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The Local Political Economy Effects of School Construction in Indonesia

by Monica Martinez-Bravo

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Abstract

A by-product of the extension of mass education is the increase in the level of education of those eligible to political o_ces. This can have a profound impact on the e_ectiveness of local governments. In this paper, I examine the e_ects of a large school construction program in Indonesia on local governance and public good provision. The results show that the program led to important increases in the provision of public goods. Furthermore, I provide evidence consistent with the hypothesis that that the increase in the education of the village heads was one of the main mechanisms behind these results.

JEL Codes: D72, H75, O12, P16.
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Monica Martinez-Bravo
CEMFI
mmb@cemfi.es

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1 Introduction

In the last decades, numerous developing countries have experienced unprecedented increases in school enrolment and educational attainment. A large number of studies have documented these changes and have examined the implications for labor force productivity and economic growth (Bills and Klenow (2000), Duflo (2001), among others). However, less is understood about the *local* political economy effects of a rapid increase in the level of educational attainment. One important implication of the extension of mass education is that the level of education of potential candidates to local political offices increases. This, in turn, can lead to changes in the quality of leaders ultimately elected and have a profound impact on the effectiveness of local governance.

In this paper, I investigate the impact of a large school construction program in Indonesia—known as INPRES program—on village governance and public good provision. Between the years 1974 and 1978, 61,000 new schools were constructed, which doubled the existing stock of schools.

A key challenge of this project is to discern whether changes in public good provision are driven by changes in local governance or by the increase in the education of the labor force. To isolate the effects of changes in local governance, I combine the presence of the school construction program with other sources of variation. First, Indonesian adults can join the labor force when they are 16 years old, but they have to wait until they turn 25 in order to contest the village head position. Hence, the effects on public good provision that are driven by the influx educated candidates start taking place considerably later in time. Second, village electoral calendars are not synchronized. Consequently, the timing of the first village-head election to which newly educated cohorts are eligible to run as candidates is staggered across villages. My main identification strategy combines the intensity of the school construction program with the timing of village elections to isolate the effects on public good provision that are driven by changes in local governance.

In order to conduct this research design, I construct a village-level panel dataset of approximately 10,000 villages in the island of Java between the years 1986 and 2003. I create this panel by merging several waves of the Indonesia village census (*Potensi Desa*) which is collected every 3 or 4 years. These data contain a number of measures of public good provision such as number of health facilities, household access to basic services, and number of schools. To my knowledge, this is one of the largest panel datasets, in terms of the number of administrative units, ever used to document the evolution of public good provision in a developing country.

Using these data, I implement a *Differences-in-Differences* strategy that provides esti-

mates of the impact of the first election to which new educated cohorts run as candidates on public good provision. In particular, I compare public good provision in each village before and after the first post-1992 election, and across villages that have already held their post-1992 election to those that had not. The inclusion of village fixed effects controls for any time-invariant unobserved determinant of the level of public good provision, such as geographic characteristics, cultural factors, and the quality of informal institutions. Furthermore, I include year fixed effects which capture changes over time that affect all villages in a similar way. Since the year when the newly educated cohorts join the labor market is the same across villages, the year fixed effects control for part of the secular improvement in the level of education of the labor force.[1]

The main set of results shows that the first election to which new educated cohorts could run as candidates led to important increases in the provision of key public goods: number of doctors, presence of primary health care centers, and access to basic services, such as safe drinking water and garbage disposal. Furthermore, I show that the effects are stronger for villages where the 1970's school construction program was more intense. This is as expected, since those are the villages that should have experienced greater improvements in local governance. The effects are also heterogeneous across villages as a function of villagers demands of public goods: The public goods that improve the most were those in worse condition at baseline.

The main identifying assumption is that the timing of the village elections and its interaction with school construction intensity is quasi-random (i.e., as good as randomly assigned conditional on controls). I provide evidence of the validity of this assumption. In particular, I show that the timing of the election is uncorrelated to pre-existing trends of a large number of covariates and that the results are robust for controlling for the main determinants of the school construction program.

There are different mechanisms through which the influx of educated candidates could have affected local governance and public good provision. In a second part of the paper I explore the empirical relevance of these different mechanisms. A particular channel, which based on the existing literature seems particularly important, corresponds to inducing increases in the education of the village head that is ultimately elected (Evers (2000)). I show that the first election where newly educated cohorts could run as candidates led to a significant increase in the level of education of the village head. Furthermore, I show that this increase was more pronounced in villages that experienced a more intense school construction

[1]By a similar argument, the year fixed effects also control for part of the increase in the education level of the population eligible to vote. The minimum voting age is 17 years old across Indonesia. See section 5 for further discussion.

program in the 1970s. In contrast, I provide evidence that changes in other characteristics of the village head, such as age or gender cannot account for the results. I also show that changes in the education of the electorate do not seem to be an important driver of the results, since controlling for the average education of the electorate in each village does not affect the results.

Under the assumption that the increase in the education of the village head is the main channel behind the estimated increases in public good provision, I compute the implied elasticities. The evidence implies that every extra year of the education of the village head increases the availability of primary health centers, doctors, and safe drinking water by 13%, 11%, and 3%, respectively.

Finally, I explore the channels through which the increase in education of village heads could improve public good provision. I provide suggestive evidence that more educated village heads reallocate the village budget towards development projects. Furthermore, the results suggest that more educated village heads manage development projects more efficiently: Implementing a methodology analogous to Rasul and Rogger (2016), I show that, conditional on the type of project and on the completion rate, more educated village leaders complete projects in shorter time.

This paper relates to a number of different literatures. First, it relates to the literature that has studied the political economy effects of the extension of mass education. This literature has typically focused on its impact on institutional change and democratization (Lipset (1959), Acemoglu et al. (2005), Glaeser, Ponzetto, and Shleifer (2007)) or on political attitudes and democratic values (Milligan et al. (2004), Dee (2004), Campante and Chor (2012), Friedman et al. (2016), Wantchekon, Kasnja, and Novta (2015)). My paper contributes to this literature by focusing on a particular channel through which the expansion of mass education leads to better development outcomes: the improvement on the quality of local governance. Since a large fraction of public goods in developing countries are provided locally, improvements in the quality of local governance have a large potential to improve the well being of the poor. In this aspect, this paper also relates to the nascent literature on the determinants of public sector effectiveness in developing countries (Dal Bó, Finan, and Rossi (2013), Hanna and Wang (2014), Callen et al. (2014), Ashraf, Bandiera, and Lee (2015), Rasul and Rogger (2016)).

Second, this paper is related to the literature that examines the impact of the level of education of politicians on their performance. Besley, Pande, and Rao (2005) find that the level of education of village heads in India is correlated with lower corruption. Besley, Montalvo, and Reynal-Querol (2011) exploit natural deaths in office of heads of state to document that the exogenous removal of a highly educated leader has a negative impact

on economic growth. This paper contributes to this literature by exploiting within-country variation in the education of village leaders originating from a large school construction program. It provides evidence consistent with the education of village heads being the main channel behind identified effects on public good provision.[2]

Third, this paper also relates to the recent literature in empirical political economy that studies the effects of institutional variations on the quality of local leaders. Some examples are Ferraz and Finan (2011), Fisman et al. (2015), Gagliarducci and Nannicini (2013), Brollo et al. (2013), Beath, et al. (2015). These papers use the level of formal education of politicians as a proxy for their quality or level of competence. This paper supports the idea that the education of politicians is a good proxy for competence, since I find that a policy change that increased the education of village heads is associated with significant improvements in the provision of a range of public goods.

Finally, this paper relates to the literature that has studied the determinants of local governance and public good provision in Indonesia. See for instance Evers (2000), Olken (2010), Alatas et al. (2012, 2013). This paper also relates to the set of studies that have analyzed the impact of the INPRES school construction program in Indonesia. See for instance, Duflo (2001, 2004), Breierova and Duflo (2004), Somanathan (2008), Ashraf et al. (2015). This paper contributes to this literature by examining the impact of the program on the quality of local governance, which has been unexplored by the previous literature. Furthermore, this paper uses a novel dataset and empirical strategy.

The rest of the paper proceeds as follows. Section 2 describes the institutional background with special emphasis on the process of provision of public goods. Section 3 describes the data and the empirical strategy. Section 4 presents the main results and robustness checks. Section 5 explores the mechanism behind the main effects and, finally, section 6 concludes.

2 Institutional Background

2.1 Political and Administrative Context

Java is the world's most populous island containing 141 million people (57% of the Indonesian population). The region is divided into four different administrative jurisdictions: provinces,

[2]This paper also relates to a recent paper by Carnes and Lupu (2016). The authors provide evidence that US congressmen and Brazilian mayors with college education do not sponsor more bills and do not engage in more corruption, respectively. The authors obtain these results by comparing highly educated politicians who entered politics after close elections with other politicians. In contrast to their results, the current paper provides evidence that more educated local politicians perform better and deliver more public goods.

districts, sub-districts, and villages, also known as *desa*.[3]

The political context during the period of this study corresponds to the period of General Soeharto rule (1965-1998) also know as New Order. Soeharto's government undertook several institutional reforms with the objective to centralize power and to restrict political opposition. During this period, the country held elections every five years for national and local-level parliaments. However, these elections were highly controlled and Soeharto's party always obtained wide margins of victory.[4]

2.2 Village Level Institutions

Village level institutions experienced a profound transformation during the New Order. When Soeharto took power villages were considerably autonomous units governed by a variety of different traditional customs. While in Java village elections were institutionalized by the Dutch colonial powers, in other islands villages were ruled by hereditary leaders (Antlöv (2003)). Soeharto found this heterogeneity in village governance structures not conductive for his objectives of top-down development and political control. As a result, in 1979 a new law on Village Government was adopted. This law imposed a uniform structure in village institutions across the country. The village government was formed by the village head and the village assembly. These reforms concentrated a high amount of power on the figure of the village head: the village head was *ex-officio* the head of the village assembly and had the right to appoint all of its members; village legislation and the village budget were drafted by the village head in collaboration with the village assembly; the village head also had the right to appoint all members of the village administration. In exchange of this concentration of power in the village level, village leaders had to be loyal to Soeharto's party and to avoid political and social unrest in the villages (Evers (2000), Antlöv (2003, 2004)).

During the Soeharto regime there were few venues of community participation in village-level decisions. Villagers participation was mainly restricted to voting for the village head position every eight years.[5] Candidates for the village head position were pre-screened by upper levels. The fundamental objective of this pre-screening process was to ban entry of

[3]There is another type of village known as *kelurahan* or urban wards that are excluded from the analysis. *Kelurahan* have appointed village heads and do not serve for terms of a fixed amount of years. 11% of the villages in Java are *kelurahan*. For a more detailed description see Martinez-Bravo (2014).

[4]In 1998 Soeharto was forced to step down after losing crucial supports. A transitional government, lead by vice president Habibie implemented a number of reforms that effectively democratized the political system. See Martinez-Bravo (2014) for a more detailed summary of general elections during the New Order and the transitional period.

[5]Village elections were non-partisan elections. All candidates were supposed to be supporters of Soeharto's party. The most voted candidate obtained the village head position. Villagers had to be at least 17 years old to be able to vote in these elections. 17 is the minimum voting age in all elections in Indonesia.

any candidate that could have a communist past or that could be opposed to the Soeharto regime.[6] Nevertheless, among candidates loyal to the Soeharto regime there was intense political competition in many villages and there were reports of candidates spending up to 100 million rupiah (approximately, $25,000) in campaign expenditures (Evers (2000), Antlöv (2003)).

Eligible candidates for the village head position had to be at least 25 years old. Once elected, the village head could serve at most two eight-year terms. Village-level electoral cycles were not synchronized across villages and, hence, villages held elections in different years. Indonesia has a tradition of allowing the local officials to finish their term before calling for new elections or implementing institutional reforms.[7] Therefore, the accumulation of events of early termination of the term of the village head—caused by death or by voluntary resignation—through the history of village elections could generate variation in the election timing. See section 3.2 for further discussion.[8]

2.3 Village-Level Public Good Provision

Village-level public goods are provided by mainly two different mechanisms: they are either funded by the regular village-government budget, or they are funded by the National Development Planning Process (P5D).[9]

Appendix Table 1 shows the village budget of the average village in the year 1996. Most of the village government revenue, 64%, originates from within-village sources.[10] Villages also receive a lump sum transfer from the central government that amounts 6.5 million rupiah

[6]See section 7.1 in the Online Appendix, for a complete list of the requirements that candidates had to fulfill and for additional discussion.

[7]For instance, see Martinez-Bravo et al. (2016) for a study that exploits district-level variation in the timing of appointment of Soeharto mayors.

[8]After the fall of Soeharto, the transitional government passed a law on regional governance (*Law 22 of 1999*). The main change in village governance included in this law was the increase in the separation of powers between the village legislative and the executive body (i.e., the village head). However, a number of subsequent regulations limited the power of the legislative body to exert oversight over the village head. This led to many observers to argue that the village reforms had not represented a clear break from the institutional framework of the Soeharto regime. (See Antlöv (2003)). Other aspects of village-level regulations, such as the electoral rules, minimum age requirements to become a village head, village funding, etc., were unchanged by the new law. Furthermore, the new village law only came into effect once additional regulations were enacted, hence, not affecting village governance until 2002 or later. See section 4.2 for further discussion and robustness checks.

[9]The discussion in this subsection is based on Evers (2000). This report was the result of the Local Level Institutions Study. This study consisted in substantial data collection and case-study analysis conducted in 48 villages in the year 1996. Evers (2000) provides in-depth discussion about public good provision on these villages and how competent village leaders could influence the allocation of public goods.

[10]Revenues from village sources correspond to the sum of *village original income* (revenue from renting out village communal property, e.g. *titisara* land) and *village community income* (income contributed by households in cash, labor or goods to finance village development projects, *swadaya*).

and some smaller amounts from the province and district governments. Village revenues are then allocated to routine expenses or to development expenses. On average, 61% of total revenue is allocated to development projects, mainly to fund transportation facilities, social facilities, and general infrastructure expenses.

One potential way through which the village government could affect the level of public good provision is through the reallocation of expenditures of the village budget. For instance, by spending less on administration and investing more on development projects.[11]

Public good provision could also be affected through a better management of development projects. During the period that corresponds to this study, most development projects were managed by the village, even when funded by upper levels of government.[12] Funds from upper-level grants are typically used to buy materials necessary for the project while labor is provided by villagers. The tasks of coordinating labor and raising any necessary additional cash contributions from villagers are typically undertaken by the village government. A better management of the development projects that is associated to lower rent seeking, might attract more contributions from villagers and, hence, lead to better public services.

