Nepotism and Punishment: The (Mis-)Performance of Elected Local Officials in the Philippines*

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Abstract

In this paper, we employ a novel estimation strategy to estimate both the value of being connected to a local politician currently in office and the cost of being connected to a local politician in opposition. We use data collected in 2008 on 46,000 households in municipalities of the Philippines along with information on both successful and unsuccessful candidates in the 2007 and 2010 municipal elections. To account for the endogeneity of connections to local politicians, we construct a control group consisting of households related to candidates in the 2010 elections that took place after the data were collected. There is evidence that elected officials' relatives are more likely to be enrolled in a subsidized health insurance program but this effect does not appear to be causal. There is, however, robust evidence consistent with a causal impact of being related to a local official on the probability of being employed in the public sector. Strikingly, we also show that relatives of unsuccessful candidates in the 2007 elections are much less likely to work in the public sector. Findings are consistent with the view that local officials have the ability to rewards their relatives and to punish their political opponents.

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1 Introduction

Decentralizing service delivery and the identification of beneficiaries of anti-poverty programs is expected to lead to improved outcomes as local officials have more accurate information on local preferences and conditions (Wallis and Oates 1988). Those theoretical benefits rely on the assumption that officials are benevolent. Gains from decentralization ultimately depend on whether local governments are more or less likely to be captured by special interest than national governments (Bardhan and Mookherjee 2000).

While there is evidence of elite capture in certain contexts (Platteau and Gaspart 2003 Besley, Pande, Rahman, and Rao 2004, among others), whether improved information or elite capture dominate in a given setting remains an open empirical question. In a recent randomized experiment, Alatas, Banerjee, Hanna, Olken, and Tobias (2010) compare the relative effectiveness of proxy-means test, a top-down approach, and community targeting to identify the poor in Indonesia. While the two methods do not lead to the same rankings, the differences do not seem to be driven by elite capture but rather by the use of different concepts of poverty, leading to greater satisfaction with the outcome under community targeting. In other contexts, local officials have been shown to discriminate against the poor in areas where wealth inequality is higher, even though it should be easier to identify the poor (Galasso and Ravallion 2005). The difference between the two sets of findings could stem from the fact that community targeting in the Indonesia experiment was well-structured and undertaken in collaboration with external facilitators, hereby reducing opportunities for capture. Alternatively, since it was a one-off experiment, local elites might not have had time to learn how to game the system to their advantage (Alatas, Banerjee, Hanna, Olken, and Tobias 2010).

Related to the literature on elite capture, researchers have sought to estimate the value of political connections. Due to data limitations, it has been easier to establish the causal impacts of political connections for firms than for households. For example, Fisman (2001), using stock market valuation data on Indonesian firms, is able to credibly establish that firms connected with Suharto earned sizable rents. Conversely, Caeyers and Dercon (2008) find that, in rural Ethiopia, households connected to local officials are more likely to receive food aid but they are unable to test whether the effect is causal. Further, an area that has received far less attention in the empirical literature is the potential for nepotistic behavior in public sector employment at the local level. Indeed, where accountability is low, officials

might be able to capture rents by favoring relatives and friends in hiring decisions. While the short-term impacts could be positive if officials are better able to supervise their relatives, long-term impacts are most likely nefarious as it might weaken incentives to invest in human capital for unconnected individuals.

The literature on the value of political connections on household welfare suffers from a variety of shortcomings. First, the endogeneity of connections is rarely adequately controlled for. Indeed, households connected to politicians are likely to differ from average citizens along a number of unobservable characteristics which would positively affect their welfare even if their relatives were not in office. As a result, researchers are only able to estimate correlations between connections and the outcomes of interest. Second, the literature focuses on links to politicians currently in office and, as a result, tends to identify benefits from the connections. However, it is possible that households connected to politicians in the opposition suffer from their connections, especially in areas where elected officials have discretionary powers. For example, Hsieh, Miguel, Ortega, and Rodriguez (2011) find that in Venezuela Chavez's opponents earn less and are less likely to be employed. Third, for lack of better data, researchers often have to rely on self-reported information on connections to local elites which might not be reliable as individuals who benefit from connections might be more likely to report them.

In this paper, we measure the value of political connections in rural municipalities of the Philippines. We use data on more than 46,000 households collected in late 2008 by the Department of Social Welfare and Development (DSWD). We employ a novel estimation strategy to test whether local politicians' relatives are more likely to be enrolled in PhilHealth Indigent Program (PHIP), a subsidized health insurance program and to be employed in the public sector. First, we distinguish between households connected to successful candidates in the 2007 local elections and relatives of unsuccessful candidates in those elections. This allows me to estimate both the value of being connected to an elected local official and the cost of being connected to a politician in opposition. Second, to account for the endogeneity of connections to local politicians, we use households connected to candidates in the 2010 local elections, who did not run in 2007, as a control group. Indeed, even if their relative was not currently in office, connected households might have access to better information about government programs which would, for example, affect their ability to lobby for inclusion

in the list of eligible households for the subsidized health insurance program. Given that households connected to candidates in the 2010 elections are likely to have access to similar information as officials relatives, they should provide a credible control group. In addition, we rely on naming conventions in the Philippines, ensuring that related households share a last or a middle name, to identify households related to local politicians and thus we do not have to rely on self-reported connections.

We find that households related to local officials are more likely to be enrolled in the subsidized health insurance program. This result is robust to controlling for a number of household characteristics that could affect the likelihood of being enrolled in the program. However, once we compare officials' relatives to households related to candidates in the 2010 elections, this effect mostly goes away. There is no evidence that relatives of unsuccessful candidates in past elections are less likely to be PHIP members, however.

We find that family members of local elected officials are more likely to be employed in the public sector. This effect persists after we control for a number of individual characteristics and when we compare those individuals with relatives of candidates in the 2010 elections. The most conservative estimate indicates that being related to an elected local official increase the probability of public sector employment by more than 36 percent, or 2.9 percentage points.

Conversely, individuals connected to candidates who lost the 2007 elections are 1.7 percentage point less likely to work in the public sector than individuals connected to 2010 candidates. This is equivalent to a more than 20 percent drop in the baseline probability of being a public sector employee. This is consistent with the argument that local politicians punish candidates who ran against them in past elections. In addition, households connected to candidates who were in office in the period 2004-2007 and relatives of candidates who were not are equally affected, suggesting that results are not driven by households related to previous office holders that decided to quit the public sector when their relative lost the 2007 elections.

A potential explanation for the difference between this set of results and the previous results on health insurance is that public sector employees might be expected to engage in behavior favoring incumbents prior to the elections and, as such, local officials might be reluctant to staff their bureaucracy with their opponents' relatives.[1] For example, Sidel

[1]The following passage by Cullinane (2009, p 190) is informative: 'During the 1984 election campaign for the National Assembly, Ramon Durano, the aging Cebuano warlord, was questioned by a foreign journalist

(1999) argue that municipal mayors have used their control over tax collection and regulatory enforcement not only to enrich themselves but also to gain electoral rewards.

