by a third party researcher, who then anonymized household ID numbers and returned half the data to us; the other half was archived. That is, there are two subsamples of approximately 6,500 households each: a "main" subsample and an "archive" subsample. We do not have access to the archive subsample, and it will not be analyzed until after final reviewer comments (i.e. when the paper has been accepted for publication). At that time we will attempt to replicate all the specifications that use these data on the second half of the

[12] The entire system of equations is not estimated using SUR, since this requires an assumption of homoskedasticity that is expected to be invalid in our context.

data. We will present the results as a part of the final paper.[13]

The advantage of this approach is that it ensures that results are not spurious results of the particular sample used. It allows us to use our archive dataset to test whether the results hold on another sample of data. This is particularly important for any changes or additional specifications we make that were not specified in the pre-analysis plan. The archival procedure gives us the flexibility to make changes or new specifications based on insight from working with the data, and test formally to guard against the concern that results found in such new specifications were driven only by specification search and are not robust.

Since the data splitting approach achieves the same objective as the pre-analysis plan itself, this gives us additional freedom for analysis going beyond the pre-analysis plan, while protecting us against concerns of data mining.

Our situation differs from that of Fafchamps and Labonne (2013) because their sample size - using census data from the Philippines - was several million, eliminating any consideration of power. We therefore modified their methodology in by using a stratified randomization to ensure that the main and archive subsamples are highly comparable. This has two benefits. First, because the main and archive subsamples are highly comparable, this improves the likelihood of non-spurious results from the main subsample replicating when tested on the archive subsample. Second, because the archive subsample is selected from within our statistical clusters, this also reduces the loss of statistical power which results from dropping part of the sample. This technique could be replicated by other researchers to apply the data split methodology to more modest sample sizes.

The matched randomized split was carried out by a third party researcher, using Stata code written by the authors, as follows:

- Stratify all observations into cells by:
  - Village (since villages are linked to particular politicians or are close to their home villages)
  - Zaat / Biradari (caste/clan).
- Within each cell, construct best-paired sets of two households using Mahalanobis matching on the right hand side variables (i.e. clan, relative, and controls).
- If only a single observation existed in a cell, it was paired with another observation in the same village, using the following procedure: match with another household in the same clan; if only

[13] If pooled results analyzed at the final stages (after journal acceptance) find significant results on a particular hypothesis test, whereas power was insufficient for that test due to the smaller size of the split sample, then we will clearly indicate this in the final version, and will show and compare the test results on the main subsample as well as the full sample.

one household existed in a clan, it was matched with another household in the same caste; if only one household existed in the caste, it was pooled with others in the village.

- Randomly select one of each paired match for the main subsample, and one for the archive subsample.
- Assign new, randomized household identification numbers to households.
- Share the main subsample with the authors, and retain the archive subsample without giving the authors access to it.

C Pre-Analysis Plan

In this section we replicate the pre-analysis plan pre-registered on the Experiments in Governance and Politics trial registry. This registration took place in September 2013, before any analysis of the data began. As proposed in Humphreys, Sanchez de la Sierra, and van der Windt (2013), our pre-analysis plan was intended as a comprehensive but non-binding document, to allow for adaptations and improvements but ensure transparency in the process. In Section D, we summarize the changes made in analysis that were not envisioned in the pre-analysis plan.

C.1 Introduction and objective

This pre-analysis plan outlines econometric analysis to be carried out on a primary dataset collected by the Lahore School of Economics in collaboration with CSAE, Oxford University, in February 2013 in selected districts of Punjab, Pakistan. This plan outlines the analysis for one of several papers planned for this dataset. [14]

The purpose of the pre-analysis plan is to register our planned specifications in advance, in order to eliminate potential concerns about data mining in empirical data analysis, which has been shown to have the potential to cause major misinterpretation of results from empirical work, even when carried out inadvertently (Casey, Glennerster, and Miguel, 2012, Simmons, Nelson, and Simonsohn, 2011, Humphreys, Sanchez de la Sierra, and van der Windt, 2013). Following the proposed approach in Humphreys, Sanchez de la Sierra, and van der Windt (2013), we will not restrict ourselves to only analysis outlined in this plan; however, any deviations from the analysis plan will be made clear in the final paper.

[14] This work builds on preliminary work with earlier data from pilot questions built into the PERI survey carried out by Lahore School of Economics in 2011, described in a draft analysis paper on this topic.