An alternative venue through which villages could increase public good provision is by applying to additional funds from the National Development Planning Process. Proposals are collected at the subdistrict level and then sent to the relevant sectoral agency at the district level for evaluation. According to Evers (2000), many village governments were not aware of these calls for proposals and, when aware, they submitted poor quality proposals not backed by proper cost estimates. More effective village governments might be better informed about these funding opportunities and may produce project proposals of higher quality.[13]

[11] Although the size of the village budget is not large, villages could still afford to finance relevant development projects. The total village budget revenue is about 40 million rupiah, which corresponds to $26,000 current USD-PPP adjusted. According to Evers (2000), a full school renovation costed 18 million rupiah in 1996. Hence, if a village were to spend all its development budget (25.5 million rupiah) in school renovations, it could fully renovate 1.4 schools every year.

[12] The data used in this project reveals that in 1986, 80.5% of all development projects undertaken in villages were managed by villagers themselves. See section 5.4 for further discussion.

[13] In this paper, I examine the effects of a range of public goods. These public goods will be described in more detail in the next section. Public goods differ on their main source of funding. Some public goods, such as primary care health centers or doctors, are likely to be funded predominantly through the P5D program. Other public goods could be funded by the P5D program but also by village sources: for instance, improving access to safe drinking water typically requires conducting some small-scale infrastructure projects that villages regularly undertake and fund. For larger infrastructure requirements, villages would be required to obtain funds from the P5D program. Finally, other projects are funded and managed exclusively by the village government: that is the case of the system for garbage bin disposal or the presence of health posts in the villages. Unfortunately, there is no data on the particular funding source for each public good present in the villages.

2.4 The Sekolah Dasar INPRES Program

In the mid 1970s Indonesia undertook one of the fastest school construction programs in the world to that date (World Bank, (1990)). Between the years 1974 and 1978, the stock of schools in Indonesia doubled: over 61,000 new primary schools were constructed. This corresponded to one new school per 500 children of ages 5 to 14 in 1971 (Duflo (2001, 2004)). The program was named Sekolah Dasar INPRES program and it was funded by oil revenues.

The INPRES program was designed by the central government. A number of presidential regulations were enacted, which stipulated the number of schools to be constructed in each district. This number was approximately proportional to the number of school-aged children not enrolled in school prior to the program.[14] Each of the new schools was designed to host 3 teachers and 120 pupils of primary school age. In particular, children between the ages of 7 and 12 could attend these new schools.

3 Data and Empirical Specifications

3.1 Data

The main data source used in this project corresponds to different waves of the Indonesian Village Census (*Potensi Desa*). These data are collected by the Central Bureau of Statistics (*Badan Pusat Statistik*) every 3 or 4 years. The data comprise a large number of measures of public goods provided in the village, such as health and educational facilities, and a host of village characteristics.[15] Each year, the village census has a different focus (agriculture, economy, or population) and, as a result, several variables are not consistently reported in all waves of the village census. In this paper I focus on public good outcomes that are consistently reported across the different waves of the survey.

To construct the dataset I merge 6 different waves of the village census collected between the years 1986 and 2003.[16] The Central Bureau of Statistics does not keep consistent village identifiers across waves and, as a result, the merge has to be implemented based on village names. Given the difficulty in merging datasets with a large number of observations, I focus on the most populous island of Indonesia: Java. The final dataset contains information on

[14] Duflo (2001) shows that a 10% increase in the number of children not enrolled in school is associated with a 7.8% increase in the number of schools. Hence, the program was less redistributive than intended.

[15] Survey enumerators collect the answers from members of the village administration and also check village administrative records. The measures of public good provision are typically easily verifiable in the village (e.g. number of schools or number of health facilities). Hence, this survey provides a quite accurate representation of the level of public goods in these villages.

[16] In particular, the waves correspond to the years 1986, 1990, 1993, 1996, 2000, and 2003.

a balanced sample of 9,855 villages comprised in 4 provinces and 82 districts.[17]

From the year 1986, the village census incorporates information on basic characteristics of the village head such as the highest level of education achieved, age, and gender. Based on the information on the education level of village heads, I construct a measure of the number of years of schooling of the village head in office at each point in time. The 1993 village census also reports the length of tenure of the incumbent village head. I use this variable to derive the predicted electoral calendar of each village. In particular, I predict the timing of the upcoming election by using the date when the the term of the incumbent village head in 1993 is scheduled to expire. Using the timing of the expected election rather than the date of the actual election can reduce the power of the empirical test. However, an important advantage of this approach is that the expected timing is a more exogenous regressors than the actual election timing, since the predicted election timing is less likely to be correlated with contemporaneous village-level shocks.[18,19]

Table 1 presents some descriptive statistics. The average village on the baseline sample comprises 3,195 people. Most of the inhabitants of these villages, 63%, are employed in the agricultural sector and only 10% of the villages are classified as urban, according to the Bureau of Statistics. Hence, the average village in the sample corresponds to a considerably rural village. The data also contain the number of INPRES schools constructed in each village. The average number of INPRES schools per village is 0.89, hence confirming the large magnitude of the school construction program.[20,21] The next set of rows contains descriptive statistics of some of the outcomes of interest. About 10% of the villages have a primary care

[17]The provinces included in the study are West Java, Central Java, East Java, Jakarta, and Yogjakarta. The province of Jakarta is not included in the sample because all of its villages are urban wards (i.e., *kelurahan*) and, hence, have a different governance structure. For more details on construction of the data see section 7.2 in the Online Appendix.

[18]The only exception corresponds to villages where the village head reports having been in office for 0 or 1 year. In this case I have to use the actual timing of the previous election. See the data appendix (section 7.2) for more information on how this variable is constructed. The results are robust to using the actual election timing rather than the predicted election timing. See section 4.2 for more details.

[19]Note that while these data are rich in the number of jurisdictions covered, they contain limited information about elections for the village head position. In particular, there is no information on turnout, vote shares, or on the number or characteristics of competing candidates.

[20]In particular, one INPRES school was constructed in 4,566 villages, two INPRES schools were constructed in 2,122 villages, and no INPRES schools were constructed in 3,167 villages. See Appendix Figure 1 in the Online Appendix for the distribution of schools by province.

[21]The previous studies that analysed the impact of the INPRES program have used data collected for the project Duflo (2001). These data correspond to administrative data from the INPRES program that listed the number of schools to be constructed in each district. In contrast, the data I use in this project contains village-level information on the number of schools that were actually constructed in each village. The measures on the number of INPRES schools from the two sources are closely related. The main advantage of the data I use in this project is that it is provided at the village level, hence a much finer geographical area (on average, each district contains 383 villages).

health center in the village and 12% of the villages have a formally trained doctor working in the village. 77% of villages have access to safe drinking water (tap water or access to a pump). 91% of villages have access to some garbage disposal system (disposal using bins or hole). Finally, each village has on average 3.13 primary schools and there is one high school for every 2.5 villages. The last set of statistics in Table 1 provide a description of the characteristics of village heads. The average number of years of education of the village head is 9.9. This level of educational attainment is comparable to the one of the general population: According to Duflo (2001), the average years of schooling in her sample of individuals was 7.98, while it was 9 for the sample of wage earners. The average age of village heads is 43 years old, while the median is 42. The age distribution is right-skewed because there are a few villages where the village head is quite old. A considerable fraction of village heads, 25% of them, are younger than 36 years old. Finally, the table shows that the large majority of village heads—97% of them—are male.

3.2 Main Empirical Specifications

Figure 1 presents the timeline of events, which is useful to illustrate the main empirical strategy. The INPRES school construction program began in 1974. Children start primary school in Indonesia at the age of 7. Hence, the first cohort that could have completed primary school in the INPRES schools corresponds to children that were 7 years old in 1974. Nine years later, in 1983, this first treated cohort turned 16 and joined the labor market. From that point on, every year one more educated cohort joined the labor market. In 1992, individuals that belonged to the first treated cohort turned 25 and became eligible candidates for the village head position. However, since village-head elections were held in different years, the newly educated cohorts were not able to run for elections until the following village-head election. Village elections take place every 8 years. A village that held a village head election in 1991 had to wait until 2000 for the INPRES program to affect outcomes through the village government office. In contrast, a village that held a village-head election in 1992 could have experienced the effects of the INPRES program through changes in village governance from that point in time.

In other words, the year of the first election post-1992 captures the time when cohorts fully educated in the INPRES schools could start running for office. Hence, that is the year when the effects driven by improvements in the village governance could start taking place. The shaded area of Figure 1 highlights those years for different villages. See Appendix Table

2 for descriptive statistics on the timing of village elections.[22,23]

Before presenting the main empirical specification, I examine whether the timing of the increase in public good provision corresponds to the timing of the first election post-1992. With this objective, I divide villages in three groups depending on when they held the village election. Since the data is available every three years, the empirical predictions for all villages within the same group is the same: we expect to start observing effects in 1993 for villages that held elections in 1992-1993, while we expect to see effects from 1996 and 2000 for villages that held elections in 1994-1996, and 1997-2000, respectively.[24]

For each group of villages, I examine how the intensity of the INPRES school construction program is associated with public good provision. In order to examine the timing of the effect, I allow the effect to differ by calendar year. More specifically, I estimate the following model:

$$y_{vt} = \beta_0 + \sum_{s=1990}^{2003} (\delta_s \times Num\_INPRES_v)\gamma_s + \alpha_v + \delta_t + \varepsilon_{vt} \quad (1)$$

where y_{vt} is the outcome of interest measured in village v and year t, δ_s is a dummy that takes value 1 in year s, Num_INPRES_v is the number of INPRES schools constructed in village v, and α_v are village fixed effects.

Figure 2 plots the coefficients γ_s for the different sets of villages and for the main outcomes of the paper.[25] The vertical red line denotes when we expect the coefficients to become positive.

Figures 2a to 2c examine the effects on the education of the village head. Figure 2a indicates that there is no effect for villages that held elections shortly after 1992. The fact that very few of the cohorts educated in the INPRES schools could contest the election by that time could explain the lack of effects. However, for most villages—those holding elections

[22]Java has a long history of village elections: village heads have been elected by popular vote during the last 150 years (Antlöv (2000)). The variation in the timing of elections across villages is driven by the accumulation of early terminations of the 8 year terms of village heads throughout the history of village elections. Early terminations of village heads' term can be caused by death or by voluntary resignation of the village head. The implementation of the village law of 1979 did not reset the electoral calendar of villages. The incumbent village head was allowed to finish his 8 year term, before calling the following election. These historical events explain the variation in electoral timing across villages and support the assumption of the quasi-randomness of the village electoral timing.

[23]Note that it is theoretically plausible that cohorts partially educated in the INPRES schools—i.e., for a few years of their primary school education—contested the village elections earlier than 1992. However, there is no empirical evidence that they had an effect on outcomes. In particular, the level of education of the village head only starts being related to the intensity of the INPRES program after the first election post-1992. See section 5.1 for further discussion.

[24]The data is available for the following years: 1986, 1990, 1993, 1996, 2000, and 2003.

[25]Appendix Figure 2 presents the results for some additional outcomes. See section 4 for further details on each of the dependent variables.

between 1994 and 2000—the timing of the increase in village head education coincides with the post-1992 electoral cycle. Figures 2b and 2c show that the level of education of the village head starts being related to the intensity of the INPRES program after the first election post-1992, while there is no effect before that year.

The following rows of Figure 2 implement the same specification when examining a number of measures of public good provision: presence of primary health centers, presence of doctors and access to safe drinking water. In general, the results do not show a significant pre-trend, and the main increase in public good provision starts being related to the intensity of the INPRES school construction program after the year of the first election post-1992.[26]

These results suggest that once cohorts educated in the INPRES schools start contesting elections we observe increases in both the education of village heads and in public good provision. In order to assess the magnitude of the overall effect, I estimate the following empirical specification in the whole sample of villages:

$$y_{vt} = \lambda_0 + \lambda_1 postel92_{vt} + \lambda_2 postel92_{vt} \times Num\_INPRES_v + \alpha_v + \delta_t + \varepsilon_{vt} \qquad (2)$$

where y_{vt} corresponds to the outcome of interest in village v and year t. The variable $postel92_{vt}$ is a dummy that takes value 1 for all the periods t after village v has held its first village-head election after 1992.[27] The next regressor is the interaction betweeen the $postel92_{vt}$ dummy and a measure of intensity of the INPRES school construction program (e.g. the number of INPRES schools constructed in the village). Finally, α_v are village fixed effects and δ_t are years fixed effects.[28] I present robust standard errors, clustered at the district level.

A number of features of this specification are worth highlighting. First, since there is

[26]While occasionally, the year before the predicted treatment experiences an increase in the coefficients, such as in 2f, in other cases it experiences a decline, such as in 2k. Appendix Tables 3.A. and 3.B. show the regression estimates that correspond to these figures. The last row of the table reports the p-value of the joint significance test of all the pre-treatment point estimates. As we can see, out of 18 regressions only two of these tests are statistically significant at the 10% level, which could have been generated by chance.

[27]$postel92_{vt}$ is constructed using the reported length of tenure of the village head in office in the year 1993. It is important to note that, with the exception of villages that report a length of tenure lower than one year in office, I use the next predicted election to construct the measure of the electoral timing. Using the scheduled election instead of the actual election decreases the power of the empirical strategy, but increases our confidence on the quasi-randomness assumption of the election timing. The results are robust to using the actual election timing rather than the predicted election timing. See section 4.2 for further details.

[28]Note that the INPRES intensity measure is defined in absolute values, while in Duflo (2001) it was defined as number of schools per 1,000 children. Unfortunately, at the village level there are no measures of school-aged children prior to the school construction program. However, villages in Indonesia are much more comparable to one another than districts. Hence, the number of schools INPRES schools at the village level is a reasonable proxy for program intensity. See section 4.2 for robustness checks controlling for village population.

variation across villages in the timing of the first election post-1992, the $postel92_{vt}$ variable is not collinear with the year fixed effects. Controlling for year fixed effects is particularly helpful because the year when the first educated cohort joins the labor market is the same for all villages. Hence, the year fixed effects capture part of the secular improvement in the education of the labor force, as well as any other factors that change over time and that affect all villages similarly.

This reduced form specification is equivalent to a *Differences-in-Differences* strategy where I compare the change in public good provision in each village before and after the first post-1992 election is held and across villages that have already held their post-1992 elections to those that had not yet held their post-1992 election. Furthermore, I explore whether this effect is stronger for villages that experienced a higher intensity of school construction.

I expect to obtain estimates such that $\hat{\lambda}_2 > 0$: villages that had a more intense INPRES school construction program in the 1970s should experience greater improvements in public good provision after the first election post-1992. Those are the villages where I expect the local governance to improve the most, given the larger influx of educated cohorts contesting the village elections.