This paper provides a strong note of caution in debates over decentralization in areas of weak accountability, a description that fits most rural municipalities in the Philippines (De Dios 2007). In such settings, local officials might be able to favor their relatives in their hiring decisions. Further research is needed to understand the officials' motivations for engaging in nepotistic behavior. Indeed, they could do so to capture rents or to secure their relatives' votes in future elections. Alternatively, they might be driven by altruistic motives towards their relatives. Finally, if local officials are indeed able to punish their political opponents' relatives, it is likely to have negative long-term impacts on electoral competition and, as a result, on the quality of political leadership at the local level.

Results discussed in this paper have implications for the literature on the value of political connections. First, in the absence of an adequate control group, estimates of the value of connections to local officials will tend to be upward biased. Second, researchers interested in using households connected to candidates who barely lost an election as a control group need to provide evidence that they did not suffer from their connections. Indeed, available evidence indicates that the stable unit treatment value assumption might be violated which, in this case, would lead to upward biased estimates.

The paper is organized as follows. The setting is presented in Section 2. The estimation strategy is discussed in Section 3 and the data in Section 4. Results are discussed in Section 5 and Section 6 concludes.

2 The Setting

After the fall of Marcos in 1986, the Philippines engaged on a path towards decentralization. One of the most acclaimed law passed by the Aquino administration is the 1991 Local Government Code (Republic Act 7160), which transferred a number of responsibilities along with fiscal resources to local government units at the provincial, municipal and village level. The code was expected to improve the delivery of basic services and to make the bureaucracy more efficient (Brillantes 1992).

about the many family members holding government positions in Danao City. Durano informed the reporter that **"politics is not something you can entrust to non-relatives"**'(emphasis added).

After two decades, the benefits of decentralization are far from clear, however. While some local governments have innovated and improved, a large number of them experienced limited gains in quality of service delivery (Capuno 2007). While this situation is often blamed on inadequate fiscal arrangements, additional resources are not used effectively due to poor governance practices at the local level. As some had originally feared (Brillantes 1992), municipal mayors tend to use their new resources and discretion to prolong their time in office. The primary drivers of resource allocations tend to be political considerations. For example, when the Department of Social Welfare and Development started implementing a large-scale conditional cash transfer program in 2008, it was deemed necessary to establish a centralized targeting system rather than to rely on local officials to identify beneficiaries.

In 1997, the government launched the PhilHealth Indigent program, a subsidized health insurance program to improve health outcomes of the poor. Selected households receive a PhilHealth card, valid for a year, which entitle them and their dependents to a range of health services in accredited hospitals and health stations. Premiums contributions are paid by the local and the national government. The sharing rule depends on poverty levels and length of program implementation, with poorer localities contributing less and contributions increasing over time.

In line with the spirit of the 1991 Local Government Code, enrollment decisions rest with local officials. The target population is the 25% poorest households in the municipality. According to Reyes (2006) PHIP members are expected to be identified as follows: first, the municipal social worker identifies poor villages in the municipality. A survey is then carried out in those villages and households with reported income below a given threshold are eligible for the subsidy. Finally, municipal staff review the list of eligible households and makes the final decision on enrollment (Capuno 2006).

The selection is believed to be influenced by local political considerations and, as a result, to be fraught with both inclusion and exclusion errors. For example, the World Bank estimates that about 40-50 percent of PhilHealth Indigent Program members are classified as non-poor (World Bank 2010). Further, anecdotal evidence is consistent with the argument that local politicians influence the selection procedures to benefit their relatives and friends.

Another area where local officials are believed to have significant discretionary powers is public sector employment as a large number of staff from national agencies, and associated

hiring powers, were transferred to municipalities (Hodder 2009).[2] Again, available evidence indicates that, in addition to favoring their relatives and friends, local officials use their discretionary power over local public employment for electoral gains (Sidel 1999).

Finally, there is qualitative evidence that Filipino politicians have the ability to punish their opponents. For example, Marcos "used his martial-law powers to punish enemies among the old oligarchy, stripping them of assets" (McCoy 2009a, p 17). One of the most well-known examples involves the relationship between Ferdinand Marcos and Fernando Lopez (McCoy 2009b). While Lopez was Marcos' running mate in the 1965 and 1969 elections, a rift occurred in 1971. Marcos imprisoned one of Lopez's nephews on dubious charges and forced Lopez's brother Eugenio to sell his shares in the country's leading utility. He was also stripped of his media empire and suffered total losses amounting to millions of US dollars (McCoy 2009b). Highlighting the benefits of being related to an elected politician, some of the assets were transferred to Marcos' brother-in-law, Benjamin Romualdez.

3 Estimation Strategy

In this section we present the empirical strategy. We start by discussing the approach that we follow to test whether local politicians' relatives are more or less likely to be enrolled in the subsidized health insurance program. We then attempt to assess if being linked with a local politician influences occupational choice.

3.1 Enrollment in the subsidized health insurance program

We start by estimating a simple linear probability model. Specifically, let Y_{ij} be a dummy variable which equals one if household i in village j is a PHIP member, the subsidized health insurance program. It is modeled as follows:

$$Y_{ij} = \alpha P_{ij} + u_{ij} \tag{1}$$

where α is the parameter of interest and P_{ij} is a dummy variable which equals one if the

[2]Nepotism was a common occurrence even before decentralization. For example, in 1963, the Durano clan was accused of having relatives run 10 public offices in Danao City: the mayor, the vice-mayor, the president of the city council, the chief of police, the city assessor, the Bureau of Internal Revenue collecting agent, the city health officer, the city medical officer, the supervising nurse and the school division's head nurse (Cullinane 2009, p 210).

household is a relative of either the mayor or the vice-mayor and, u_{ij} is the usual idiosyncratic error term. We cluster standard errors at the village-level.

In addition to the simple linear probability model, we report results from probit models. Similar to test of discrimination in the labor market, a positive α would not be sufficient to conclude that officials are engaging in nepotistic behavior. Indeed u_{ij} might be correlated with P_{ij} and as such a positive coefficient might simply capture the fact that connected households differ from the average citizens along a range of characteristics that could affect both eligibility and their ability to lobby officials. We first attempt to deal with this concern by including a range of observable household characteristics, such as household composition and educational achievement of the household head, that could affect the likelihood of receiving the program. We augment equation (1) as follows:

$$Y_{ij} = \alpha P_{ij} + f(W_{ij}) + \beta X_{ij} + v_j + u_{ij} \tag{2}$$

where $f(.)$ is a functional form and β parameters to be estimated, W_{ij} is a measure of welfare for household i in village j,[3] X_{ij} is a vector of household characteristics and, v_j is an unobservable characteristic that affects all households in village j equally.[4]

When estimating equation (2) as a linear probability or as a probit model, we assume that f is a quadratic function: $f(W_{ij}) = f_1 W_{ij} + f_2 W_{ij}^2$. In addition, to allow for a more flexible relationship between wealth and the probability of being a PHIP member, we also estimate equation (2) using semi-parametric methods. Specifically, we focus on results obtained with the differencing approach (Yatchew 2003). This method delivers unbiased and consistent estimates and, when the vector X include a large number of covariates, it is less computationally intensive than alternative methods such as the double-residuals or back-fitting. We implement this procedure with the plreg command in Stata.

[3] A potential concern is that local communities might perceive poverty differently and thus might not consider per capita income to be an adequate measure of poverty. One source of variation could arise if local officials did not use per capita income but rather income per adult equivalent to compute poverty measures. This would capture potential economies of scale in the household and the fact that needs vary with age of household member. To account for this possibility we reproduce the results with different equivalence scales.