We affirm that as of the finalization of this document, which will now be filed in EGAP's hypothesis registry, we have not carried out any data analysis on our 2013 data.

The objective of this paper is to carry out well-identified causal tests of the following two central hypotheses that result from a separate paper developing a simple theoretical model of patronage interactions between politicians, local patrons and households (Vyborny and Chaudhry in progress):

Hypothesis 1 *A link between a politician and a household causes an increase in goods received.*

Hypothesis 2 *A link with a more powerful politician causes an increase in government assistance to the household.*

To develop a richer understanding of the results, this paper will also do the following:

- Test which factors of delivery mechanism and program design strengthen or weaken these two causal relationships.
- Provide indicative tests of possible mechanisms.

C.2 Specifications

Our basic relationship of interest is:

$$g_{it} = \beta_0 + \delta\psi_{ijt} + \eta X_{it} + \epsilon_{it} \tag{5}$$

where δ is the coefficient of interest: the effect of ψ_{ijt}, i.e. household i having a link with the politician, j, who is in office at time t on household receipt of government services and assistance g_{it}; and X_{it} is a vector of household and community characteristics.

In order to identify δ, we sampled competitive constituencies with a change in politician (Member of National Assembly) in the 2008 election. We define "competitive" as follows:

- The vote margin between the top two candidates is less than 5 % of the total votes that the two of them received; and/or
- A "two-way switch" occurred, i.e. politician A was the winner and politician B the runner-up in 2002, then politician B won and A was the runner-up in 2008.

Each hypothesis will be tested using the close elections and two-way switch elections pooled together; we will then test each of these two subsets as a robustness check.

To test Hypothesis 1, we will estimate:

$$g_{it} = \beta_0 T_{1t} + \beta_1 T_{2t} + \beta_3 \psi_{i,j} + \beta_4 \psi_{i,j^*} + \delta \psi_{i,j^*} T_{j^*t} + \eta X_{it} + \epsilon_{it} \tag{6}$$

Where:

- δ is our coefficient of interest: the effect of being connected to a winning politician on g_{it}.
- $\psi_{i,j}$ is a dummy which equals 1 for all t if household i is connected to either the winner or the runner-up politician in a competitive election in either period.[15]
- ψ_{i,j^*} is a dummy which equals 1 if household i is connected to the winner of a competitive election in either period.
- T_{1t} and T_{2t} are dummies for the first and second electoral term in the data, i.e. 2002-2007 and 2008-2013.
- T_{j^*t} is a dummy for the time period in which politician j was in office; so $\psi_{i,j^*} T_{j^*t}$ will equal 1 for households connected to a winning politician in the period in which he was in office.
- For "two-way switches," $\psi_{i,j} = \psi_{i,j^*}$, leading to perfect collinearity, so ψ_{i,j^*} is defined as zero in those cases.

The variables for government services and assistance, g, are listed in Table 27.
ψ is a vector of the following binary link variables:

- *Home village
- Same patwari circle (administrative area) as home village
- Shared zaat (caste)
- *Shared biradari (sub-caste or clan)
- *Relative: binary for any family relationship

If these variables exhibit multicollinearity (any Variance Inflation Factor > 10), we will instead use the subset of variables that is starred above.

[15] All the villages we sampled were home villages or close to home villages of politicians who contested in the 2002 or 2008 elections; all the 2008 races selected were competitive. However, some were also competitive in 2002. This variable will equal zero for households connected to politicians who contested in a non-competitive election. For "two-way switches," both elections are treated as competitive elections.

To test hypothesis 2, we will estimate the interacted model:

$$\begin{aligned} g_{it} =& \beta_1 T_{1t} + \beta_2 T_{2t} + \beta_3 \psi_{i,j} + \beta_4 \psi_{i,j^*} \\ &+ \beta_5 \xi_{j^*} + \beta_6 \xi_{j^*} \psi_{i,j} + \beta_7 \xi_{j^*} \psi_{i,j^*} \\ &+ \delta_1 \psi_{i,j^*} T_{j^*t} + \delta_2 \xi_{j^*} T_{j^*t} + \delta_3 \xi_{j^*} \psi_{i,j^*} T_{j^*t} + \eta X_{it} + \epsilon_{it} \end{aligned} \tag{7}$$

Where:

- ξ is defined as a dummy for whether the politician is affiliated with a party in the governing coalition at the national level.
- ξ_{j^*} is 1 if the winning politician in a competitive election in *either* time period was of a party in the governing coalition;
- $\xi_{j^*} T_{j^*t}$ is 1 if the winning politician in a competitive election in time period t was of a party in the governing coalition.
- In two-way switch constituencies, $\psi_{ij} = \psi_{ij^*}$, leading to perfect collinearity, so the second term is defined as zero.