The identifying assumption requires that the timing of the first village-election post-1992 and its interaction with the program intensity are quasi-random (i.e., as good as randomly assigned, conditional on the controls).[29] In Table 2, I test the validity of quasi-exogeneity of the election timing. In particular, I document that the election timing is uncorrelated to a large number of pre-existing trends in village-level characteristics.[30] Each row of Table 2 shows the results of a different cross-sectional regression where the dependent variable is the year of the first election post-1992 and the independent variable is the percentage change of the corresponding regressor between the years 1986 and 1990. Columns 1 and 2 show the point estimates and the standard errors, while columns 3 and 4 show the standardized (beta) coefficients and their corresponding standard errors. Out of 50 pairwise correlations performed, only 3 of them are statistically significant at the 10% level or lower. These represent a 6% of the regressions performed, which is consistent with the quasi-randomness assumption of the election timing.[31]

[29]Since the treatment is staggered in time, the quasi-randomness assumption is equivalent to the "parallel trends assumption" of standard *Differences-in-Differences* specifications.

[30]Note that, since the main specification controls for village fixed effects, the identification assumption does not require that the timing of elections is independent of time-invariant village characteristics. Instead, it requires the weaker assumption that the timing of elections is independent of underlying trends in village-characteristics. For instance, if villages with a low growth rate of public services in the village tend to hold election sooner in the 1992 to 2000 time window, this would invalidate the identification assumption.

[31]The covariates that are predictive of the timing of elections are the availability of a horse-drawn cart in the village, the presence of village cooperatives, and the number of banks. The full set of pairwise correlations are available from the author upon request.

The exogeneity of the election timing also supports the hypothesis that the interaction of the election timing and the intensity of the INPRES school construction program is quasi-random. Note that the village fixed effects control for the main effect of the intensity of the INPRES program and by all time-invariant determinants of the allocation of the program. A remaining concern is the potential presence of omitted time-varying village-specific factors that correlate to the presence of the INPRES program and co-vary with the electoral cycle. In order to address this concern, I follow a similar approach to the one presented in Duflo (2001) and show that the results are robust to controlling flexibly by the initial level of school enrolment, which was the main determinant of the allocation of INPRES schools. Similarly, I show that the results are robust to controlling for the presence of other development programs implemented during the sample period. See section 4.2 for further discussion.

In a second empirical specification, I explore the presence of heterogeneous effects across villages. More specifically, I study whether public goods, whose provision was most deficient at baseline, experience the largest increases after the first election post-1992. Increases in the quality of local government may not lead to increases in all types of public goods. It is likely that the largest increases are concentrated in those public goods that were in worse condition at baseline and, hence, more highly demanded by villagers. To capture this, I estimate the following specification

$$\begin{aligned} y_{vt} &= \beta_0 + \beta_1 postel92_{vt} + \\ &+ \beta_2 postel92_{vt} \times BadService_v + \\ &+ \beta_3 postel92_{vt} \times Num\_INPRES_v + \\ &+ \beta_4 postel92_{vt} \times BadService_v \times Num\_INPRES_v + \\ &+ \alpha_v + \delta_t + \varepsilon_{vt} \end{aligned} \tag{3}$$

where $BadService_v$ is a dummy that takes value 1 if public good y was underprovided at baseline (year 1986). The rest of variables are defined as in (2).[32]

I expect to obtain estimates such that $\hat{\beta}_2 > 0$ and $\hat{\beta}_4 > 0$: villages with a greater demand of a particular public good should increase their provision after the first election post-1992, and to a greater extent for villages that experienced a more intense INPRES construction program.

[32]When the variable is not available for the year 1986, I measure it using the year 1990. See the Data Appendix in section 7.2 for a detailed definition of measures of public good provision, and for measures of under-provision of public goods at baseline.

4 Results

4.1 Main Results: The Effect of the School Construction program on Public Goods Provision

Next, I present the results of estimating equation (2) when the dependent variables correspond to different types of public goods. In the main text of the paper, I present the results for the availability of primary health care facilities in the village, presence of doctors and access to safe drinking water. These three are important public goods that have a substantial impact on the health and well-being of poor villagers. Because of space constraints I relegate to the appendix the results on number of health posts and the availability of a garbage disposal system in the village.[33]

I explore three different measures of intensity of the INPRES school construction program: The number of INPRES schools (in deviations from its sample mean), a dummy that takes value one if at least one INPRES school was constructed, and two dummies for whether one or two INPRES schools were constructed.[34]

Each panel of Table 3 presents the results for one of the three main public good outcomes. As we can see, the interaction coefficients of the post dummy and the measures of INPRES intensity are positive and statistically significant. Furthermore, the evidence presented in column 4 suggests that the magnitude of the effects monotonically increases with the number of INPRES schools constructed in the village. This evidence suggests that, after the first election post-1992, public good provision specially increased in villages that experienced a more intense INPRES school construction. This is consistent with the hypothesis that those villages experimented larger improvements in local governance due to the influx of more educated individuals contesting village elections.

The results indicate that every additional INPRES school constructed in the village leads to an additional increase in public good provision after the first election post-1992 of 1.2 percentage points for primary health centers, 0.6 percentage points for doctors, and 1.8 percentage points access to safe water. To assess the magnitude of these effects, I compute the change in public good provision after the first election post-1992 for villages with one

[33]Health posts correspond to *posyandu*, small community-based healthcare facilities that are responsible for family planning and maternal and child care. Primary care health centers correspond to *puskesmas* and *polyclinics*, where formally trained doctors and nurses provide basic medical services. I focus on these measures of public goods because they are consistently reported across waves of the village census and because the village government is involved in its provision. See section 4.2 for further discussion and for additional results.

[34]When the intensity measure is the number of INPRES in deviations from its sample mean, the coefficient λ_1 of specification (2) is interpreted as the effect of the post dummy for villages with an average number of INPRES schools constructed. The interpretation of the interaction coefficient λ_2 remains the same.

INPRES school over the sample mean. The results suggest that these villages experience increases in these public goods of 7%, 6%, and 1.7%, respectively, over the corresponding sample mean.[35,36]

Appendix Table 4 in the Online Appendix, presents the results on number of health posts in the village and access to garbage disposal system. The results suggest that, villages with one INPRES school over the sample mean, experience an increase in health posts of 0.7% and in access to garbage disposal of 1.8%, over their corresponding sample mean. However, these effects are less precisely estimated.

Table 4 explores whether these results are heterogenous across villages based on villagers' demands. It is unlikely that an improvement in local governance leads to increases in all types of public goods. Instead, I expect better quality of governance to improve those public goods that were in worse condition at baseline. To investigate this possibility I estimate specification (3).[37] I present the results for two measures of INPRES program intensity: the demeaned number of schools constructed (columns 1, 3, and 5) and the dummy for any INPRES school constructed (columns 2, 4, and 6).

The last row of coefficients presents the estimates on the triple interaction between the post dummy, the measure of the INPRES program intensity, and the dummy for bad service of the corresponding public good at baseline. As the table shows, the coefficients associated to this triple interaction are positive and statistically significant. The results are similar for both measures of INPRES program intensity.

These estimates suggest the above described effects are stronger for villages with bad quality of service at baseline. In those villages, the additional increase in public good provision per extra INPRES school constructed is of 1.4 percentage points in the likelihood of having a primary health care center, of 0.7 percentage point in the likelihood of having a

[35]These estimates are obtained by adding the coefficients on the post-1992 election dummy and its interaction with the number of INPRES schools constructed, and then dividing by the corresponding sample mean.

[36]Note that, all specifications include year fixed effects. Hence, the negative coefficients on the uninteracted post-1992 election dummy indicate that, in villages with no INPRES schools, public good provision increased less than for the average village. Villages did not experience actual declines of the availability of public services over the sample period.

[37]I define villages with bad quality of service as follows: in columns 1 and 2, villages with no primary care facilities in 1986; in columns 3 and 4, villages with no doctors in 1986; in columns 5 and 6 villages with no access to purified drinking water in 1986. See section 7.2 in the Online Appendix for further details.

doctor, and 5.7 percentage points in the likelihood of access to safe drinking water.[38,39]

Overall, these results suggest that villages with higher intensity of the INPRES program became more sensitive to underlying village needs in the provision of public goods. This is again consistent with the hypothesis that those villages experienced greater increases in the quality of village governance and, as a result, public good provision became more responsive to the needs of villagers.

4.2 Robustness Checks

In this section, I explore whether the results are robust to alternative econometric specifications and different sets of controls. Table 5 displays the robustness checks on the results presented in Table 3. Column 1 includes controls for log population. The results are highly robust, hence, reducing the concern that different sizes of villages could affect the results. Column 2 incorporates as controls the pre-treatment value of the corresponding dependent variable interacted with year fixed effects. The results are robust to this alternative specification. This mitigates the concern that villages with different levels of pre-treatment public goods had differential underlying trends of public good provision. In other words, it mitigates the concern of mean reversion. Column 3 adds as controls the pre-treatment value of the 3 covariates that were correlated to the timing elections (reported in Table 2), interacted with year fixed effects. The results are robust to this specification as well. Following the specifications in Duflo (2001), column 4 includes as controls the primary school enrolment rate in 1986—the first year of our sample—interacted with year fixed effects. Furthermore, I include an indicator of whether the village received a water and sanitation program, that was contemporaneous to the INPRES school construction program, interacted with year fixed effects. This mitigates the concern that the results are driven by time-varying factors that correlate to the determinants of the program.

Column 5 explores the presence of pre-trends in the interaction of the post dummy with the INPRES program intensity. Note that the absence of pre-trends was graphically

[38]Note that for some public goods, the coefficient of uninteracted $postel92_{vpt}$ dummy is negative and statistically significant. This suggests that villages with low demand of a specific public good and no school construction experienced a lower expansion of that public good, relative to villages where there was high demand. There is not an *a priori* prediction of the sign of the unineteracted coefficient: it is possible that entrepreneurial village heads transferred resources from public goods in low demand to public goods in high demand within a village, consequently, leading to lower growth of public goods that were not in high demand.

[39]Appendix Table 5 in the Online Appendix shows the results for number of health posts and system of garbage disposal. While the triple interaction is not statistically significant, adding the four coefficients lead to positive effects, suggesting that villages with poor quality of service and high number of INPRES schools experienced an increase in public good provision. These results predict an increase of 1.9 in the number of health posts and an increase of 34 percentage points in the availability of a garbage disposal system, for villages with bad service at baseline and at least one INPRES school.

examined in Figure 2. To further investigate the presence of pre-trends, I display in Table 5 the interaction terms of the measure of INPRES intensity with dummies for one, two, and three years before the first election post-1992. The omitted category corresponds to the interaction with four or more years before this election. The results show that the interaction of the post dummy and INPRES intensity is highly robust to the inclusion of flexible controls in the pre-trend. Furthermore, the coefficients of the pre-treatment interactions are, in general, small and insignificant: The last row of each panel shows that the pre-treatment interactions are jointly insignificant.[40]

Another set of concerns relates to the presence of other development programs during the 1990s and 2000s. If the determinants of the allocation of the INPRES program in the 1970s correlate with the demands or needs for national-development programs three decades later, the effects could be mistakenly attributed to the improvement in local governance. Note that the empirical design mitigates, to some extent, this concern. First, the inclusion of village fixed effects controls for all time-invariant determinants of the allocation of development programs. Second, the main identifying variation combines the INPRES program with the village-specific electoral calendar. Since the timing of implementation of national-level programs is unlikely to be related to the village electoral calendar, the year fixed effects would control by the potential implementation of these programs.

A related concern, consists on the differential impact of the 1998 East Asian financial crises across villages. If villages that obtained the INPRES schools were also more negatively affected by the crises, the crisis-mitigation spending might be a confounding factor.

Appendix Table 8 investigates these concerns. The first column for each outcome shows that the results are robust to flexibly controlling by the incidence of the economic crises. I follow the work of Chen (2010) and use the fraction agricultural land in the village devoted to rice cultivation—denoted as wetland—as a proxy for low impact of the economic crises. This exploits the fact that, during the crises, the price of rice increased much more relative to the price of other food items. The results are unaffected by these controls. The second to the fifth column for each outcome incorporate controls for the IDT program and by a number of crises-mitigation programs.[41] The results are also unaffected. Finally, the last

[40] Appendix Table 6 in the Online Appendix provides the robustness checks for the number of health posts and the garbage disposal system. The point estimates and the standard errors are robust to the different specifications, with the only exception of controlling for pre-treatment values of the dependent variable, which increases the magnitude of the effect for number of health posts, and reduces it for garbage disposal system. These outcomes show no pre-trends in the interactions with the INPRES school intensity. Finally, Appendix Tables 7A and 7B show the robustness checks of the heterogenous results presented in Table 4. The results of this alternative specification are also highly robust to the addition of the previously discussed controls.

[41] The Inpres Desa Tertinghal (IDT) program was implemented in the late 1990s and consisted on a village-level credit rotating fund that was targeted to poor individuals in the village. The IDT program is considered

column for each outcome incorporates all the above controls in a single specification. As we can see, the results are highly robust. Overall, the empirical evidence does not support that the results presented in this paper are driven by the implementation of other national-level programs or by the differential impact of the economic crises.

The results are also robust to excluding from the sample villages that held elections during the late 1990s. As discussed in section 2, during those years Indonesia experienced important political and economic changes. A potential concern is that, during that period, village elections were different in nature, and this could have lead to particular dynamics that affected public good provision. Appendix Table 9 presents the results. The first column of each outcome presents the baseline results for comparison. The second and third column of each outcome drops from the sample villages that had elections in 1999-2000 and 1998-2000, respectively. The results are fully robust to the first sample restriction. The results decrease in statistical significance when implementing the second sample restriction. However, this is not surprising since this restriction eliminates 67% of the sample. Still the point estimates are similar to the baseline results.[42]

5 Mechanisms

The results presented so far indicate that the first village election to which INPRES-educated cohorts can run as candidates leads to increases in public good provision. These increases are larger for those goods demanded by villagers and in areas where the INPRES school construction program was more intense. There are a number of different channels through which

as one of the precursors of the Kecamatan Development Program (KDP). See Guggenheim et al. (2004).