[4] Given the number of observations per group in our sample, we are not concerned about potential bias due to the incidental parameter problem. Indeed, models of binary choice with fixed effects suffer from severe inconsistency and bias when the number of observations per groups is small but, when the number of observations per group is sufficiently large, the bias disappears and the estimates are consistent (Greene 2004). Given that we have on average 356 households per village (minimum 41, median 240 and maximum 2262), we argue that our estimates are consistent.

While this is the favored approach in the literature (*e.g.,* Caeyers and Dercon (2008)), equation (2) delivers biased estimates of the value of political connections in the likely situation where households connected to local officials differ from other citizens along characteristics not included in X. For example, even if their relative was not currently in office, connected households might have access to better information about government programs which would affect their ability to lobby for inclusion in the list of eligible households. To deal with those concerns, we implement a novel estimation strategy which consists of comparing households connected to individuals currently holding elected office with households with similar connections to individuals that are about to launch their political careers. Specifically, we augment equation (2) as follows:

$$Y_{ij} = f(W_{ij}) + \alpha P_{ij} + \beta X_{ij} + \gamma C_{ij} + v_j + u_{ij} \tag{3}$$

where γ is a parameter to be estimated and C_{ij} is a dummy variable which equals one if the household is a relative of a candidate for either the position of mayor or for the position of vice-mayor in the 2010 elections.

Rejecting the null hypothesis $H_o : \alpha = \gamma$ against the one-sided alternative $H_a : \alpha > \gamma$ would provide more credible evidence that local officials are giving preferential treatment to their relatives.[5] The best estimate of the value of political connections at the local level would then be $\alpha - \gamma$. The idea behind this approach is that future candidates' relatives are likely to be similar to officials' relatives along unobservables and as such γ captures the extent to which officials' relatives would be more likely to be enrolled in PHIP even if their relative was not in office.

Estimating equation (3) on the full sample leads to efficiency gains as we have more observations to estimate f and β. However, those gains require the assumption that f and β are similar for connected and unconnected households. As a result, we estimate equations (1) and (2) on the sub-sample of households connected to either elected local officials or to candidates in the 2010 local elections. Under those specifications, a positive α would provide further evidence consistent with the view that local officials favor their relatives when selecting PHIP members.

[5] We carry out the one-sided test in the following reparameterized model: $Y_{ij} = f(W_{ij}) + \phi P_{ij} + \beta X_{ij} + \gamma(C_{ij} + P_{ij}) + v_j + u_{ij}$. Specifically, we test the null hypothesis $H_0 : \phi = 0$ against the one-sided alternative $H_a : \phi > 0$.

In addition, to test whether households connected to politicians that ran against current elected officials in the 2007 elections suffer from their connections, we compare outcomes for relatives of 2007 candidates with outcomes for relatives of 2010 candidates. We estimate an equation of the form:

$$Y_{ij} = f(W_{ij}) + \beta X_{ij} + \gamma C_{ij} + +\phi L_{ij} + v_j + u_{ij} \tag{4}$$

where ϕ is a parameter to be estimated and L_{ij} is a dummy variable which equals one if the household is a relative of an unsuccessful candidate for either the position of mayor or for the position of vice-mayor in the 2007 elections.

As above, rejecting the null hypothesis $H_o : \phi = \gamma$ against the one-sided alternative $H_a : \phi < \gamma$ would be consistent with the argument that local officials punish their opponents. Importantly, we only classify a household as being related to a 2010 candidate if the relative did not also compete in the 2007 elections. Indeed, households connected to candidates who lost in 2007 might have suffered from their connections and would not provide an adequate control group. In addition, we estimate equation (4) on the sub-sample of households connected to unsuccessful candidates in the 2007 elections or to candidates in the 2010 elections.

3.2 Public sector employment

We then turn our attention to exploring whether individuals connected to local politicians are more or less likely to be employed in the public sector. Let PSE_{ijk} be a dummy variable which equals one if individual i in household j in village k is employed in the public sector. We estimate an equation of the form:

$$PSE_{ijk} = \alpha P_{jk} + \beta X_{ijk} + v_k + u_{ijk} \tag{5}$$

where α and β are parameters to be estimated, P_{jk} is a dummy which equals one if household j is related to elected officials, X_{ijk} is a vector of individual characteristics, v_k is an unobservable characteristic that affects all households in village k equally and, u_{ijk} is the usual idiosyncratic error term. we present results from both linear probability and probit models.

As above, equation (5) only uncovers correlations between being connected to a local official and being a public sector employee. Indeed, even after controlling for individual charac-

teristics included in vector X, connected households might differ along important unobserved characteristics that would prevent me from giving a causal interpretation to the estimated coefficient. If, for example, the notion of "public service" is correlated among individuals in an extended family, the parameter α might simply capture public spiritedness. Similarly, connected households could have better information about job openings. To deal with those concerns, we control for whether the household is related to one of the candidates in the 2010 elections and perform one-sided tests of equality of the relevant coefficients. Specifically, we estimate:

$$PSE_{ijk} = \alpha P_{jk} + \beta X_{ijk} + \gamma C_{jk} + v_k + u_{ijk} \tag{6}$$

we also estimate equation (7) on the sub-sample of households connected to either elected local officials or to candidates in the 2010 local elections.

Finally, to test whether individuals related to local politicians in opposition suffer from their connections, we estimate:

$$PSE_{ijk} = \beta X_{ijk} + \gamma C_{jk} + \phi L_{jk} + v_k + u_{ijk} \tag{7}$$

where ϕ is a parameter to be estimated and L_{jk} is a dummy variable which equals one if household j is a relative of an unsuccessful candidate for either the position of mayor or for the position of vice-mayor in the 2007 elections.

4 Data

The main source of data is the National Household Targeting System (NHTS). The data are collected on all households in municipalities participating in the *Pantawid Pamilyia Pilipino Program*, a large-scale conditional cash transfer (CCT) program.[6] They are then used to predict per capita income through a Proxy Means Test and to determine eligibility in the CCT program. We have data, collected in late 2008, on almost 213,000 individuals in more than 46,000 households in 8 rural municipalities of the Philippines. For each household,

[6]In practice, not all households are interviewed but comparisons with municipal population count from the 2007 census do not indicate any major discrepancy.

we have data on, among other things, a predicted per capita income measure,[7] household composition, education and whether the household is enrolled in the PhilHealth Indigent Program.

We gathered names of all candidates for the positions of mayors, vice-mayors in the 2007 and 2010 local elections from the Commission on Elections (COMELEC). We measure family connections to mayors and vice-mayors that were elected in 2007, and thus in power at the time of data collection. We also identify households connected to candidates for the same positions in the 2010 local elections. Using the program evaluation terminology, those households serve as a control group. Further, to test whether elected officials punish their opponents in past elections, we measure connections to unsuccessful candidates in the 2007 local elections.