Here δ_1 identifies the base effect of a link with a winning politician on household receipts of government assistance, δ_2 identifies the effect of the politician being in the governing coalition on the government assistance delivered to all his constituents, and δ_3 identifies the interaction effect: the additional effect for individuals linked to politicians affiliated with the governing coalition.

We will use party affiliation in the 1997 election as an instrument for current party affiliation. The interaction effects will be instrumented by interacting the instrument in the same manner as the interaction term in the main specification. Following Angrist (2001), we use a linear probability model with a dummy endogenous regressor.

In addition, we will test it with a more detailed ξ, defined as a simple sum of:

- MNA is a member of party in governing coalition (without instrumentation);
- MNA holds the position of minister.
- MNA holds a leadership position in party.
- MNA sits on one or more powerful committees.

Tables 26 summarizes these two hypotheses along with the subsidiary hypotheses to be tested, and the exact test to be used for each one.

Table 26: **Hypotheses**

	Hypothesis	Specification	Test
	Basic hypotheses		
1	A link between a politician and a household causes an increase in goods received.	Equation 6	$\delta = 0$; F-test of joint significance of the vector of link variables.
2	A link with a more powerful politician causes an increase in government assistance to the household.	Equation 7	$\delta_3 = 0$; F-test of joint significance of vector of link variables times index of power variables.
	Program design		
1	Programs received by households are more affected than programs received at the sub-village level (e.g. street, neighborhood, school).	Equation 6; 7	Cross-equation restriction that δs are equal across regressions for program in the two categories. See Table 27 for categories.
2	Discretionary programs are more affected than non-discretionary programs.	Equation 6; 7	Cross-equation restriction that δs are equal across regressions for program in the two categories. See Table 27 for categories.
3	Cash programs are more affected than in-kind programs distributed to households.	Equation 6; 7	Cross-equation restriction that δs are equal across regressions for program in the two categories. See Table 27 for categories.
	Mechanisms		
1	Politicians are able to influence who receives programs that are administered at their own level more than those administered at a different level.	Equation 6; 7	Cross-equation restriction that δs are equal across regressions for programs (a) identified, and (b) delivered at national versus provincial level.
2	Politicians influence programs targeted at levels where they have political influence.	Equation 7, with party membership only; add an interaction term for membership in the party that is in power at the provincial level (in addition to existing term for membership at the national level).	Cross-equation restriction $\delta_{n,n} + \delta_{p,p} = \delta_{p,n} + \delta_{n,p}$; where subscripts denote: level at which politician holds power, then level of program administration.

Table 26: **Hypotheses**

	Hypothesis	Specification	Test
3	Politicians work through local patrons: politicians with better connections with local patrons distribute more goods to their connections.	Equation 6; 7; add interaction terms with ψ for: winning and losing politicians' connections to local officials; households' connections to the local officials; and interaction between the two.	F-test on coefficients on interaction terms = 0
4	Politicians assist their connections, regardless of whether those connections are their political constituents.	Equation 6; 7; split ψ into two elements: connection with an in-constituency MNA, and with an out-of-constituency MNA (i.e. who does not represent household i's constituency).	$\delta_{in} = \delta_{out}$
5	Politicians direct assistance to households or blocs of households in exchange for their votes.	Equation 6; 7; add controls for whether the household head voted and proportion of the votes at local polling stations which went to the MNA. This will be estimated on the pooled sample (including pilot data from non-close elections).	Cross-equation restriction: δ in main specification = δ in specification with control added.
6	Politicians give assistance to households they know personally, not more broadly (e.g. to any member of their ethnic group).	Equation 6; 7; add controls for household's reported personal interaction with the MNA (Binaries for knows him; has his telephone number).	Cross-equation restriction: δ on caste / clan and home village elements of ψ in main specification = δ in specification with control added.
7	Politicians assist connected households by sharing information with them (directly or through social networks).	Equation 6; 7; (a) add controls for household's knowledge of program mechanisms / application procedure. (b) split ψ up into two interaction terms: a connection with an incoming politician or an outgoing politician.	(a) Cross-equation restriction: δ on caste / clan and home village elements of ψ in main specification = δ in specification with control added. (b) $\delta_{incoming} = \delta_{outgoing}$
8	Ineligible individuals benefit from the link effect.	Equation 6; 7; add interaction term with eligibility proxies.	Coefficient on interaction term with eligibility proxies = 0