[42]I also present in the Online Appendix some additional robustness checks. Appendix Table 10 shows that the results are robust to adding as controls the covariates presented in Table 5 interacted with the post dummy, instead of interacted with the year fixed effects. Appendix Tables 11.A and 11.B show that the results are robust to defining the post-1992 election dummy according to the actual election timing, rather than the predicted election timing. Appendix Table 12 shows the results for other outcomes available in the village census for the six waves corresponding to the sample period. First, I examine the effects for presence of irrigation, usage of electricity or kerosene for cooking, and availability of motor transportation in the village. The results suggest there are no effects on these variables. While the measures could be affected by the actions of the village government, they could also be provided by the private sector or crowded out by private sector goods—such in the case of toilets. Hence, it is unclear that the village government is fully accountable for the presence of these goods in the village. The next set of columns examine education facilities, such as number of kindergarten, number of primary schools, and number of high schools. The results suggest that there is an increase in the availability of these goods after the 1992 election, in particular in those villages with more INPRES schools. However, I do not focus on these outcomes because the construction of INPRES in the 1970s could have affected the demands for school facilities in the following decades. Column 8 reports the effects on the presence of asphalt roads. The results indicate that there is not a significant effect. Finally, note that all results of the paper are robust to adding a full set of province fixed effects interacted with year fixed effects as controls. The results are available from the author upon request.

the influx of educated candidates could have led to changes in public good provision. A first set of mechanisms relate to the characteristics of the village head that is ultimately elected in those elections. A particular characteristic, which based on the Indonesian literature seems specially important, is the level of education of the village head: A more educated village head, could be more entrepreneurial and start more development projects. It is also likely that additional years of schooling enhance the cognitive skills of village heads, allowing them to implement development projects more effectively. In the next subsections, I explore the effects of the school construction program on village head characteristics. I also investigate the relevance of alternative channels, such as village-specific changes in the level of education of the electorate.

5.1 The Effect of School Construction on Village Head Characteristics

Education of the Village Head

The results in Table 6 explore whether the first election to which newly educated cohorts can contest the election leads to more educated village heads being elected. I estimate variations of equation (2) when the dependent variable is the number of years of education of the village head. Column 1 shows that the first election post-1992 leads to an increase of 0.43 in the years of education of the village head. This effect represents a 4.4% increase over the sample mean.

Columns 2 to 4 show that the increase was higher in villages where the INPRES program was more intense. For instance, column 4 suggests that villages that had the highest intensity of INPRES school construction experienced an increase in the education of the village had of 0.6 years of schooling, which represent a 6.1% increase over the sample mean.

Figures 2a to 2c above these lines illustrated the timing of the increase in village head education for different sets of villages. The figures show that, villages that adopt elections after 1994, experienced a sizable increase in the education of the village head that coincides with the timing of elections. Note that the figures also show a clear absence of pre-trends in the years leading to the first election post 1992. In addition to supporting the validity of the empirical strategy, this result also suggest that the entry of cohorts partially educated in the INPRES schools does not result in a more educated village head. To further illustrate this point Figure 3 in the Online Appendix presents a similar figure restricted to villages that held elections between 1997 and 1999. These villages also held elections between 1989 and 1991. However for the pre-1992 electoral cycle we observe no effects, while the effect is sizeable for the post-1992 electoral cycle.

Changes in the Local Political Equilibrium

The results presented in Table 6 suggest that the entry of young and educated candidates changed the local political equilibrium and, as a result, a more educated village leader was elected. Data limitations prevent an exhaustive analysis of the nature of this change in the political equilibrium. In particular, no dataset has systematically recorded the characteristics of the candidates contesting village elections.

In order to mitigate the scarcity of data, I hired an Indonesian research assistant to conduct online searches on village elections. He was able to obtain information on the age and education of candidates for 38 village-level electoral contests that took place between 2009 and 2015 in 10 districts.[43] Despite the limited geographic and time coverage, these data provide anecdotal evidence about the characteristics of the pool of candidates of some village elections.

Appendix Table 13 presents some summary statistics. Panel A reproduces information from the main dataset for comparison. Panel B summarizes the information on village election candidates, obtained through online searches. On average, each election has about 4 contestants. The average number of years of education of the winner—i.e., the resulting village head— is 12 and the average age is 42.8. Note that, the age composition of the village head quite similar to that of our main dataset. Hence, it is likely that the age profile of candidates in our main dataset is also similar to that featured in the online data.

The last set of rows report descriptive statistics for the youngest contestant of each election. On average the youngest contestant is quite young: 34 years old, nine years younger than the average winner. Figure 4 in the Online Appendix presents the age distributions of the youngest contestant and of the village head. First, we observe that the age distribution of the youngest contestant is substantially shifted to the left, with respect to the age distribution of village heads. Second, there is a substantial mass of villages that had at least one contestant of very young age: 31.6% of villages had a contestant younger than 30 years old and 63% of villages had a contestant younger than 35 years old.

Using this information I can compute what percentage of villages in the estimating sample could have had a contestant that attended the INPRES schools. Note that, as reported in Appendix Table 2, most villages have their first election post-1992 in the second half of the period 1992-2000. In villages that held their election later, a higher number of cohorts educated in the INPRES schools were eligible to run as candidates in the first election post-1992.[44] To compute the fraction of elections that could have had a contestant that attended

[43]The source of information was typically news portals and Indonesian district-government websites. The research assistant was required to provide the link to the online source.

[44]For example, in villages that held elections in 2000, 8 cohorts potentially educated in the INPRES schools

the INPRES schools, I use the empirical distribution of dates of the first election post-1992 and assume that all elections had the age-distribution of contestants that we observe in the new data collected. The results suggest that 40% of the electoral contests in the sample could have had at least one contestant that attended the INPRES schools. This corresponds to a substantial fraction of villages.

Note that, if the reduced for results are driven by this 40% of villages, the magnitude of the effects would be larger, but still plausible. In particular, this would suggest that villages with one INPRES schools over the mean and where there was a young entrant, experienced increases of 17%, 15%, and 4.2% in their likelihood of having access to health centers, doctors, and safe drinking water after the first election post-1992.

Next, I discuss the empirical relevance of two ways in which the entry of these cohorts could have resulted in a more educated village head. First, one of the new entrants could obtain the largest vote share and become the new village head. The second channel would emerge if the entry of new candidates generated an equilibrium effect: the new configuration of electoral supports could lead to changes in the identity of the preferred candidate, even when the preferred candidate is not among the cohort of new entrants. For instance, the competition of more educated entrants could make the level of education a more salient characteristic of candidates. As a result, among two leading candidates, the population might start preferring the more educated one after the entry of more educated cohorts. This could happen even if the two leading candidates were established candidates, hence, not members of the new cohorts of entrants.

Columns 5 and 6 of Table 6 investigate the empirical relevance of each these two channels. I create a dummy—*"Young VH"*—that takes value 1 if the village head elected after the first post-1992 election was young enough to have attended the INPRES schools.[45] Eighteen percent of the villages elected a village head who is young enough to have attended the INPRES schools in the first election after 1992. This implies that among the 40% of villages with at least one young candidate, 45% of them resulted in the young candidate elected as village head. In column 5, I interact the *Young VH* with the $postel92_{vpt}$ dummy. The results suggest that the increase in the level of education of the village head was much stronger in villages where the elected village head was young enough to have attended the INPRES schools. In those villages, the increase in the education of the village head was of 1.4 years of education, which represents a 14% increase over the sample mean. However, even in villages where the village head elected in the first election after 1992 was too old to had attended

could run as candidates for the first time: those with ages between 25 and 33.

[45]This dummy takes value 1 if the village head was 25 or younger and the village had elections in 1992, takes value 1 if the village head was 26 or younger and the village had elections in 1993, etc.

the INPRES schools, there was also a small increase in the years of education of the village head: the unineteracted post-1992 dummy is positive and statistically significant. Hence, these results suggest that, while the two types of changes in the political equilibrium seem to play some role, the first one—younger entrants becoming the village head—is quantitatively more important.[46]

Age and Gender of the Village Head

There is a large literature on the effects of gender of politicians on policy outcomes (see for instance Chattopadhyay and Duflo (2004) and Clots-Figueras (2011, 2012)) and a recent literature on the effects of the age of politicians (Alesina, Cassidy, and Troiano (2015)). In order to explore the relevance of these characteristics in the Indonesian local context, I estimate the baseline econometric specification when the dependent variable is the age of the village head or a dummy for whether the village head is male. The results on age are presented in Appendix Table 14. They suggest that the age of the village head declined by 3.2 years after the first election post-1992. However, the intensity of the INPRES program does not seem to be associated with a differential change in age. This suggest that changes in the age of the village head are unlikely to be the driver of the expansion of public goods which, as shown in the previous section, was particularly strong in villages with more intensive school construction program.

Appendix Table 15 shows that there are no effects on the likelihood that the village head is a male. This is not surprising given that in only 3% of the villages the village head is a woman. Hence, changes in the gender composition are unlikely to be a relevant mechanism behind the results.

5.2 The Effect of School Construction on The Level of Education of the Electorate

The previous subsection provided evidence that the INPRES program increased the average education of village heads. However, the INPRES program also lead to increases in the educational attainment of the population. Since the population elects the village head, some of the effects in public good provision can potentially be driven by a more educated electorate. Previous research on the effects of education on political attitudes has produced mixed results: while Milligan et al. (2004) and Friedman et al. (2016) find that more educated individuals are more informed about politics, the authors of these studies do not find evidence

[46]Column 6 presents results when the interaction with the intensity of the program are incorporated. While both interaction terms are positive, the triple interaction term is imprecisely estimated.

that more education makes individuals more likely to turn out to vote. Milligan et al. (2004) find that more educated individuals participate more actively in community meetings, while Friedman et al. (2016) find no effects on participation in politics or community affairs.[47]

It is possible that part of the results presented in this paper are driven by more educated villagers demanding greater provision of public goods. Note that the empirical specification mitigates, to some extent, this possibility. The main identifying variation exploits the combination of the INPRES intensity with the village-level electoral calendar. Furthermore, the year fixed effects control for all secular changes that affect all villages in a similar way. Since the minimum working and voting ages are the same across Indonesia, in all villages every year one more educated cohort joins the labor force and the electorate. To the extent that the increase in the average education attainment of these groups is similar for all villages, these increases would be captured by the year fixed effects. However, if there are differential trends across villages, there could be residual variation in the education of the electorate.

The most direct way of testing for this possibility is to control for the average education of the electorate in each village. I construct a time-varying village-level measure of the average level of education of population eligible to vote using the Indonesia Population census. Appendix Table 16 shows the baseline results when adding as a control the average education of the electorate.[48] The results show that, with the exception of number of doctors, changes in the average education of the electorate are not associated with changes in public good provision. Furthermore, the coefficients on the post-1992 election and its interaction with the intensity of the INPRES school program are remarkably stable to the introduction of controls for the education of the electorate.

Note, that this should not be interpreted as suggesting that the education of the electorate is not a relevant factor in the provision of public goods. The empirical strategy adopted in this study probably controls by a large fraction of the secular increase in the level of education

[47]For identification, Milligan et al. (2004) exploit changes in compulsory schooling laws in the US and the UK. They find positive effects of the level of education on the likelihood of turning out to vote in the US, but this result is driven by the fact that more educated individuals are more likely to register to vote. The authors do not find effects on the likelihood to vote in the UK, where registration is automatic. Friedman et al. (2016) conduct a randomized control trial in Kenya. A random set of schools obtained a scholarship program for top performing girls. The authors study how political attitudes changed for girls that the treatment induced to stay longer in school.

[48]The 2000 population census contains a representative sample of the population of Indonesia. I was able to obtain measures of the education of the population for approximately 20% of the villages in the sample. For these villages, I obtain the average education of the population above 17 years of age at each point in time. For villages with missing information, I use the average value of this variable for neighboring villages (in particular, I use the average for villages within the same subdistrict). A few subdistricts are dropped from the sample because I was unable to identify any of their villages in the population census. For each of the outcomes presented in Appendix Table 16, the first column reproduces the baseline results restricting the sample to the set of villages for which there are measures from the population census. The second column of each outcome includes as controls the average education of the population.

of the population. Therefore, it is natural that adding controls for direct measures of the education of the electorate does not affect the results. Overall, it is unlikely that the level of education of the electorate is a confounding factor of the results presented in this paper.

5.3 The Effect of the Village Head Education on Public Goods Provision

The results presented so far suggest that the most likely mechanism behind the estimated increase in public good provision is the increase in the level of education of village heads. This mechanism is also consistent with the literature on the Indonesian village context. The high concentration of power in the figure of the village head during the Soeharto period, made the village head a key player in the management of village public goods (Evers (2000), Antlöv (2003)).

In this subsection, I combine the results from Tables 3 and 6 to infer the implied elasticities for public good provision per extra year of education of the village head. Villages with one INPRES school over the sample mean experience an increase of 0.54 in the years of education of the village head after the post-1992 election. These villages also experience increases of 0.7, 0.7 and 1.3 percentage points in the likelihood of having a primary health-care center, doctors, and access to safe water. If the identified increase in these public goods was only driven by the education of the village head, these results would imply that every year of education of the village head increases the provision of these public goods by 13%, 11% and 3%, respectively.

These results suggest that the increase in the education of village heads could account for 44%, 32%, and 69% of the increase in the provision of each of these public goods over the sample period.[49,50]

Overall these results suggest that the increase in the education of village heads could account by a substantial fraction of the provision of public goods over the sample period.

5.4 Additional Results

Since the higher education of village heads is one of the most likely mechanisms for the estimated effects in public good provision, it is important to investigate through which channels could they deliver more public goods. Based on the Indonesian qualitative literature

[49]During the sample period the average years of education of the village head increased by 3.3. I compute the predicted increase in public good provision using the aforementioned elasticities. Then I divide this predicted increase by the actual increase in each public good during the sample period.

[50]In section 7.3 of the Online Appendix, I present the complete set of 2SLS results, a detailed discussion on the identification assumptions, and a number of additional robustness checks.

and on my own conversations with experts in village governance, I identify three main ways through which village heads can affect public good provision in their villages:

1. Obtaining more funds from upper levels of government.

2. Reallocating village government expenditures towards development projects.

3. Using village funds and managing development projects more efficiently.

One way of providing evidence for these channels is to study the effect of village head education on the different components of the village budget. Information on the village budget is provided for the year 1996. Hence, I estimate the following cross-sectional specification:

$$y_{vp} = \beta_0 + \beta_1 educ_v + \gamma_p + \mathbf{X'_{vp}}\boldsymbol{\delta} + \varepsilon_{\mathbf{vp}} \tag{4}$$

where y_{vp} is the amount of a particular budget category in village v of province p, $educ_v$ is the level of education of the village head, γ_p are province fixed effects, and $\mathbf{X'_{vp}}$ is a vector of village-level controls.[51] I estimate equation (4) by OLS and by 2SLS. In the latter methodology, I instrument the education of the village head using a dummy that takes value 1 if the village has had their first election after 1992 by the year 1996, which corresponds to the year when information on the village budget is available.

Appendix Table 17A shows the result on different sources of village government revenue, and Appendix Table 17B shows the results on village government expenditures. The OLS results suggest that villages with more educated village heads have higher total revenues and that this is driven by sources of income within the village. However, the 2SLS results are very imprecisely estimated and, hence, the results are not conclusive. The results also suggest that more educated village heads do not obtain more funding from upper levels through the transfers to the village budget.

Appendix Table 17B shows the effect on village government expenditures. The results suggest that more educated governments spend more on infrastructure, expenses in goods, and social facilities. These results are consistent with the hypothesis that more educated village heads reallocate the village budget towards development expenses.