We take advantage of naming conventions in the Philippines to assess blood and marriage links between households.[8] Specifically, we use the fact that a man's last name is his father's and his middle name is his mother's. Similar conventions apply to unmarried women. However, a married woman has her husband's last name and her middle name is her maiden name. The NHTS data include information on the first, middle and last names of all individuals surveyed. Using this information, a household is classified as being related to a local elected official if one of its members either has a middle name or a last name that match either the mayor or the vice-mayor's last name. We use the same method to assess links to unsuccessful candidates for the positions of mayors and vice-mayors in 2007 and to candidates for the positions of mayors and vice-mayors in 2010. Given than some of the 2007 candidates decided to run again in 2010, we only classify as relatives of 2010 candidates, relatives of candidates who did not run in 2007.

While conventions ensure that related households share a last or a middle name, our interpretation of the results could be challenged if names were too common. For example, if individuals from the same ethnic group all shared the same last name, results would capture ethnic networks rather than nepotism. We argue that, in our sample, sharing a last or a

[7]The formula used to compute per capita income was calibrated with data from the nationally representative 2006 Family Income and Expenditure Survey using variables that do not vary greatly through time. It is thus expected to capture permanent income. The exact formula is not public.

[8]Angelucci, De Giorgi, Rangel, and Rasul (2010) use a similar strategy to measure family networks in Mexico. They find evidence consistent with the argument that PROGRESA beneficiaries shared some of their resources with family members which did not participate in the program (Angelucci, De Giorgi, Rangel, and Rasul 2010). Similarly, the strategy has been used to assess blood links between Filipino politicians through time (Querubin 2010 Querubin 2011).

middle name is a good indicator of family ties. First, names used in the Philippines were imposed by Spanish colonial officials in the mid-19th century to facilitate census-taking and identification of families (Scott 1998). Last names were selected from the *Catalogo alfabetico de apellidos*, a list of Spanish surnames and thus did not reflect pre-existing ties. Second, there is a lot of heterogeneity in names used at the local level, reducing concerns that names capture similar ethnic background or other group membership. In the small municipalities that characterize our sample, the average last name is shared by only 2.6 households per municipality (or about 0.02 percent of household). No last name is used by more than 3.4 percent of households in any municipality. There is also a great diversity of names. We compute an index equal to $1 - \sum s_i^2$, where s_i is the share of households in the municipality using name i. The index is higher than 0.99 in all municipalities, indicating a high level of heterogeneity.

The method described above generates credible measures of links. Indeed, 0.5 percent of households are classified as being connected to a local official. Put differently, the average local official is connected to 16 households in his/her municipality. Further, 1.2 percent of households are connected to individuals who ran in the 2007 elections and another 1.2 percent are connected to candidates in the 2010 elections (but not in the 2007 elections).

The NHTS data include information on the sector of occupation of all individuals surveyed. The classification include 12 categories.[9] Since there is no public sector employment category, and given the extensive transfer of social services staff to municipalities, we proxy it with the category "Community, social, recreational, personal services".[10] While this category include services that are not provided by the public sector, we argue that this should not significantly affect results discussed in this paper. Indeed, the official poverty rate in the sample municipalities is 62 percent and, as a result, personal services are unlikely to employ a large share of the workforce. Further, if this proxy was actually capturing services provided by the private sector, we should observe, at the municipal level, a positive correlation between

[9]The categories are: (i) Crop, farming, gardening; (ii) Livestock, poultry raising; (iii) Fishing; (iv) Forestry, hunting; (v) Wholesale, retail; (vi) Manufacturing; (vii) Community, social recreational, personal services; (viii) Transportation, storage, communication services; (ix) Mining, quarrying; (x) Construction; (xi) Entrepreneurial activities and; (xii) Others.

[10]**Community, Social and Personal Services:** these include public administration and defense, sanitary and similar services, educational services, medical, dental, other health and veterinary services, other social and related community services, recreational and cultural services, personal and household services, restaurants and hotels and international organizations and other extra-territorial bodies. (http://www.bles.dole.gov.ph/HTML%20FILES/glossary2.html#cd027 visited on March 15,2011)

the share of individuals employed in the category "Community, social recreational, personal services" and the share of individuals engaged in "Entrepreneurial activities". However, the correlation is negative (-.21).

There are two sources of measurement error in our measure of links. First, even in the small municipalities that characterize our sample, it is possible that non-related households share the same last name. Second, data entry errors might have led to some names being mis-spelled (*e.g.,* De Los Reyes spelled De Los Reyez). Those sources of measurement errors generate attenuation bias which will work *against* finding a positive effect of being related to a local official on either being enrolled in PHIP or of being employed in the public sector.

Available data indicate that 27.6 percent of households in our sample municipalities are enrolled in the subsidized health insurance program. Simple comparisons of means provide evidence that the poorest households are not more likely to be enrolled in the program.[11] Indeed, while average per capita income is PHP 16,707 for PHIP members, it is PHP 14,826 for non-members, a difference that is statistically different from zero at less than the 1% level (t-stat: 19). We also plot the density of the per capita income distribution for both PHIP and non-PHIP members (Figure 1), which provides a similar intuition: richer households appear more likely to be enrolled in the program.[12] Compared with the allocation that should prevail, there is significant exclusion error. Among the target population of the 25 percent poorest households in the municipalities, less than a quarter (24.3 percent) are actually enrolled in the program. Inclusion error is also large with more than a quarter (28.8 percent) of the 75 percent richest households in each municipality receiving the subsidy.

Thirty-eight percent of local officials' relatives are PHIP members. The null hypothesis that there is no difference between officials' relatives and the general citizenry is easily rejected ($\chi^2 = 14.1$). Focusing on relatives of 2010 candidates, we find that 23.7 percent of them are enrolled in PHIP. Conversely, 24.3 percent of 2007 candidates' relatives are PHIP members.

The public sector is not prevalent in the sample municipalities, with only 7.9 percent of individuals age 20-60 employed in that sector.[13] Politicians' relatives are more likely

[11]This is confirmed by results from semi-parametric regressions. We estimate, as a partially linear model, equation (2) without controlling for links to elected officials. Results are available in Table A-1 and Figure A-1. There is an increasing relationship between per capita income and the probability of being enrolled in PHIP.

[12]To improve the figure's clarity, we exclude the top 1 percent of households.

[13]This is consistent with findings from other data sources. Indeed, we have access to data from the 2006 Labor Force Survey. Seven of the 8 municipalities in our sample were included in the LFS sample. In those

to be public sector employee than non-connected individuals. Specifically, 14.2 percent of local officials' relatives, 9.7 percent of 2010 candidates' relatives and 10.7 percent of 2007 candidates' relatives are public sector employees. In the three cases, the difference between the general citizenry and politicians' relatives are statistically different from zero at less than the one percent level ($\chi^2 = 33.8$, $\chi^2 = 5.3$, $\chi^2 = 10.7$). Those results reinforce the need to find a suitable control group to understand the value of being related to a politician in office.

5 Results

5.1 Enrollment in the subsidized health insurance program

In this section, we present results on access to PHIP, the subsidized health insurance program. We start by testing whether local officials' relatives are more likely to be PHIP members. Results are available in Panel A of Table 1.

Results from baseline regressions indicate that local officials' relatives are 10.3 percentage point more likely than the average citizen to be enrolled in the program (Columns 1 and 4). Introducing village fixed effects yields quantitatively similar estimates, with the smallest point estimate of 8.9 percentage points (Columns 2 and 5). Those results might simply capture the fact that local officials' relatives are richer and better educated, however. As a result, we include additional control variables in the regressions. The point estimate reduces to 4.1 percentage points after we control for household income and composition as well as for the household head's education levels (Column 3). Given that 27 percent of households are enrolled in the program, this still represents a 15 percent increase in the baseline likelihood of being a PHIP member. Estimating equation (2) through either probit or semi-parametric methods yield qualitatively and quantitatively similar results (Columns 6-8). Except in one regression, the coefficients on the local officials' relatives dummy are statistically different from zero at, at least, the 10 percent level.