C.3 Specifications

We have observations of a binary for whether a household received each program g for each of the last eleven years: 2002 to early 2013. The politicians we study stood election in the 2002 and 2008 elections. The two aggregate time periods we consider are T_1, 2002 to 2007, inclusive, and T_2, 2008 to 2013, inclusive.

Continuous: Program value We will estimate Equations 6 and 7 using a single summary index as a dependent variable, using weights based on an estimate of the value of each government assistance program provided. We will also test these specifications in program-specific regressions. Here we will use the total value received in an individual program over T as a dependent variable. P-value correction for multiple inference will be used in the individual program regressions. If the index tests do not reject the null, then the program-specific regressions may still be interpreted as a valid test of the hypothesis after p-value correction.

We do not have direct data on the value of services received, but will approximate this using estimates of the value of each program from administrative sources and government survey data, and information from respondents in our survey on how many years they received each program. In the case of programs with benefits that are hard to quantify, such as receiving a new ID card, we use values intended to provide a rough estimate of the marginal cost of providing the service.

We will test this approach using a linear specification.

For the summary index, this will be carried out separately for the full sample (N = 13,000), with the government programs on which we have data for all 13,000 households, and for the long survey subsample (N = 1,000), with the full set of government programs.

Discrete: programs received In addition, we will estimate Equations 6 and 7 on a discrete variable representing distinct programs received.

Our default aggregation method will be to construct a binary for whether household i received the program in *any year* t in aggregate period T.

We will construct the binary dependent variable differently for programs that appear in the data to continue for a long period by default. If in 50% or more of our observations, where a household received a program at least once in 2003 or 2004, they continued to receive it for three years subsequently, we will consider that a long-running program and we will define two binary variables: "new receipt," i.e. binary for household i *started* receiving the program at any year t in aggregate period T (that is, it

received the program at any time t in T, and not having received it at $t-1$); and "end receipt," a binary for household i *started* receiving the program at any year t in aggregate period T (that is, it received the program at any time t in T, and not having received it at $t-1$).

We will analyze these using a Linear Probability Model diff-in-diff. In addition, we will test the robustness of the results to a logit specification.

These regressions will be estimated on a program-by-program basis. Again, multiple inference p-value correction will be used.

We will also test the specifications on a summary index simple sum: the number of distinct programs (or new receipt of continuous programs) that the household received, i.e. the sum of the binary variable just described. This model will be tested using a negative binomial specification.

C.4 Control variable specifications

We will select a parsimonious set of controls and a full set of controls.

C.4.1 Control selection: Full set

To determine the exact set of control variables in the full set and the functional form of continuous controls, we will use the multivariate fractional polynomial procedure developed by Altman and G. (1994), Royston and Sauerbrei (2008) and implemented in Stata's mfp procedure. We will use this procedure on the main specification *without* the main independent variable of interest.

This procedure will be carried out using a simple sum of all the g_{it} variables in each family (see Table 27) as the dependent variable. Separate sets of controls will thus be developed for each category listed in Table 27, since different programs are likely to be determined by different key household characteristics.

C.4.2 Parsimonious set

To determine the parsimonious set, we will divide the full set into two subsets.

First, the subset of "eligibility variables," which a priori we expect will predict a large share of variation in government services targeting. These will all be included in the parsimonious set in order to increase precision of the estimate of $\hat{\delta}$. These are listed in Table 27.

Among the remaining control variables, we will select regressors that are not balanced across the control and treatment group (i.e. those for which a statistically significant difference exists at the 5

% level between households who share the clan of the just-winning candidate and those who share the clan of the just-losing candidate).

The full set of potential control variables gathered in the questionnaire will be made public as an appendix to the paper.