In Table 7, I explore the impact of the education of village heads on the number and characteristics of development projects undertaken in the village. Information on development projects is available for the years 1983 and 1986. Hence, I estimate equation (4) by OLS

[51] The controls include a quartic in log population and a quartic in the percentage of households whose main occupation is in agriculture. Since this specification does not include village fixed effects, it is important to add these other additional controls.

and 2SLS in a cross section of villages where the dependent variable are different characteristics of these projects.[52] The 2SLS specification uses as instrument the average education of the village head in neighboring villages. Column 1 in panel B suggests that one additional year of education of the village head increases the number of development projects in the village by 0.17. However, the effect is not statistically significant. The estimate becomes more precise when I examine the effect on the number of projects managed by the village (column 2). In this case, one additional year of education increases the number of village projects by 0.21. The data also provide information on the main source of funding for each project. Columns 3 to 5 suggest that the increase in the number of projects is mainly driven by projects funded by villagers and not by projects funded by upper-levels of government. While this is inconsistent with mechanism 1 described above, an important caveat is that the list of development projects recorded in the village census, might not have been an exhaustive list. It is possible that village leaders were more likely to report projects funded by village sources, since those may have been the projects for which villagers had a greater sense of ownership.

Overall, these results suggest that mechanisms 2 and 3 were empirically relevant, but are not fully conclusive about mechanism 1. In other words, I find that more educated village heads reallocate resources towards infrastructure and social facilities, and engage in more development projects managed by the village.

In order to test whether more educated village heads are more effective managers, I develop a measure of efficiency in the management of development projects. Following an analysis analogous to Rasul and Rogger (2016), I examine within-type-of-project variation on duration of projects. For a subset of projects there is information on the duration of the project, the completion rate and the type of project. I estimate a regression analogous to (4) where the unit of observation is the project level. Column 6 of Table 7 explores the effect of village head education on project duration. Both the OLS and the 2SLS results suggest that more educated village heads spend fewer time in development projects. However, this result could be confounded by more educated village heads engaging in less ambitious project. The results presented in column 7 include dummies for the type of project and dummies for the completion rate of each project. The coefficient on the education of the village head remains negative and statistically significant.[53] This evidence suggests that for the same type of

[52]Note, that information on the education of the village head is reported in the year 1986, but not in 1983. I combine both waves in order to increase the precision of the estimates, but still the analysis is cross-sectional. The dependent variables are defined as the average characteristics of projects undertaken in village v in the years 1983 and 1986.

[53]More formally, the specification I estimate in column 7 is the following

$y_{jvp} = \beta_0 + \beta_1 educ_{vp} + \beta_2 completion_{jvp} + \beta_3 type_{jvp} + \beta_4 completion_{jvp} \times type_{jvp} + \gamma_p + \mathbf{X'_{vp}}\boldsymbol{\delta} + \varepsilon_{\mathbf{vp}}$

where subindex j denotes the project, y_{jvp} is the duration of project j, that took place in village v in

project, villages with more educated village heads are able to achieve a given completion rate in less amount of time than villages with less educated village heads. This constitutes highly suggestive evidence that better educated village heads are better managers of development projects. This in turn could explain why villagers are more willing to contribute to these projects and also the overall increase in the number of projects managed by the village.

6 Discussion

This paper investigates the impact of a large school construction program in Indonesia on public good provision. By combining the presence of the program with the staggered timing of village elections, I isolate the effects that are driven by changes in local governance. The results show that public good provision substantially increased after the first election where newly educated cohorts could run as candidates, especially in villages that experienced an intense INPRES school construction program in the mid 1970s. Furthermore, the effects are heterogeneous across villages: public goods increased more in villages where there is a particular underlying demand for that specific public good.

In a second part of the paper, I explore the mechanisms behind these results. First, I provide anecdotal evidence that suggests that 40% of the elections had a candidate that could have attended the INPRES schools. It is likely that the entry of these candidates lead to changes in the local political equilibrium. I provide evidence of this by documenting that the village head that resulted from these election had more years of education. In contrast, other characteristics of village heads did not experience significant changes after these elections. Furthermore, I provide evidence that changes in the level of education of the electorate can not account for these results. This suggests that the increase in the level of schooling of the village head is a key driver of the increase in public good provision.

Building on this insight, I compute the implied elasticities: If the identified increase in these public goods was only driven by the increase in the education of the village head, these results would imply that every year of education of the village head leads to increases of 13%, 11% and 3% in access of health centers, doctors, and safe drinking water, respectively. These results could account for 44%, 32%, and 69% of the increase in the provision of each of these public goods over the sample period.

Additional empirical results suggest that more educated village heads achieve higher public good provision by reallocating funds towards development projects and managing

province p, $completion_{jvp}$ are a set of dummies for different completion intervals and $type_{jvp}$ are a set of dummies for each type of project. Interactions of these two set of dummies are also included. The rest of controls are the same as in specification (4).

them more efficiently.

Overall, these findings indicate that improvements in the level of education of village heads could generate large increases in household access to basic services. The results of this paper are consistent with the recent literature that suggests that the level of human capital of the public sector is a key determinant of state capacity, public good provision, and long run development in developing countries.

Still, there are a number of open questions. First, it would be interesting to evaluate if the same results would apply to other contexts where local leaders had less autonomy over public good provision. Second, it is unclear what specific constraints educated village heads overcame. The evidence that they manage projects more efficiently could suggest that their skills and cognitive abilities might have been enhanced by staying extra years in school. However, it could also be that the entry of younger and more educated cohorts in the village electoral contests breaks a previous equilibrium of elite capture. Discerning between these different hypotheses seems a fruitful area for further research.

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Figure 1: Timeline of the INPRES Program and its Effects on Village Government

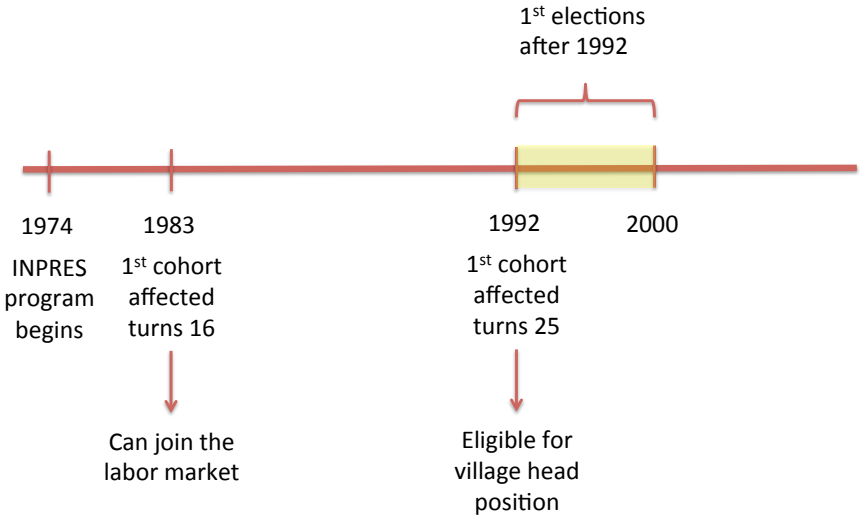
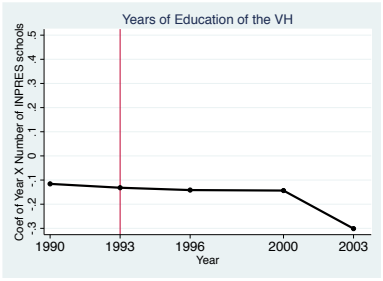
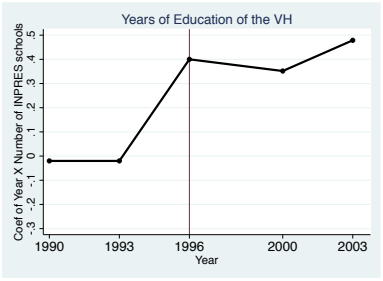


Figure 2: Yearly Effects of Number of INPRES schools for Different Sets of Villages

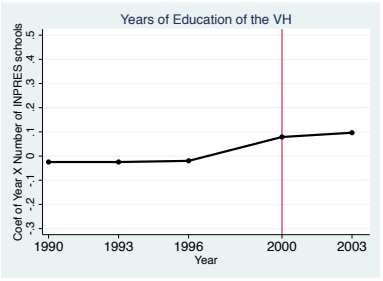
(a) Villlages with Elections 1992-1993



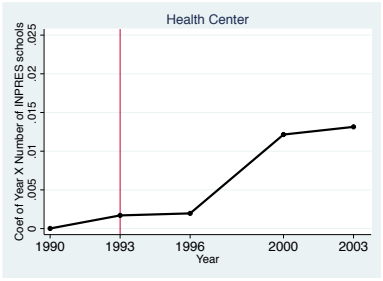
(b) Villages with Elections 1994-1996



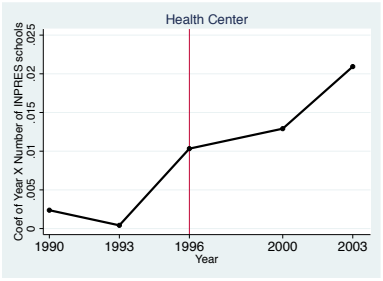
(c) Villages with Elections 1997-2000



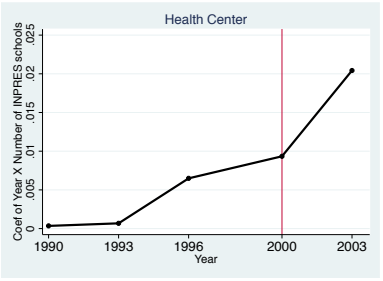
(d) Villages with Elections 1992-1993



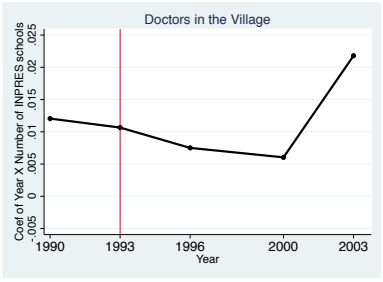
(e) Villages with Elections 1994-1996



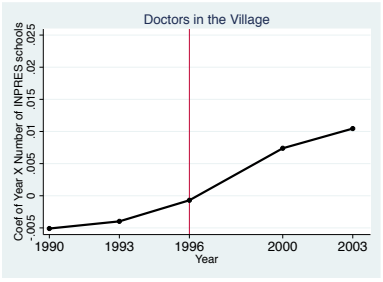
(f) Villages with Elections 1997-2000



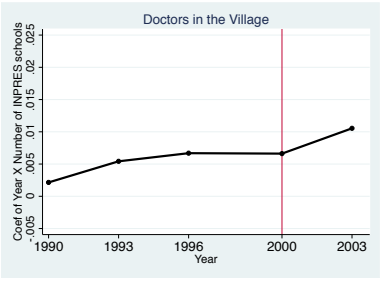
(g) Villages with Elections 1992-1993



(h) Villages with Elections 1994-1996



(i) Villages with Elections 1997-2000



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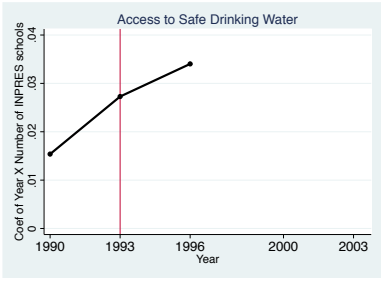
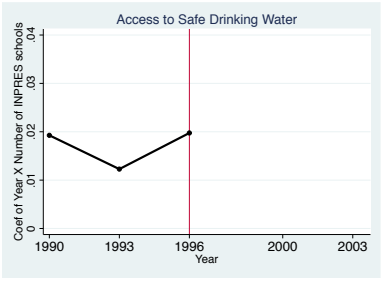


(k) Villages with Elections 1994-1996



(l) Villages with Elections 1997-2000

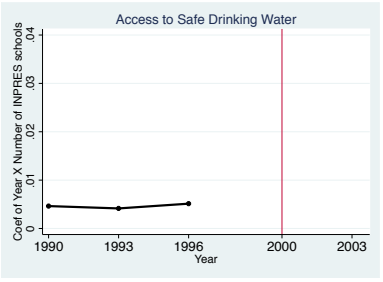


Table 1. Summary Statistics

	(1) Observations	(2) Mean	(3) Std. Dev.	(4) Years Included
Number of Population	59,130	3195	1,654	1986-2003
Number of Households	59,130	759	411	1986-2003
Primary School Enrolment Rate	9,851	0.94	0.10	1986
Percentage of Rural Households	59,130	0.63	0.31	1986-2003
Urban Village	59,130	0.10	0.30	1986-2003
Distance to District Capital	39,420	23.69	20.71	90, 93, 00, 03
Number of Inpres Schools	9,855	0.89	0.72	1980
Primary Health Center in the Village	59,130	0.10	0.30	1986-2003
Doctor in the Village	59,130	0.12	0.33	1986-2003
Access to Safe Drinking Water	39,420	0.77	0.42	1986-1996
Number of Health Posts	38,748	4.08	2.47	90, 93, 00, 03
Garbage Disposal	59,130	0.91	0.29	1986-2003
Number of Primary Schools	59,130	3.13	1.67	1986-2003
Number of High Schools	59,130	0.41	0.75	1986-2003
Years of Education of the Village Head	59,130	9.87	3.13	1986-2003
Years of Education 25th percentile	59,130	6	-	1986-2003
Years of Education 50th percentile	59,130	9	-	1986-2003
Years of Education 75th percentile	59,130	12	-	1986-2003
Age of the Village Head	59,105	42.95	9.23	1986-2003
Age 25th percentile	59,130	36	-	1986-2003
Age 50th percentile	59,130	42	-	1986-2003
Age 75th percentile	59,130	49	-	1986-2003
Male Village Head	59,122	0.97	0.17	1986-2003

Notes: Summary statistics corresponding to 9,855 villages in 4 provinces and 82 districts. For most variables, information is available for the years: 1986, 1990, 1993, 1996, 2000, and 2003.