We now control for whether the household is related to relatives of candidates in the 2010 local elections. Results are available in Panel B of Table 1. Doing so barely affects the point estimates. Local officials' relatives are about 4 percentage points more likely to be enrolled in the program. As discussed in section 2.3.1, those estimates could still be biased if connected

municipalities, 8.4 percent of individuals are employed in the public sector.

households differ from other households along unobservable characteristics. As a result, we test whether the coefficients on local officials' relatives are greater than the coefficients on 2010 candidates' relatives. Rejecting the null hypothesis would be consistent with the view that local officials engage in nepotistic behavior. Once we include either village dummies or household controls in the linear probability and probit models, we are unable to reject the null hypothesis that the coefficients on local officials relatives are equal to the coefficients on 2010 candidates' relatives, against the one-sided alternative, at the 5 or 10 percent level. Taken together those results indicate that evidence of a causal impact of being related to a local official on the probability of being a PHIP member is much weaker than what one would obtain by comparing connected households to average citizens. This suggests that existing estimates on the positive welfare impacts of connections to politicians might be upward biased.

In addition, we estimate equations (1) and (2) on the sub-sample of households connected to either elected local officials or to candidates in the 2010 local elections. Results are available in Table 2. As above, once we include adequate controls, we are unable to reject the null hypothesis that the coefficients on local officials relatives are equal to the coefficients on 2010 candidates' relatives.

While the respondents were asked specifically about PhilHealth Indigent Program, some of them might have confused the subsidy with the health insurance they might receive from their employer. If that is the case, we could simply be capturing differences in occupational choice between connected and non-connected households. In the sample, public sector employees are the most likely to receive health insurance. To deal with those concerns, we re-estimate equation (3) on the sub-sample of households not working in the public sector. Results, available in Table A-2, are similar to those discussed above.

We now test whether households related to unsuccessful candidates in the 2007 elections suffer from their connections. We estimate equation (4). Results are available in Table 3. we find no evidence that 2007 candidates' relatives are less likely to be enrolled in PHIP than relatives of candidates in the 2010 local elections. Results are similar once we restrict the sample to households connected to candidates in the 2007 or 2010 local elections (Table 4).

5.2 Public sector employment

In this section, we discuss results on occupational choice, focusing on whether or not there is a positive impact to being connected to local politicians on the probability of being employed in the public sector.

Baseline results, available in Panel A of Table 5, indicate that members of local officials' extended families are about 6.4 percentage points more likely to be employed in the public sector (Columns 1 and 4). This effect is large as only 7.9 percent of individuals are employed in the public sector in the sample areas. We obtain similar results once we include village fixed-effects (Columns 2 and 5). It could simply capture the fact that officials come from better off families and that their relatives are better educated and thus better able to secure employment in the public sector. As expected, the size of the effect reduces, to about 3.5 percentage points, after we control for gender, age and education (Columns 3 and 6).

While the above results are consistent with the view that nepotism is a common occurrence in Philippines municipalities, one could argue that, even if their relatives were not currently in office, officials' relatives would still have better information about job openings and, thus, might be better able to secure public sector employment. Again, to control for those alternative explanations, we estimate equation (7) which include controls for whether the individual is related to candidates in the 2010 elections. Results are available in Panel B of Table 5.

The most conservative estimates indicate that local officials' relatives are 3.5 percentage points more likely to be employed in the public sector. We can reject the null hypothesis that the coefficients on local officials relatives are equal to the coefficients on 2010 candidates' relatives, against the one-sided alternative, at the 5 percent level. This is consistent with the argument that local officials provide preferential treatment to their relatives in their hiring decisions.[14] Under this approach, the best estimate of the causal impact of being connected to a local politician in office is a 2.9 percentage point increase in the probability of being employed in the public sector. This represents about 36 percent of the baseline probability of being a public sector employee.

[14] A potential explanation is that their relatives are more productive, possibly because it is easier to monitor them. For example, Barr and Oduro (2002), using data on the Ghanaian manufacturing sector, report results consistent with the view that individuals related to their employers tend to be more productive. However, in our context, if relatives are favored because they are easier to monitor, we should not find a negative effect for individuals related to local opponents.

Restricting the sample to households connected to either local officials or to candidates in the 2010 elections yields slightly less statistically significant but similar results (Table 6). The most conservative estimates suggest that individuals connected with local officials are about 4.5 percentage point more likely than individuals related to 2010 candidates to be employed in the public sector. Given that 11.3 percent of individuals in this sub-sample are public sector employee, this represents about 40 percent of the baseline probability.

In addition, we estimate whether individuals related to unsuccessful candidates in the 2007 elections are less likely to work in the public sector. This would be consistent with the view that local officials find ways to punish their opponents, hereby potentially reducing electoral competition. We do so by comparing relatives of 2007 candidates to relatives of, yet to be announced at time of data collection, 2010 candidates. Findings, available in Table 7, are striking. Once we control for individual characteristics, relatives are 2007 candidates are 1 percentage point less likely to be employed in the public sector. Further, we can reject the hypothesis that the coefficient on the 2007 candidates' relatives dummy is equal to the coefficient on the 2010 candidates' dummy. While not a direct proof that local officials punish their opponents, this provides credible evidence consistent with the argument that they do so.

We also restrict the sample to individuals related to either unsuccessful candidates in the 2007 elections or to candidates in the 2010 elections. Results are available in Table 8. Except in the regressions without any further controls, the coefficients on the dummy is negative. We can only reject the hypothesis that it is different from zero at the 10 percent level (against the two-sided alternative) in the regression with all the control variables estimated through probit. Relatives of unsuccessful candidates in 2007 are 4.1 percentage points less likely to be employed in the public sector than 2010 candidates' relatives. This is a substantial negative effects as it represents a drop of more than 39 percent in the baseline probability of public sector employment in this sub-sample. This is consistent with findings from Venezuela discussed above (Hsieh, Miguel, Ortega, and Rodriguez 2011).

A potential issue with results discussed above would arise if too many of the 2010 candidates were successful in the elections and thus would not constitute a credible control group for unsuccessful candidates in the 2007 elections. As a result, we estimate equation (4) excluding households connected to 2010 candidates that managed to be elected. Results, available

in Table 9, are similar to those obtained previously. This provides further evidence consistent with the argument that local officials have the ability to punish their opponents. An alternative explanation for the result is that they capture households, connected to incumbents in the 2007 elections, who left the public service once their relative lost the elections. As an additional test, among the relatives of 2007 candidates, we separate relatives of candidates who were in office in the period 2004-2007 from relatives of candidates who were not.[15] Results are available in Table 10. We cannot reject the hypothesis that the coefficients on the two set of relatives are equal. We can still reject the hypothesis that the coefficients are equal to the coefficient on 2010 candidates' relatives, however. In addition, this provides evidence that results are not driven by individuals related to previous office holders who left public sector employment when their relatives lost their elected position.