Table 27: **Public programs**

Program / Service	Sample	Description	Eligibility variables	Delivery level	Discretionary	Cash or in-kind
Social safety net transfers						
BISP pre 2009	Full	Unconditional cash transfer for needy HHs	Female head; rudimentary housing; no land	HH	Yes	Cash
BISP post 2009	Full	Unconditional cash transfer for needy HHs	Female head; rudimentary housing; no land	HH	No	Cash
Zakat	Full	Unconditional cash transfer for needy HHs	Female head; rudimentary housing; no land	HH	Yes	Cash
Sasta rashan / Ramzan rashan	Full	Free or reduced-price food supplies	Female head; rudimentary housing; no land	HH	Yes	In-kind
Education - transfers						
Girls' Stipend Program	Full	Conditional cash transfer to HHs with girls in school	School aged girls	HH	No	Cash
Free textbooks	Full	Free textbooks to children in government schools	Children in government school (full); children of school age (subsample)	Sub-village	No	In-kind
Education - staffing						

Table 27: **Public programs**

Program / Service	Sample	Description	Eligibility variables	Delivery level	Discretionary	Cash or in-kind
Public school teacher presence in child's school	Full; subset with children of primary school age. [16]	Teacher presence (days) / class size	Children in government school (full); children of school age (subsample)	Sub-village	N/A	N/A
Infrastructure (newly built in year t)						
Drains outside household's dwelling	Subsample		N/A	Sub-village	N/A	N/A
Paved street outside household's dwelling	Subsample		N/A	Sub-village	N/A	N/A
Gas supply to household's dwelling	Subsample		N/A	Sub-village	N/A	N/A
Electricity supply to household's dwelling	Subsample		N/A	Sub-village	N/A	N/A
Land line connection to household's dwelling	Subsample		N/A	Sub-village	N/A	N/A
Piped water to household's dwelling	Subsample		N/A	Sub-village	N/A	N/A
Irrigation water allocation to household	Subsample		Owns agricultural land [17]	Individual	N/A	N/A
Infrastructure of public school attended by household's children	Full		N/A	Sub-village	N/A	N/A
Identity cards (newly issued in year t)						

[16] If children are not in school or are in private school, use average value for government primary schools in village.
[17] To be used as a control, but not included in the "ineligible individuals" hypothesis.

Table 27: **Public programs**

Program / Service	Sample	Description	Eligibility variables	Delivery level	Discretionary	Cash or in-kind
ID card	Full		None	HH	N/A	N/A
Public sector jobs (tested separately, not in summary index of value)						
Public sector employment	Full		Education of HH head is above primary	HH	N/A	N/A

D Modifications from pre-analysis plan

- We have not yet completed analysis of variables from the other survey instruments besides the large sample short questionnaire. This includes the subsample data (infrastructure), school survey data, and the local official survey. We are also still in the process of analyzing political power variables.

- Because the control variables we have available in the short questionnaire are almost exclusively binary variables, we have not used the fractional polynomial approach described in the PAP with this dataset.

- We decided to keep analysis of government employment for a separate paper, because the conceptual issues surrounding employment are substantially different from those related to public services.

- On further consideration of the clan analysis, we realized that our unique sample of political villages implies that analysis of any other connection variable is very specific to this kind of sample - in a sense it already incorporates an interaction effect. Therefore it made more sense to model clan as an interaction effect, rather than on its own.

- We omitted "two-way switch" races with a greater than 10% vote margin from the analysis. We decided to use a more conservative definition of competitive elections rather than both definitions in order to achieve a cleaner and clearer identification strategy. However, as shown in Table 28, the results are unchanged with or without the inclusion of these races.

- We improved the RD specification by following the subsample selection procedure discussed in 3. This is a more conservative approach than the one outlined in the PAP. The new approach is equivalent to dropping observations outside a bandwidth, rather than modeling the relationship of the running variable parametrically.

- There was substantial overlap in the clan (quom/zaat) and caste/subcaste (biradari) data. We simplified the caste/clan variables into a single variable and consider a match between either level to be a common clan link.

- The PAP incorrectly specified that we sampled constituencies based on a 5% bandwidth.