Table 2. Determinants of the Timing of Village Elections

	Dependent Variable: Year of the 1st election after 1992			
	Coefficient	Standard Error	Standardized Effect	Standard Error of Standardized Coefficient
	(1)	(2)	(3)	(4)
Log Population	-1.905	(1.603)	-0.016	(0.013)
Percentage of Rural Households	0.008	(0.059)	0.002	(0.012)
Urban Village	-0.013	(0.020)	-0.007	(0.010)
Share of Land in Agriculture	0.021	(0.094)	0.003	(0.012)
Number of Doctors	-0.057	(0.038)	-0.018	(0.012)
Number of Health Centers	0.046	(0.172)	0.003	(0.011)
Number of Health Posts	0.011	(0.027)	0.004	(0.011)
Safe Drinking Water	0.080	(0.051)	0.019	(0.012)
Critical Land	0.005	(0.017)	0.003	(0.010)
Bin Garbage Disposal	-0.010	(0.019)	-0.006	(0.013)
Asphalt / Hard Road	0.001	(0.016)	0.001	(0.011)
Horse-drawn cart (*pedati*)	-0.023**	(0.009)	-0.020**	(0.008)
Number of Primary Schools	-0.053	(0.092)	-0.006	(0.011)
Number of High Schools	-0.023	(0.022)	-0.010	(0.009)
Village Cooperative	-0.023	(0.029)	-0.009	(0.011)
Other type of Village Cooperative	-0.032**	(0.014)	-0.024**	(0.011)
Village Group Shop	0.003	(0.025)	0.001	(0.011)
Number of Churches	-0.075	(0.048)	-0.018	(0.012)
Number of Mosques	0.047	(0.030)	0.014	(0.009)
Number of Markets	-0.029	(0.046)	-0.007	(0.011)
Number of Banks	-0.056*	(0.029)	-0.026*	(0.014)

Notes: Bivariate regressions estimated in the cross-section of villages. The dependent variable is the year of the 1st election post-1992. The regressor of interest is defined by each row. In particular it corresponds to the percentage change of the corresponding covariate between two pre-treatment years: 1986 and 1990. When information for 1990 is missing, I use the percentage change between 1983 and 1986. Columns 3 and 4 show the standardized (beta) coefficient and its corresponding standard error. 9,855 villages included.

Table 3. The Effects of School Construction on Public Good Provision

	(1)	(2)	(3)	(4)
Panel A. Dependent Variable: Primary Health Center in the Village				
post 1st Elec after 1992	-0.005*	-0.005*	-0.016***	-0.017***
	(0.003)	(0.003)	(0.004)	(0.004)
post * Num INPRES Schools§		0.012***		
		(0.003)		
post * Num INPRES > 0			0.017***	
			(0.004)	
post * INPRES schools =1				0.013***
				(0.005)
post * INPRES schools =2				0.024***
				(0.006)
Observations	59,130	59,130	59,130	59,130
R-squared	0.836	0.837	0.837	0.837
Mean of the Dependent Variable	0.10	0.10	0.10	0.10
Panel B. Dependent Variable: Doctors in the Village				
post 1st Elec after 1992	0.001	0.001	-0.006	-0.006
	(0.003)	(0.003)	(0.005)	(0.005)
post * Num INPRES Schools§		0.006*		
		(0.003)		
post * Num INPRES > 0			0.010*	
			(0.005)	
post * INPRES schools =1				0.009*
				(0.005)
post * INPRES schools =2				0.011
				(0.007)
Observations	59,130	59,130	59,130	59,130
R-squared	0.820	0.820	0.820	0.820
Mean of the Dependent Variable	0.12	0.12	0.12	0.12
Panel C. Dependent Variable: Access to Safe Drinking Water				
post 1st Elec after 1992	-0.003	-0.005	-0.017**	-0.017**
	(0.005)	(0.005)	(0.007)	(0.007)
post * Num INPRES Schools§		0.018**		
		(0.007)		
post * Num INPRES > 0			0.019**	
			(0.009)	
post * INPRES schools =1				0.010
				(0.009)
post * INPRES schools =2				0.036**
				(0.014)
Observations	39,420	39,420	39,420	39,420
R-squared	0.922	0.923	0.922	0.923
Mean of the Dependent Variable	0.77	0.77	0.77	0.77

Notes: Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts and 9,855 villages. The unit of observation is the village-year level. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003. § The number of INPRES schools is defined in deviations from its sample mean. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects. *** p<0.01, ** p<0.05, *p<0.1.

Table 4. Heterogeneous Effects of School Construction on Public Good Provision

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variables:					
	Primary Health Center in the Village		Doctors in the Village		Access to Safe Drinking Water	
	Interaction: Num INPRES Schools	Interaction: Dummy INPRES Schools >0	Interaction: Num INPRES Schools	Interaction: Dummy INPRES Schools >0	Interaction: Num INPRES Schools	Interaction: Dummy INPRES Schools >0
Dep. Var. Mean	*0.10*	*0.10*	*0.12*	*0.12*	*0.77*	*0.77*
post 1st Elec after 1992	-0.056***	-0.055***	-0.063***	-0.063***	-0.044***	-0.044***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
post * measure of INPRES school intensity	0.000	-0.001	0.000	-0.000	-0.000	0.000
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
post * bad baseline service	0.055***	0.040***	0.069***	0.060***	0.160***	0.126***
	(0.004)	(0.005)	(0.005)	(0.007)	(0.019)	(0.033)
post * bad baseline service * INPRES sch. intensity	0.014***	0.021***	0.007*	0.013**	0.057**	0.057
	(0.003)	(0.005)	(0.004)	(0.005)	(0.023)	(0.036)
Observations	59,130	59,130	59,130	59,130	39,420	39,420
R-squared	0.837	0.837	0.821	0.821	0.924	0.924

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts and 9,855 villages. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003. In the odd columns the measure of INPRES school intensity used corresponds to the number of INPRES schools in deviations from its sample mean. In even columns the intensity measure corresponds to a dummy for any INPRES schools constructed in the village. In columns 1 and 2 bad baseine service equals 1 for villages with no primary care facilities in year 1986. In columns 3 and 4 bad baseine service equals 1 for villages with no doctos in year 1986. In columns 5 and 6 bad baseine service equals 1 for villages with no access to purified drinking water in 1986. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects.*** p<0.01, ** p<0.05, *p<0.1.

Table 5. Robustness Checks of the Effects of School Construction on Public Goods

	Additional Controls:				
	Log Population	Pre-treatment Dependent Variable * Year FE	Pre-treatment Covariates* Year FE	Enrolment & Water and Sanitation *Year FE	Pre-treatment effects
	(1)	(2)	(3)	(4)	(5)
Panel A. Dependent Variable: Primary Health Center in the Village					
post	-0.005*	-0.005*	-0.005*	-0.005*	-0.005*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
post * Num INPRES Schools§	0.012***	0.014***	0.012***	0.012***	0.013***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
1 Year Before Election*Num INPRES Schools§					0.005
					(0.004)
2 Year Before Election*Num INPRES Schools§					0.005
					(0.004)
3 Year Before Election*Num INPRES Schools§					0.005
					(0.003)
Observations	59,130	59,130	59,130	59,106	59,130
R-squared	0.837	0.838	0.837	0.837	0.837
Mean of the Dependent Variable	0.10	0.10	0.10	0.10	0.10
P-value Joint Significance of Pre-treatment Interactions					0.221
Panel B. Dependent Variable: Doctors in the Village					
post	0.001	0.000	0.000	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
post * Num INPRES Schools§	0.006*	0.007*	0.006*	0.006*	0.007*
	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
1 Year Before Election*Num INPRES Schools§					0.001
					(0.005)
2 Year Before Election*Num INPRES Schools§					0.005
					(0.004)
3 Year Before Election*Num INPRES Schools§					0.006*
					(0.003)
Observations	59,130	59,130	59,130	59,106	59,130
R-squared	0.820	0.821	0.822	0.821	0.820
Mean of the Dependent Variable	0.12	0.12	0.12	0.12	0.12
P-value Joint Significance of Pre-treatment Interactions					0.280
Panel C. Dependent Variable: Access to Safe Drinking Water					
post	-0.005	0.000	-0.005	-0.005	-0.005
	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)
post * Num INPRES Schools§	0.018**	0.013**	0.017**	0.017**	0.019**
	(0.007)	(0.006)	(0.007)	(0.007)	(0.008)
1 Year Before Election*Num INPRES Schools§					0.003
					(0.007)
2 Year Before Election*Num INPRES Schools§					0.001
					(0.007)
3 Year Before Election*Num INPRES Schools§					0.005
					(0.004)
Observations	39,420	39,420	39,420	39,404	39,420
R-squared	0.923	0.933	0.923	0.923	0.923
Mean of the Dependent Variable	0.77	0.77	0.77	0.77	0.77
P-value Joint Significance of Pre-treatment Interactions					0.690

Notes: Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts and 9,855 villages. The unit of observation is the village-year level. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects. Additional controls are included as shown in the column headings.*** p<0.01, ** p<0.05, *p<0.1.

Table 6. First Stage: The Effects of School Construction on Village Head Education

	Dependent Variable: Years of Education of the Village Head					
	(1)	(2)	(3)	(4)	(5)	(6)
Mean of the Dependent Variable	*9.87*	*9.87*	*9.87*	*9.87*	*9.87*	*9.87*
post 1st Election after 1992	0.433***	0.429***	0.367***	0.364***	0.299***	0.294***
	(0.089)	(0.088)	(0.087)	(0.087)	(0.092)	(0.090)
post 1st Election after 1992*Num INPRES schools§		0.110*				0.113*
		(0.057)				(0.062)
post 1st Election after 1992*INPRES schools >0			0.095			
			(0.075)			
post 1st Election after 1992*INPRES schools =1				0.029		
				(0.076)		
post 1st Election after 1992*INPRES schools =2				0.238**		
				(0.119)		
post * Young VH					1.125***	1.129***
					(0.126)	(0.125)
post * Young VH * Num INPRES schools§						0.035
						(0.118)
Observations	59,130	59,130	59,130	59,130	59,105	59,105
R-squared	0.583	0.583	0.583	0.583	0.588	0.588

Notes: Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts. The unit of observation is the village-year level. The sample includes 9,855 villages. The dependent variable is the numer of years of education of the village head in office in the corresponding village and year. § The number of INPRES schools is defined in deviations from its sample mean. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003. All regressions control for village fixed effects and year fixed effects. *** p<0.01, ** p<0.05, *p<0.1.

Table 7. Effect of Village Head Education on Development Projects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent Variables:						
			Number of pojects by main source of funding:				
	Number of projects	Number of projects managed by the village	by the villagers	by central government grants	by central government budget allocation	Project duration	Project duration (controlling for project completon and type)
Dep. Var. Mean	*5.66*	*4.56*	*2.38*	*1.48*	*1.49*	*3.56*	*3.56*
	Panel A. OLS						
years of educ VH	0.015	0.026**	0.025**	-0.013*	0.004	-0.040***	-0.036***
	(0.013)	(0.012)	(0.012)	(0.006)	(0.006)	(0.013)	(0.013)
	Panel B. 2SLS						
years of educ VH	0.169	0.213**	0.238**	-0.094	0.042	-0.245***	-0.175**
	(0.108)	(0.095)	(0.107)	(0.060)	(0.052)	(0.071)	(0.069)
Observations	9,855	9,855	9,855	9,855	9,855	7,637	7,637
R-squared (OLS)	0.123	0.075	0.093	0.054	0.104	0.008	0.076

Notes: Robust Standard errors, clustered at the district level, in parenthesis. Columns 1 to 5 correspond to cross-sectional regression where the unit of observation is the village level. The dependent variable in column 1 corresponds to number of development projects undertaken in the corresponding village between the years 1983 and 1986. The dependent variable in columns 2 to 5 corresponds to the number of projects that had the characteristic defined by the column heading. The regressor of interest corresponds to the years of education of the village head in office in 1986. Columns 6 and 7 correspond to regressions at the project level. Each observation corresponds to a development project undertaken in the years 1983 or 1986. The regressor of interest corresponds to the years of education of the village head in office in 1986 in the village where the project was undertaken. Panel A shows the OLS results while Panel B shows the 2SLS results where the average education of village heads in neighboring villages is used as instruments for the education of the village head. All regressions include province fixed effects, a quartic on log population, and a quartic on the percentage of rural households as controls. *** p<0.01, ** p<0.05, * p<0.1.

7 Online Appendix for "The Local Political Economy Effects of School Construction in Indonesia"

Monica Martinez-Bravo

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[For Online Publication Only]

7.1 Further Details on the Historical Background

Requirements for Candidates to the Village Head position

Before every village head election a Village Election Board was created. The Board was in charge of supervising the election process and to verify that all candidates satisfied the requirements stipulated in the Law no. 5 of 1979 on Village Governance. The set of requirements are the following:

1. Devoted to God Almighty.
2. Faithful and loyal to the Pancasila—national ideology of Indonesia—and the 1945 Constitution.
3. Well-behaved, honest, fair, intelligent, and authoritative.
4. No direct of indirect involvement with the communist party.
5. No voting rights revoked.
6. Not a criminal record.
7. Registered as a permanent resident and living in the village in question for at least 2 years or born in the village.
8. Being at least 25 years old and at most 60 years old.
9. Junior high school education or equivalent knowledge or experience.

Note that the last requirement specifies a minimum education level: junior high school. However, the wording of the law is explicitly ambiguous to allow a lax enforcement of the requirement: many candidates without a junior high school degree could claim that they have an "equivalent knowledge or experience" to a junior high school degree. Not surprisingly, the junior high school requirement was not strictly enforced, as shown in Appendix Figure 5.

7.2 Data Appendix

Of the 22,000 villages in the island of Java, I am able to consistently match 14,569 across the 6 waves included in this study. Among these villages, 2,839 of them do not report consistent term lengths of the village head for years 1996, 2000, and 2003 and are dropped for the sample. In addition to this, 368 villages are dropped because of having missing information on the years of education of the village head, the length of tenure of the village head or the number of INPRES schools. Finally, 1,091 villages are dropped because of having more than 10,000 inhabitants (the average village has 3,378 inhabitants) or more than two INPRES schools constructed. The large size of these villages could influence some of the results. 416 of the remaining villages have an appointed village head (*kelurahan* villages) and are excluded from the main analysis. The resulting sample contains information on 9,855 villages. The baseline specifications in the paper focus on the period 1986-2003 when information of the education of the village head is available. There are 6 census waves in this period. Consequently, the number of village-year observations in the baseline specification is 59,130. In some regressions the number of observations is lower than this. This is due to the fact that some outcome variables are not reported in every wave of the census or by a few missing observations in the outcome variable for some years. When information missing for some years, I mention it in the table notes.

Construction of Variables

Years of Education of the Village Head: This variable is constructed as follows: no schooling = 0 years of schooling; not completed primary school = 2 years; primary school = 6 years; junior high school = 9 years; senior high school = 12 years; academy = 14 years; college = 16 years.

Village-level electoral calendar: The electoral calendar in each village is derived from the reported length of tenure of the village head in 1993. In particular, the year of the first election post-1992 is inferred using the following procedure.

- If the village head reports having been in office between 0 and 1 years in 1993, I know there was an election in that village between 1992 and 1993. I assume that election was the first election post-1992.
- If the village head reports having been in office for 2 or 10 years in 1993, I know there was an election in 1991. Since this election was prior to 1992, the first election after 1992 is scheduled to take place in 6 years. Hence, in 1999.