As indicated previously, the proxy used for public sector employment might include some services not provided by the public sector. An alternative interpretation of the results discussed above would be that local officials favor their relatives in the delivery of business permits. To test this hypothesis, we estimate equation (5) on the sub-samples of households connected to local politicians and substitute PSE_{ijk} with a dummy variable which equals one if the individual is engaged in entrepreneurial activities. We find no evidence that local officials' relatives are more likely than 2010 candidates' relatives to be engaged in entrepreneurial activities (Table 11). We obtain similar results when we compare households related to unsuccessful candidates in the 2007 elections with relatives of candidates in the 2010 elections. This provides evidence that results discussed above are unlikely to be driven by the mis-classification of entrepreneurs in the measure of public sector employment.

6 Conclusion

In this paper, using data on more than 46,000 households, we have employed a novel estimation strategy to establish a causal impact of being connected to an elected local official on the likelihood of being employed in the public sector. The effects are fairly strong and are robust to controlling for a number of household and individual characteristics. Most importantly, the positive impacts of political connections do no fade away when we use relatives of candidates

[15] We also separated the two sets of candidates when estimating equation (4). Results were consistent with those obtained previously and we do not report them.

in the 2010 elections as a control group. This is an improvement over existing studies of the role of political connections on household welfare which were only able to uncover correlations between household welfare and political connections.

In addition, we am able to identify some cost of being related to a candidate that ran in the 2007 elections against officials currently in office. The most conservative estimates indicate that relatives of candidates in the 2007 municipal elections are 1.1 percentage points less likely to be employed in the public sector than the average citizen after we control for a number of individual characteristics. Once we compare them to relatives of candidates in the 2010 municipal elections, the negative effect increases to 1.7 percentage points which is equivalent to a more than 20 percent drop of the baseline probability of being a public sector employee. When deciding whether to run candidates need to account for this large negative impact that their relatives will experience. This could explain the low levels of electoral competition in the Philippines, with a large number of candidates running unopposed in the 2010 elections. In the more than 1,600 cities and municipalities that compose the country, about 8% of mayoral candidates and 9% of vice-mayoral candidates ran opposed.[16] Those candidates only need one vote to win, hereby muting electoral competition.

[16] http://www.abs-cbnnews.com/-depth/04/30/10/only-one-vote-win visited on March 18, 2011.

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Table 1: Are officials' relatives more likely to be enrolled in PhilHealth Indigent Program?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Linear Probability				Probit		Semi-Par.
Panel A							
Officials' relative	0.103*	0.089***	0.041*	0.103*	0.098***	0.034	0.064**
	(0.058)	(0.025)	(0.022)	(0.056)	(0.026)	(0.024)	(0.028)
R-squared	0.000	0.256	0.307				0.263
Panel B							
A: Officials' relative	0.103*	0.089***	0.041*	0.103*	0.098***	0.034	0.063**
	(0.058)	(0.025)	(0.022)	(0.056)	(0.026)	(0.024)	(0.028)
B: 2010 candidates' relative	-0.039	0.035*	0.016	-0.039	0.045*	0.002	0.004
	(0.045)	(0.021)	(0.019)	(0.047)	(0.025)	(0.022)	(0.021)
Test $H_0 : A = B$	3.19	2.47	1.22	3.05	1.63	0.67	2.79
$H_a : A > B$ [p-value]	[0.038]	[0.059]	[0.135]	[0.040]	[0.100]	[0.206]	[0.047]
R-squared	0.000	0.256	0.307				0.264
Additional Controls	No	No	Yes	No	No	Yes	Yes
Village Dummies	No	Yes	Yes	No	Yes	Yes	Yes

Notes: Results from OLS (Columns 1-3), Probit (Columns 4-6) and Partially Linear (Column 7) regressions. The dependent variable is a dummy equal to one if the household is enrolled in PhilHealth Indigent Program. The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation (Columns 1-6). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. Observations: 46,301 (Columns 1-6) 46,300 (Column 7). In Columns 3, 6 and 7, we control for household per capita income, household composition and household head's education levels.

Table 2: Are officials' relatives more likely to be enrolled in PhilHealth Indigent Program? (Restricted Sample)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Linear Probability				Probit		Semi-Par
Officials' relative	0.141*	0.013	-0.014	0.141*	0.025	-0.009	-0.147
	(0.080)	(0.074)	(0.072)	(0.081)	(0.084)	(0.086))	(0.291)
Additional Controls	No	No	Yes	No	No	Yes	Yes
Village Dummies	No	Yes	Yes	No	Yes	Yes	Yes
Observations	843	843	843	843	682	682	842
R-squared	0.021	0.395	0.460				0.455

Notes: Results from OLS (Columns 1-3), Probit (Columns 4-6) and Partially Linear (Column 7) regressions. The dependent variable is a dummy equal to one if the household is enrolled in PhilHealth Indigent Program. The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation (Columns 1-6). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. In Columns 3, 6 and 7, we control for household per capita income, household composition and household head's education levels.

Table 3: Are opponents' relatives less likely to be enrolled in PhilHealth Indigent Program?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Linear Probability				Probit		Semi-Par
A: 2007 candidates' relative	-0.035	0.031*	-0.009	-0.035	0.038*	-0.013	0.010
	(0.036)	(0.017)	(0.017)	(0.037)	(0.021)	(0.022)	(0.020)
B: 2010 candidates' relative	-0.040	0.035*	0.003	-0.040	0.046*	0.001	-0.001
	(0.045)	(0.021)	(0.019)	(0.047)	(0.025)	(0.022)	(0.021)
Test $H_0 : A = B$	0.01	0.02	0.20	0.01	0.05	0.21	0.15
$H_a : A < B$ [p-value]	[0.464]	[0.561]	[0.672]	[0.462]	[0.589]	[0.677]	[0.349]
Additional Controls	No	No	Yes	No	No	Yes	Yes
Village Dummies	No	Yes	Yes	No	Yes	Yes	Yes
Observations	46,301	46,301	46,301	46,301	46,301	46,301	46,300
R-squared	0.000	0.256	0.307				0.264

Notes: Results from OLS (Columns 1-3), Probit (Columns 4-6) and Partially Linear (Column 7) regressions. The dependent variable is a dummy equal to one if the household is enrolled in PhilHealth Indigent Program. The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation (Columns 1-6). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. In Columns 3, 6 and 7, we control for household per capita income, household composition and household head's education levels.

Table 4: Are opponents' relatives less likely to be enrolled in PhilHealth Indigent Program? (Restricted Sample)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Linear Probability				Probit		Semi-Par
2007 candidates' relative	0.005	-0.005	-0.017	0.005	0.001	-0.025	0.011
	(0.061)	(0.060)	(0.056)	(0.061)	(0.063)	(0.059)	(0.052)
Additional Controls	No	No	Yes	No	No	Yes	Yes
Village Dummies	No	Yes	Yes	No	Yes	Yes	Yes
Observations	1,172	1,172	1,172	1,172	1,001	1,001	1,171
R-squared	0.000	0.329	0.398				0.376

Notes: Results from OLS (Columns 1-3), Probit (Columns 3-6) and Partially Linear (Column 7) regressions. The dependent variable is a dummy equal to one if the household is enrolled in PhilHealth Indigent Program. The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation (Columns 1-6). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. In Columns 3, 6 and 7, we control for household per capita income, household composition and household head's education levels.