- The PAP omitted two government programs included in the questionnaire and planned for analysis: the Nawaz Sharif fund (full sample, but only X households reported receiving this program),

Table 28: Alternative definition of competitive elections: "Two-way switch"

	(1)	(2)
	Total value of government services received	
Close election margin definition of treatment:		
Home village politician in office (Treated)	862.24***	
	(283.83)	
Home village politician ever won (Treatment group)	-70.18	
	(205.79)	
"Two-way switch" definition of treatment:		
Home village politician in office (Treated)		795.18**
		(356.14)
Home village politician ever won (Treatment group)		-92.18
		(242.70)
Observations	6998	6998

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

and PASSCO government wheat procurement (sub-sample only; will be analyzed in a future version of the paper).

E Sampling procedure

The data used in this paper was collected through a household survey carried out in 61 different rural clusters of Punjab, Pakistan. As mentioned in Data section, the data collection was designed to incorporate regression discontinuity design. Thus only competitive constituencies were surveyed. This appendix explains all the steps taken towards data collection.

Constituencies were selected using a stratified sample using the following procedure:

1. Select all NA constituencies that have two close elections in a row (<5% difference between number of votes received by winner and number of votes by runner-up), in which biradari of MNA changed

2. Select all NA constituencies with 5% bandwidth for the 2008 election only, in which biradari of MNA changed and girls' stipends are offered in the districts

3. Select all NA constituencies with 7% bandwidth for the 2008 election only, in which biradari of MNA changed

4. Select 11 of the 14 NA constituencies that have a two-way switch (i.e. person A was winner of MNA election in 2002, person B was runner-up of MNA election in 2002; then B won in 2008 and A was runner-up) and biradari change of MNA. For cost reasons, we selected all such constituencies that were withindistricts already selected. Then to complete the sample we drew three using a random sample from the remaining such constituencies.

After the selection of competitive constituencies, we identified the home villages of the winner and runner up in both the 2002 and 2008 elections. For a subset of politicians, we also selected neighboring villages. We selected all the villages which had been included in the random sample of the 2007-8 MICS, which were in the same *patwari* circle as the politicians' home villages.[18]

On arrival in the village, the field team selected an area for survey within the village. The team supervisor first mapped out the village population with a key informant. If a village had fewer than 250 households then the whole village was surveyed. Otherwise the team divided the village map into segments of roughly 200-250 households, numbered them, and then randomly selected one by drawing its number out of a hat. Some villages were designated to have two such segments sampled in a village, to oversample areas eligible for the girls' stipend program.

The cluster selection was followed by questionnaire filling exercise. In each cluster, we carried out four different questionnaires:

- Official survey Official survey was conducted with at least two officials based in the same village who were selected based on the following instruction: This official survey will be held with the help of nambardar and patwari of the village. Supervisor as per the name given below will ask of the presence of these people and will select the first two as per the list.
  - Numberdar
  - Other Numberdar
  - Patwari
  - Other Patwari
  - Local landlord who own more than 50 acre land
  - Member of Zakat committee

  In case none of them is present, supervisor should ask of the one who had the knowledge of the area.

[18] The purpose of this was to form a panel dataset, but this was not successful due to problems with identifying and matching the MICS households.

The official survey for each cluster was pre-filled with the names of national assembly candidates identified in the first step using administrative data. At the time of official survey, further information was collected to pre-fill both household surveys with the names of local officials, and make a listing of public schools in the village.

- Household census. A two-page long census was conducted with all the households residing in the selected cluster. The questionnaire was pre-filled with the name of national assembly candidates and other village officials identified in the official survey. The data collected in the census also flagged those households who belonged to a family or were relative of an official listed in the questionnaire.

- Long household questionnaire. For the long questionnaire, total of a 10-16 households were selected randomly in the field using the following criteria:

  - oversampling of households related to village officials identified in census
  - oversampling of households who had been eligible based on their household composition for the girls stipend program (i.e. daughters of the household were currently between ages 16-30). This is an exogenous criterion which increased the proportion of households in which we could observe the stipend program. This increases our statistical power, since these programs are only distributed to a small proportion of households.
  - Repeat the sample of MICS households (only in those cluster where the Punjab government MICS, or Multiple Indicator Cluster Survey, was conducted in 2007-8); or random selection of households from the entire pool of households to replace MICS households that were not identified or in clusters where the MICS did not take place.

  The data from this segment is only used in the "program knowledge" component of this chapter.

- Headmaster survey. A headmaster survey was conducted with at least one boys and girls government primary school. In all those cases where more than one school identified in a village, enumerators were asked to survey the school with highest enrollment. The data from this component of the survey is not analyzed in this chapter.