- If the village head reports having been in office for 3 or 11 years in 1993, I know there was an election in 1990. Since this election was prior to 1992, the first election after 1992 is scheduled to take place in 5 years. Hence, in 1998.
- Similarly for the rest of villages.

Measures of Public Goods:

- **Primary Health Care Center**: This outcome variable is a dummy that takes value one if the village has a polyclinic or a *puskesmas*. *Puskesmas* are primary health care centers in charge of basic medical services and preventive care. In polyclinics households can have access to more advanced medical treatments. Formally trained doctors and nurses work in both type of health facilities.
- **Doctors in the village:** This variable takes value one if at least one formally trained doctor lives and works in the village.
- **Access to safe drinking water:** This variable takes value one if most households in the village obtain their drinking water from a pump or from a water company. It takes value zero if households drink water from a natural well, from rain, river or other source.
- **Number of Health Posts**: This variable corresponds to the number of health posts or *posyandu* in the village. These are small community-based healthcare facilities that are responsible for family planning and maternal and child care.
- **Garbage Disposal**: This outcome variable takes value one if the village has a system of garbage disposal through the use of bins or by burying the waste into a hole. It takes value zero if households through their wast to the river or dispose their garbage through some other method.

Proxies for Demands of Public Goods:

- Villages are considered to have a low level of provision of public goods at baseline if in the year 1986 they do not have primary health centers in the village, there are no doctors living in the village, there is no access to purified water, and/or there is not a system of garbage bin disposal. Number of health posts is reported from 1990 onward and, hence, 1990 is considered the baseline year.

- Mortality. Appendix Table 22 uses the mortality rate at baseline as a predictor of the demand for health services. In particular high level of mortality is a dummy that takes value one if the number of deaths per capita in 1986 in the village is above the median number of deaths per capita in the sample.

7.3 The Effect of the Village Head Education on Public Goods Provision: 2SLS Estimates

The results presented in this paper suggest that the most likely mechanism behind the estimated increase in public good provision is the increase in the level of education of village heads. This mechanism is also consistent with the literature on Indonesian village context during the period. The high concentration of power in the figure of the village head during the Soeharto period, made the village head a key player in the management of village public goods (Evers (2000), Antlöv (2003)).

In this subsection I implement an instrumental variables strategy where I use the interaction of the timing of elections and the intensity of the INPRES program to construct an instrument for the level of education of the village head. This strategy allows me to provide estimates of the returns to schooling of village heads in terms of the extra public good provision that they deliver. More formally, I estimate the following econometric model where equation (5) presents the structural equation of interest while equation (6) shows the first stage:

$$y_{vt} = \beta_0 + \beta_1 educ_{vt} + \beta_2 postel92_{vt} + \alpha_v + \delta_t + \varepsilon_{vt} \tag{5}$$

$$educ_{vt} = \rho_0 + \rho_1 postel92_{vt} + \rho_2 postel92_{vt} \times Num\_INPRES_v + \alpha_v + \delta_t + \epsilon_{vt} \tag{6}$$

where $educ_{vt}$ is the number of years of education of the village head in office in year t in village v. The rest of variables are defined as before. As the specification shows, the interaction of the timing of the first election after 1992 and the number of INPRES schools is used as an instrument for the level of education of the village head.[54]

In order to explore whether more educated village heads increase the provision of those public goods in worse condition at baseline, I estimate the following econometric model.

$$y_{vt} = \beta_0 + \beta_1 educ_{vt} + \beta_2 educ_{vt} \times BadService_v + \beta_3 postel92_{vt} + \alpha_v + \delta_t + \varepsilon_{vt} \tag{7}$$

where $BadService_v$ takes value 1 for villages that had bad quality of the public good y in the year 1986. Both the level of education of the village head and its the interaction

[54]Note that the results are robust to excluding $postel92_{vt}$ from equation (5) and, hence, using as instruments both $postel92_{vt}$ and its interaction with the number of INPRES schools.

with the bad service dummy are instrumented for, using specification (3) as first stage. Hence, the instrumental variables used in this specification are the dummy for the period after first election post-1992, $postel92_{vt}$, interacted with the number of INPRES schools, the post dummy interacted with bad service at baseline and the triple interaction of the three variables.

The validity of these instrumental variables strategies require that the following assumptions are satisfied:

1. Relevance: the instruments are correlated with the endogenous regressors.

2. Validity of the first stage: the timing of elections and its interaction terms are quasi-random.

3. Exclusion restriction: the interaction of the timing of the first election post-1992 with INPRES intensity and bad baseline service, conditional on controls, only affects public good provision through changing the level of education of the village head.

The evidence presented in Table 6 supports the validity of the first assumption: the first election after 1992 raises the level of education of village heads by 0.43 years of education, on average. Furthermore, the effect is higher for villages that experienced a more intense INPRES school construction program. Note that the R-squared of the relevant specification (column 2) is high, 0.58. Appendix Table 20 shows the first stage of the heterogeneous effects specification, which also shows substantial R-squares. To further support assumption 1, all the instrumental variables results provided in the paper present the F-statistic of the Cragg-Donald weak instruments test.

The results presented in Table 2 support the validity of the second assumption, since changes in a large number of covariates do not predict the timing of the elections. Furthermore, given that all specifications include village fixed effects, the interaction of the timing of elections with time-invariant factors—such as the number of INPRES schools or bad baseline at service—will also be uncorrelated to the error term of the first stage.

In section 5 of the paper, we provide a number of pieces of evidence that suggest that the main channel behind the estimated effects is the changes in the level of education of the village head. Hence, the assumption that the interactions of the timing of the first election post-1992 only affect outcomes by increasing the education of the village head is a plausible assumption. Nevertheless, in the next subsection, I discuss some potential threats to the exclusion restriction and I present a number of additional robustness checks.

Appendix Tables 18.A and 18.B present the Ordinary Least Squares (OLS henceforth) and the Two Stage Least Squares (2SLS henceforth) results for the instrumental variables

strategies described above. The first and the third column of each outcome variable present the OLS results. The second and fourth columns present the 2SLS results of specifications (5) and (7), respectively.[55]

The results on the estimation of equation (5) suggest that more educated village heads only lead to average increases in public goods for primary health care centers and garbage bin disposal systems. While the point estimates suggest that there are also increases in the other public goods, the results are less precisely estimated. This could be driven by the fact that effective leaders may focus on increasing the provision of those public goods that are in greater need or in worse condition. To further explore the hypothesis of heterogenous effects across villages, the third and the fourth column for each public good present the results where the level of education of the village head is interacted with a measure of bad quality of service at baseline. The results confirm the heterogeneous pattern of public good provision: more educated village heads increase the provision of those public goods that were in worse condition at baseline.[56]

Overall, the results suggest that more educated village heads lead to increases in those public goods for which there is a greater need. Although village head education does not seem to be the only factor in explaining the evolution of the provision of these goods, the increase in village head education can account for a sizable share of the increase.

7.4 2SLS Robustness Checks

In this section I present a number of robustness checks for the 2SLS results and discuss potential threats to the exclusion restriction.

A potential concern with the results presented in Appendix Tables 18.A and 18.B is that the strength of the first stage is moderate: the Cragg-Donald statistic ranges between 0.36 and 8.4t. Appendix Table 21 implements an alternative instrumental variable strategy where the post first-election after 1992 dummy is also used as an instrumental variable—i.e.,

[55]The first stage corresponding to the second column is presented in Table 6, column 2. The first stage corresponding the fourth column is presented in the appropriate column of Appendix Table 20.

[56]The OLS results are smaller in magnitude than the 2SLS results. This could be driven by a number of reasons. First, the number of years of education of village heads might be measured with error, which would generate attenuation bias in the OLS estimation. Second, the OLS might suffer from omitted variable bias. For instance, villages might suffer positive economic shocks (e.g. discovery of natural resources) that, all else equal, increase public good provision. If those same shocks weaken the a accountability relationship and allow ineffective low-educated leaders to stay for longer in power, the OLS would be downward biased. Results along these lines were found in Brollo et al. (2013). Third, in the presence of heterogenous treatment effects, the 2SLS captures the effect of the education of village heads in those villages where the INPRES program induced them to replace their village leaders by more educated ones. This set of villages might be the ones with greater returns to the education of village heads, and hence, higher point estimates of the effect of education of the village head on public good provision.

I impose $\beta_2 = 0$ in equation (5). This additional instrument leads to stronger first stages, with Cragg-Donald statistics substantially higher. Both instrumental variable strategies lead to similar effects of the years of education of the village heads interacted with poor quality of service at baseline. Hence, it is unlikely that the main results are severely affected by weak instruments bias.

Appendix Table 23 presents additional robustness checks similar to those implemented on the reduced form specifications and presented in Table 5. The results show that the results are robust to controlling for population, adding as controls the interaction of the pre-treatment level of primary school enrolment in the village and year fixed effects, and adding as controls the pre-treatment value of the covariates that were correlated to the timing of elections interacted with year fixed effects.[57]

Next, I discuss the possibility that the increases in public good provision were driven by changes in the age of the village head. First, note that the regressions presented in Appendix Table 14 indicate that, while the age of the village heads decreases on average after the first election post-1992, the decline is not associated to the intensity of the INPRES program. This supports the use of the interaction of the post dummy and the intensity of the INPRES program as an instrument for the education of the village head, since it is uncorrelated to the age of the village head.

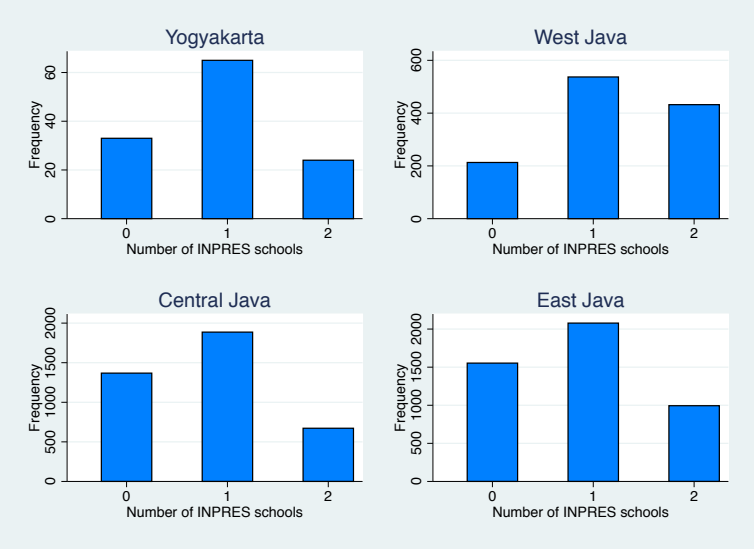
Nevertheless, I implement an additional robustness check where I incorporate the age of the village head as a regressor in the baseline econometric specification of interest. Appendix Tables 19.A and 19.B presents the results. The first column for each outcome variable reproduces the baseline 2SLS reported in Table 7 when restricted to the sample for which age of the village head is reported. The second column includes age as an exogenous regressor to the 2SLS specification. Although the age regressor is statistically significant for primary care facilities and doctors, incorporating this regressor does not affect the results of the interaction terms.[58] The third column includes age and age interacted with poor quality of the corresponding public goods at baseline as endogenous regressors. In order to increase the strength of the first stage, I include as instruments the average age and education of village heads in neighboring villages. The coefficients on years of education and its interaction with bad service are robust to this alternative specification, while the coefficients on age and its interaction are small in magnitude and statistically insignificant.

[57] Appendix Table 22 shows that the results related to health services are robust to alternative measures of demand for public goods. In particular, I show that more educated village heads generate larger increases in the availability of health services in villages that have a high mortality rate at baseline.

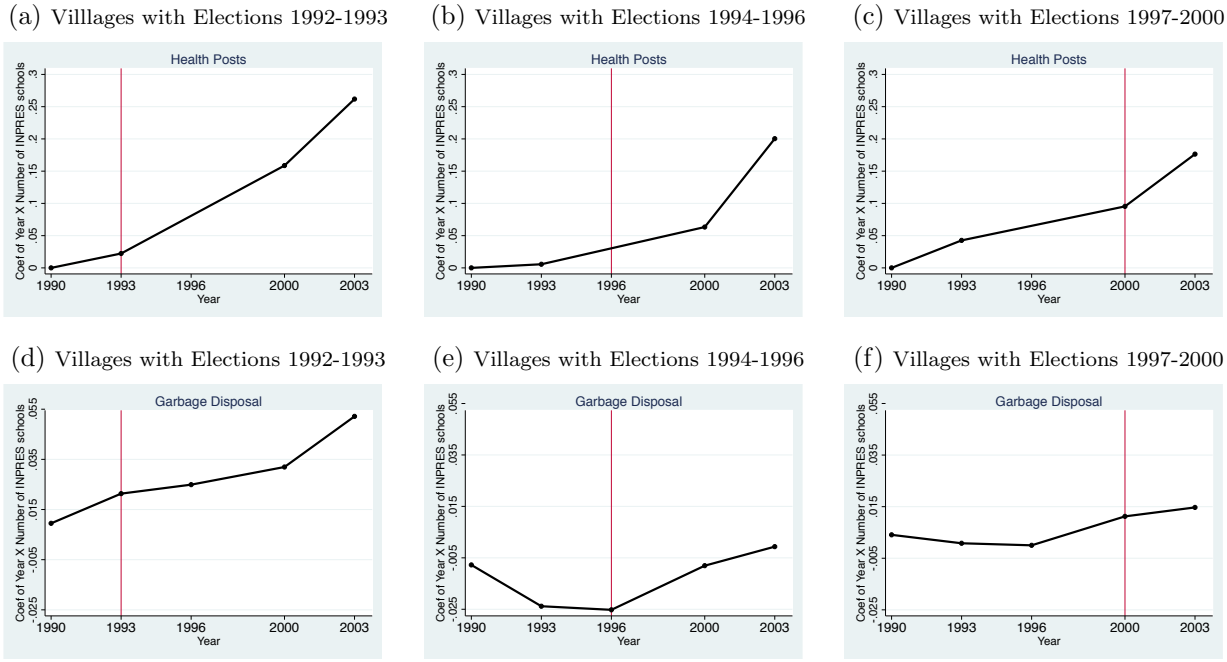
[58] Also, note that results suggest that, if anything, older village heads deliver more public goods. Since age and years of education are inversely correlated the exclusion of age as a regressor downward bias the estimates. Hence, the baseline results should be considered a lower bound on the effect of education on public good provision.

Overall, these results suggest that changes in the age of the village head are unlikely to account for the observed results. Given the similarity of the 2SLS point estimates in both specification, I focus on the main specification.