Table 5: Are elected officials' relatives more likely to be employed in the public sector?

	(1)	(2)	(3)	(4)	(5)	(6)
	Linear Probability			Probit		
Panel A						
Officials' relative	0.064***	0.071***	0.046**	0.064***	0.074***	0.035***
	(0.024)	(0.021)	(0.018)	(0.020)	(0.018)	(0.012)
R-squared	0.000	0.053	0.123			
Panel B						
A: Officials' relative	0.064***	0.071***	0.046**	0.064***	0.075***	0.035***
	(0.024)	(0.021)	(0.018)	(0.020)	(0.018)	(0.012)
B: 2010 candidates' relative	0.018	0.027**	0.003	0.019	0.029***	0.006
	(0.013)	(0.011)	(0.008)	(0.013)	(0.011)	(0.007)
Test $H_0 : A = B$	2.84	3.14	4.41	3.34	2.97	3.06
$H_a : A > B$ [p-value]	[0.047]	[0.039]	[0.018]	[0.033]	[0.042]	[0.040]
R-squared	0.000	0.053	0.123			
Additional Controls	No	No	Yes	No	No	Yes
Village Dummies	No	Yes	Yes	No	Yes	Yes

Notes: Results from OLS (Columns 1-3) and Probit (Columns 4-6) regressions. The dependent variable is a dummy equal to one if the individual is employed in the public sector. The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. Observations: 93,985 (Columns 1-4) 93,845 (Columns 5-6). In Columns 3 and 6, we control for gender, age and education levels.

Table 6: Are elected officials' relatives more likely to be employed in the public sector? (Restricted Sample)

	(1)	(2)	(3)	(4)	(5)	(6)
	Linear Probability			Probit		
Officials' relative	0.045*	0.077	0.071	0.045*	0.067*	0.053**
	(0.027)	(0.052)	(0.047)	(0.025)	(0.041)	(0.026)
Additional Controls	No	No	Yes	No	No	Yes
Village Dummies	No	Yes	Yes	No	Yes	Yes
Observations	1,786	1,786	1,786	1,786	1,543	1,505
R-squared	0.005	0.130	0.218			

Notes: Results from OLS (Columns 1-5) and Probit (Columns 6-10) regressions. The dependent variable is a dummy equal to one if the individual is employed in the public sector. The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. In Columns 3 and 6, we control for gender, age and education levels.

Table 7: Are opponents' relatives less likely to be employed in the public sector?

	(1)	(2)	(3)	(4)	(5)	(6)
	Linear Probability			Probit		
A: 2007 candidates' relative	0.031	0.008	-0.013	0.031*	0.006	-0.011**
	(0.019)	(0.010)	(0.009)	(0.018)	(0.008)	(0.005)
B: 2010 candidates' relative	0.019	0.027**	0.003	0.019	0.029***	0.006
	(0.013)	(0.011)	(0.008)	(0.013)	(0.011)	(0.007)
Test $H_0 : A = B$	0.32	1.73	2.20	0.33	2.72	5.42
$H_a : A < B$ [p-value]	[0.714]	[0.095]	[0.070]	[0.717]	[0.049]	[0.009]
Additional Controls	No	No	Yes	No	No	Yes
Village Dummies	No	Yes	Yes	No	Yes	Yes
Observations	93,985	93,985	93,985	93,985	93,845	93,845
R-squared	0.000	0.052	0.123			

Notes: Results from OLS (Columns 1-3) and Probit (Columns 4-6) regressions. The dependent variable is a dummy equal to one if the individual is employed in the public sector. The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. In Columns 3 and 6, we control for gender, age and education levels.

Table 8: Are opponents' relatives less likely to be employed in the public sector? (Restricted Sample)

	(1)	(2)	(3)	(4)	(5)	(6)
	Linear Probability			Probit		
2007 candidates' relative	0.012	-0.040	-0.044	0.012	-0.037	-0.041*
	(0.021)	(0.040)	(0.032)	(0.021)	(0.034)	(0.023)
Additional Controls	No	No	Yes	No	No	Yes
Village Dummies	No	Yes	Yes	No	Yes	Yes
Observations	2,433	2,433	2,433	2,433	1,994	1,994
R-squared	0.000	0.098	0.183			

Notes: Results from OLS (Columns 1-3) and Probit (Columns 4-6) regressions. The dependent variable is a dummy equal to one if the individual is employed in the public sector. The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. In Columns 3 and 6, we control for gender, age and education levels.

Table 9: Are opponents' relatives less likely to be employed in the public sector? (exclude future winners)

	(1)	(2)	(3)	(4)	(5)	(6)
	Linear Probability			Probit		
A: 2007 candidates' relative	0.031	0.008	-0.013	0.031*	0.006	-0.011**
	(0.019)	(0.010)	(0.009)	(0.018)	(0.008)	(0.005)
B: 2010 candidates' relative	0.013	0.025**	0.002	0.013	0.027**	0.006
	(0.013)	(0.011)	(0.008)	(0.013)	(0.012)	(0.007)
Test $H_0 : A = B$	0.71	1.23	1.70	0.73	2.14	4.85
$H_a : A < B$ [p-value]	[0.789]	[0.134]	[0.097]	[0.803]	[0.071]	[0.013]
Additional Controls	No	No	Yes	No	No	Yes
Village Dummies	No	Yes	Yes	No	Yes	Yes
Observations	93,946	93,946	93,946	93,946	93,806	93,806
R-squared	0.000	0.052	0.123			

Notes: Results from OLS (Columns 1-3) and Probit (Columns 4-6) regressions. The dependent variable is a dummy equal to one if the individual is employed in the public sector. The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. In Columns 3 and 6, we control for gender, age and education levels.

Table 10: Are opponents' relatives less likely to be employed in the public sector? Separate previous office holders

	(1)	(2)	(3)	(4)	(5)	(6)
	Linear Probability			Probit		
A: 2007 candidates' relative	0.028	0.004	-0.009	0.029	0.002	-0.009*
Not previously in office	(0.022)	(0.009)	(0.008)	(0.021)	(0.007)	(0.005)
B: 2007 candidates' relative	0.040	0.022	-0.031	0.040	0.019	-0.018
Previously in office	(0.035)	(0.030)	(0.025)	(0.031)	(0.022)	(0.012)
C: 2010 candidates' relative	0.019	0.027**	0.003	0.019	0.029***	0.006
	(0.013)	(0.011)	(0.008)	(0.013)	(0.011)	(0.007)
Test $H_0 : A = B$	0.08	0.34	0.75	0.08	0.58	0.53
	[0.778]	[0.563]	[0.386]	[0.774]	[0.447]	[0.465]
Test $H_0 : A = C$	0.18	3.03	1.27	0.18	4.8	3.99
$H_a : A < C$ [p-value]	[0.663]	[0.042]	[0.131]	[0.665]	[0.020]	[0.022]
Test $H_0 : B = C$	0.30	0.02	1.64	0.33	0.12	2.97
$H_a : B < C$ [p-value]	[0.708]	[0.438]	[0.101]	[0.718]	[0.362]	[0.042]
Additional Controls	No	No	Yes	No	No	Yes
Village Dummies	No	Yes	Yes	No	Yes	Yes
Observations	93,985	93,985	93,985	93,985	93,845	93,845
R-squared	0.000	0.053	0.123			

Notes: Results from OLS (Columns 1-3) and Probit (Columns 4-6) regressions. The dependent variable is a dummy equal to one if the individual is employed in the public sector. The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. In Columns 3 and 6, we control for gender, age and education levels.

Table 11: Local Politicians' relatives and entrepreneurial activities

	(1)	(2)	(3)	(4)	(5)	(6)
	Linear Probability			Probit		
Panel A						
Officials' relatives	0.016	0.002	0.005	0.016	0.003	0.008
	(0.017)	(0.015)	(0.013)	(0.014)	(0.024)	(0.038)
Observations	1,786	1,786	1,786	1,786	779	461
R-squared	0.003	0.098	0.130			
Panel B						
2007 candidates' relative	0.011*	0.013	0.013	0.011*	0.020	0.019
	(0.006)	(0.012)	(0.014)	(0.007)	(0.017)	(0.027)
Observations	2,433	2,433	2,433	2,433	1,309	918
R-squared	0.001	0.057	0.082			
Additional Controls	No	No	Yes	No	No	Yes
Village Dummies	No	Yes	Yes	No	Yes	Yes

Notes: Results from OLS (Columns 1-3) and Probit (Columns 4-6) regressions. The dependent variable is a dummy equal to one if the individual is engaged in 'Entrepreneurial activities'. The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. In Columns 3 and 6, we control for gender, age and education levels.

Table A-1: Who is enrolled in PhilHealth Indigent Program? Semi-parametric results

	(1)	(2)	(3)
HH size	0.049***	0.040***	0.045***
	(0.002)	(0.002)	(0.002)
# Age 0-14	-0.002	-0.002	-0.010***
	(0.003)	(0.003)	(0.003)
# Age 60+	-0.020***	-0.018***	-0.014***
	(0.005)	(0.005)	(0.005)
No Schooling	-0.027***	-0.024**	-0.037***
	(0.010)	(0.010)	(0.010)
Secondary	0.054***	0.050***	0.052***
	(0.006)	(0.006)	(0.006)
College	0.135***	0.136***	0.129***
	(0.008)	(0.008)	(0.008)
HH Welfare	pc income	pae income (1)	pae income (2)
Village dummies	Yes	Yes	Yes
Observations	46,300	46,300	46,300
R-squared	0.262	0.266	0.268

Notes: Results from partially linear regressions. The estimates on the HH welfare measures are displayed in Figure A-1. The dependent variable is a dummy equal to one if the household is enrolled in PhilHealth Indigent Program. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. In column 2, the number of adult equivalent is computed as $(n_a + 0.92n_c)^{0.85}$, where, n_a is the number of adult members and n_c is the number of household members younger than 14. In column 3, it is computed as $1 + 0.7(n_a - 1) + 0.5n_c$.

Table A-2: Are officials' relatives more likely to be enrolled in PhilHealth Indigent Program? (Excluding public sector employees)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Linear Probability			Probit			Semi Par.
A: Officials' relative	0.078	0.065***	0.039*	0.077	0.069***	0.035	0.071**
	(0.062)	(0.022)	(0.021)	(0.060)	(0.023)	(0.022)	(0.030)
B: 2010 candidates' relative	-0.056	0.022	-0.001	-0.056	0.029	-0.003	-0.010
	(0.042)	(0.019)	(0.019)	(0.045)	(0.024)	(0.023)	(0.022)
Test $H_0 : A = B$	2.72	1.91	1.48	2.67	1.09	0.99	4.64
$H_a : A > B$ [p-value]	[0.050]	[0.084]	[0.112]	[0.051]	[0.148]	[0.159]	[0.015]
Additional Controls	No	No	Yes	No	No	Yes	Yes
Village Dummies	No	Yes	Yes	No	Yes	Yes	Yes
Observations	42,154	42,154	42,154	42,154	42,154	42,154	42,153
R-squared	0.000	0.264	0.301				0.267

Notes: Results from OLS (Columns 1-3), Probit (Columns 4-6) and Partially Linear (Column 7) regressions. The dependent variable is a dummy equal to one if the household is enrolled in PhilHealth Indigent Program. The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation (Columns 7-8). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. In Columns 3, 6 and 7, we control for household per capita income, household composition and household head's education levels.

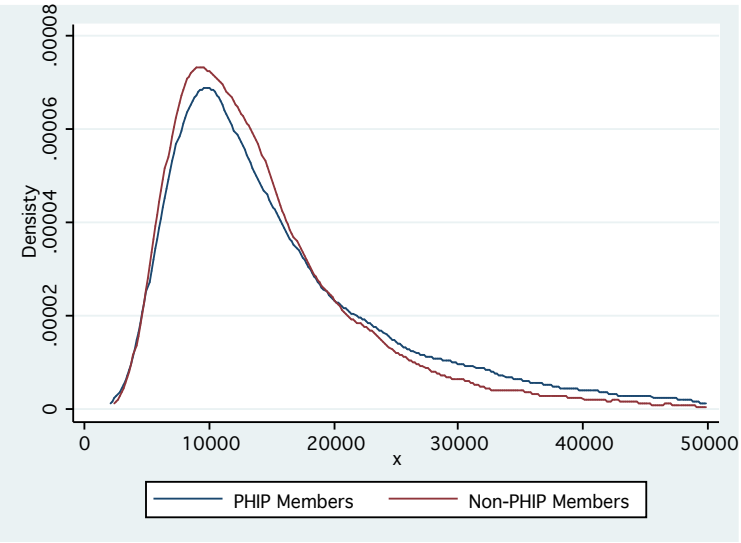

Figure 1: Density of the pc income distribution for PhilHealth and Non-PhilHealth members

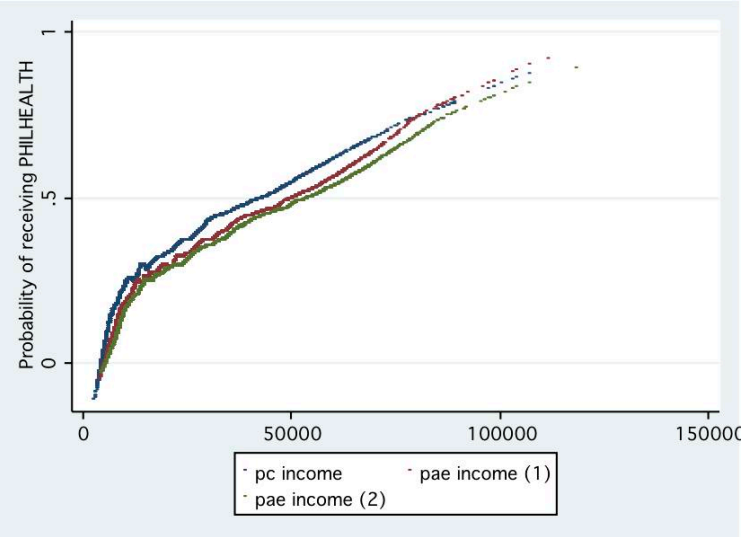

Figure A-1: Partially Linear Model (with controls and village fixed-effects)
Note: The number of adult equivalent are computed as in Table A-1.