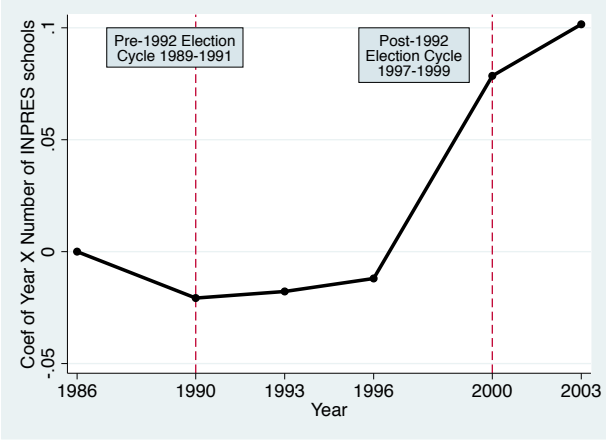
Appendix Figure 1. Frequency of the number of INPRES schools by province



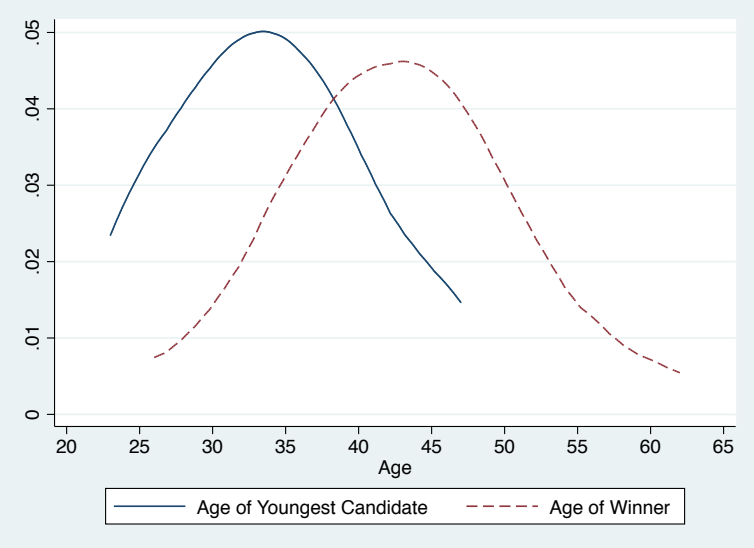
Appendix Figure 2. Yearly Effects of Number of INPRES schools for Different Sets of Villages



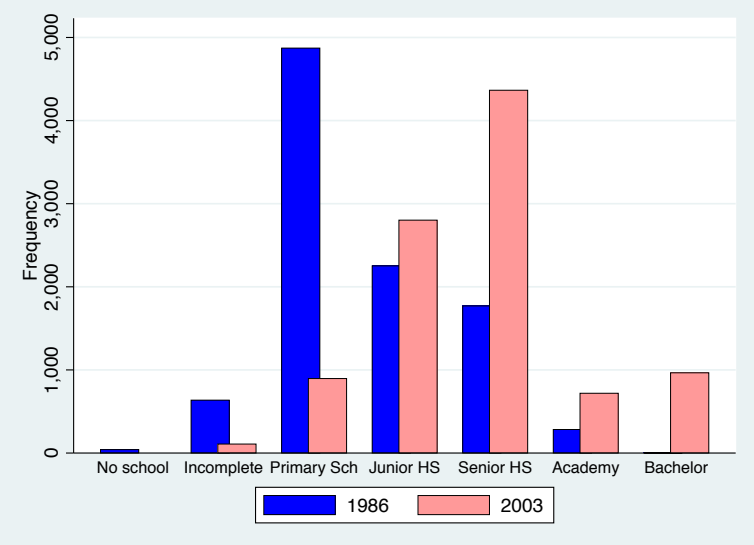
Appendix Figure 3. Effects of Number of INPRES schools on Village Head Education: Villages with Elections between 1989-1991



Appendix Figure 4. Age Distribution of Youngest Candidate and Winner in Village Elections



Appendix Figure 5. Distribution of Village Heads by Level of Education in 1986 and 2003



Appendix Table 1. Average Village Government Budget for year 1996

	Mean	Std. Dev	As a fraction of total revenue
Total Revenue (in 1,000 IDR)	39,671	31,347	
Surplus previous year	251	2,302	0.006
Village Original Income	12,578	11,192	0.317
Village Community Income *(swadaya)*	12,869	16,069	0.324
Transfer from Central Government	6,481	3,955	0.163
Transfer from Provincial Government	729	3,010	0.018
Transfer from District Government	749	2,886	0.019
Other revenues	6,014	18,948	0.152
Total Expenditures (in 1,000 IDR)	39,596	31,132	0.998
Routine expenses	15,268	10,901	0.385
Deficit previous year	41	776	0.001
Employee Expenses	10,148	7,500	0.256
Expenses in Goods	877	1,472	0.022
Expenses in Maintenance	622	1,469	0.016
Travelling Expenses	575	932	0.014
Other routine expenses	2,125	3,433	0.054
Miscellaneous expenses	880	3,177	0.022
Development Expenses	24,328	26,425	0.613
Infraestructure Expenses	2,800	5,556	0.071
Production Facilities	2,491	5,174	0.063
Transportation Facilities	6,981	9,945	0.176
Marketing Facilities	634	4,070	0.016
Social Facilities	6,303	11,447	0.159
Other facilities expenditure	5,120	11,994	0.129

Notes : Summary statistics of the village budget of 1996 corresponding to 9,855 villages.

Appendix Table 2. Timing of the First Election after 1992

Year	Number of villages having their 1st election post-1992 in the corresponding year	Percent
1992	403	4.09
1993	679	6.89
1994	564	5.72
1995	231	2.34
1996	106	1.08
1997	1,244	12.62
1998	2,846	28.88
1999	3,548	36.00
2000	234	2.37
Total	9,855	100

Appendix Table 3.A. The Effect of School Construction on Public Good Provision by Calendar Year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent Variables:								
	Primary Health Center in the Village			Doctors in the Village			Access to Safe Drinking Water		
	Elections 1992-93	Elections 1994-96	Elections 1997-00	Elections 1992-93	Elections 1994-96	Elections 1997-00	Elections 1992-93	Elections 1994-96	Elections 1997-00
Year 1990 * Num INPRES	0.000	0.002	0.000	0.012	-0.005	0.002	0.015*	0.019**	0.005
	(0.000)	(0.002)	(0.001)	(0.008)	(0.007)	(0.002)	(0.009)	(0.009)	(0.004)
Year 1993 * Num INPRES	**0.002**	0.000	0.001	**0.011**	-0.004	0.005	**0.027****	0.012	0.004
	(0.002)	(0.004)	(0.001)	**(0.011)**	(0.008)	(0.003)	**(0.012)**	(0.012)	(0.005)
Year 1996 * Num INPRES	**0.002**	**0.010**	0.006**	**0.008**	**-0.001**	0.007*	**0.034*****	**0.020**	0.005
	(0.007)	**(0.007)**	(0.003)	**(0.012)**	**(0.010)**	(0.004)	**(0.012)**	**(0.013)**	(0.007)
Year 2000 * Num INPRES	**0.012**	**0.013**	**0.009*****	**0.006**	**0.007**	**0.007**			
	(0.009)	**(0.010)**	**(0.003)**	**(0.012)**	**(0.013)**	**(0.004)**			
Year 2003 * Num INPRES	**0.013**	**0.021**	**0.020*****	**0.022**	**0.010**	**0.011**			
	(0.014)	**(0.021)**	**(0.005)**	**(0.017)**	**(0.014)**	**(0.006)**			
Observations	6,492	5,406	45,828	6,492	5,406	45,828	4,328	3,604	30,552
R-squared	0.836	0.842	0.835	0.842	0.802	0.817	0.928	0.913	0.923
P-value Joint Significance of Pre-treatment Interactions	0.872	0.406	0.129	0.144	0.739	0.394	0.089	0.085	0.694

Notes: Robust Standard errors, clustered at the district level, in parenthesis. Each column restricts the sample to a subset of villages that held elections during the years shown in the column heading. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003. All regressions include village and year fixed effects. The post-treatment point estimates and standard errors are in bold to facilitate the reading of the table. *** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 3.B. The Effect of School Construction on Public Good Provision & Village Head Education by Calendar Year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent Variables:								
	Number of Health Posts			Garbage Disposal			Years of Education of the VH		
	Elections 1992-93	Elections 1994-96	Elections 1997-00	Elections 1992-93	Elections 1994-96	Elections 1997-00	Elections 1992-93	Elections 1994-96	Elections 1997-00
Year 1990 * Num INPRES				0.009	-0.008	0.004	-0.116	-0.020	-0.025
				(0.011)	(0.016)	(0.005)	(0.096)	(0.081)	(0.100)
Year 1993 * Num INPRES	**0.022**	0.006	0.043	**0.021**	-0.024	0.001	**-0.132**	-0.020	-0.025
	(0.066)	(0.108)	(0.032)	**(0.013)**	(0.022)	(0.007)	**(0.164)**	(0.081)	(0.100)
Year 1996 * Num INPRES				**0.025***	**-0.025**	0.000	**-0.141**	**0.400****	-0.020
				(0.015)	**(0.024)**	(0.007)	**(0.176)**	**(0.183)**	(0.099)
Year 2000 * Num INPRES	**0.159**	**0.063**	0.095	**0.032***	**-0.008**	**0.011**	**-0.143**	**0.352***	**0.079**
	(0.139)	**(0.169)**	(0.091)	**(0.018)**	**(0.029)**	**(0.009)**	**(0.169)**	**(0.208)**	**(0.102)**
Year 2003 * Num INPRES	**0.262***	**0.200**	**0.176****	**0.052*****	**-0.001**	**0.015**	**-0.301****	**0.479****	**0.097**
	(0.137)	**(0.169)**	**(0.087)**	**(0.019)**	**(0.030)**	**(0.010)**	**(0.146)**	**(0.198)**	**(0.106)**
Observations	4,250	3,506	30,076	6,492	5,406	45,828	6,492	5,406	45,828
R-squared	0.694	0.708	0.706	0.719	0.706	0.720	0.561	0.600	0.596
P-value Joint Significance of Pre-treatment Interactions	0.737	0.959	0.189	0.391	0.367	0.317	0.229	0.803	0.987

Notes: Robust Standard errors, clustered at the district level, in parenthesis. Each column restricts the sample to a subset of villages that held elections during the years shown in the column heading. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003. All regressions include village and year fixed effects. The post-treatment point estimates and standard errors are in bold to facilitate the reading of the table. *** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 4. The Effects of School Construction on Public Good Provision (Additional Outcomes)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variables:							
	Number of Health Posts				Garbage Disposal			
Dep. Var. Mean	*4.08*	*4.08*	*4.08*	*4.08*	*0.91*	*0.91*	*0.91*	*0.91*
Dep. Var. Mean at Baseline	*3.83*	*3.83*	*3.83*	*3.83*	*0.82*	*0.82*	*0.82*	*0.82*
post 1st Elec after 1992	-0.080	-0.083	-0.168**	-0.170**	0.003	0.003	-0.004	-0.005
	(0.072)	(0.073)	(0.083)	(0.084)	(0.004)	(0.004)	(0.006)	(0.006)
post * Num INPRES Schools§		0.111				0.013**		
		(0.082)				(0.006)		
post * Num INPRES > 0			0.127*				0.010	
			(0.066)				(0.009)	
post * INPRES schools =1				0.081*				0.003
				(0.048)				(0.009)
post * INPRES schools =2				0.228				0.027**
				(0.181)				(0.012)
Observations	38,748	38,748	38,748	38,748	59,130	59,130	59,130	59,130
R-squared	0.709	0.709	0.709	0.709	0.718	0.718	0.718	0.718
Number of Villages	9.703	9.703	9.703	9.703	9.855	9.855	9.855	9.855

Notes: Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for number of health posts which is not reported for years 1986 and 1996. § The number of INPRES schools is defined in deviations from its sample mean. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 5. Heterogeneous Effects of School Construction on Public Good Provision (Additional Outcomes)

	(1)	(2)	(3)	(4)
	Dependent Variables:			
	Number of Health Posts		Garbage Disposal	
	Interaction: Num INPRES Schools	Interaction: Dummy INPRES Schools >0	Interaction: Num INPRES Schools	Interaction: Dummy INPRES Schools >0
Dep. Var. Mean	*4.08*	*4.08*	*0.91*	*0.91*
post 1st Elec after 1992	-0.113	-0.206**	-0.074***	-0.074***
	(0.072)	(0.084)	(0.012)	(0.012)
post * measure of INPRES school intensity	0.118	0.140**	-0.000	-0.000
	(0.078)	(0.066)	(0.000)	(0.000)
post * bad baseline service	2.225***	2.735***	0.412***	0.408***
	(0.298)	(0.738)	(0.028)	(0.026)
post * bad baseline service * INPRES sch. intensity	-0.433	-0.754	-0.001	0.005
	(0.489)	(0.813)	(0.019)	(0.025)
Observations	38,748	38,748	59,130	59,130
R-squared	0.712	0.712	0.786	0.786

Notes: Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for number of health posts which is not reported for years 1986 and 1996. In the odd columns the measure of INPRES school intensity used corresponds to the number of INPRES schools in deviations from its sample mean. In even columns the intensity measure corresponds to a dummy for any INPRES schools constructed in the village. In columns 1 and 2 bad baseine service equals 1 for villages with no health post in year 1990. In columns 3 and 4 bad baseine service equals 1 for villages with no access to garbage bin disposal in year 1986. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 6. Robustness Checks of the Effects of School Construction on Public Goods (Additional Outcomes)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Dependent Variables:									
	Number of Health Posts					Garbage Disposal				
Dep. Var. Mean	*4.08*	*4.08*	*4.08*	*4.08*	*4.08*	*0.91*	*0.91*	*0.91*	*0.91*	*0.91*
post	-0.082	-0.087	-0.082	-0.087	-0.085	0.003	0.002	0.003	0.003	0.003
	(0.072)	(0.073)	(0.071)	(0.073)	(0.073)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
post * Num INPRES Schools[§]	0.111	0.249***	0.105	0.112	0.106	0.012**	-0.000	0.012**	0.012**	0.012*
	(0.082)	(0.087)	(0.081)	(0.081)	(0.085)	(0.006)	(0.004)	(0.006)	(0.005)	(0.006)
1 Year Before Election*Num INPRES sch					-0.012					0.001
					(0.120)					(0.006)
2 Year Before Election*Num INPRES sch					-0.064					-0.001
					(0.114)					(0.004)
3 Year Before Election*Num INPRES sch					-0.016					-0.005
					(0.134)					(0.005)
Additional Controls:										
Log Population	✓					✓				
Pre-treatment Dep Var* Year FE		✓					✓			
Pre-treatment Covariates* Year FE			✓					✓		
Enrolment rate*Year FE				✓					✓	
Water and Sanitation Program*Year FE				✓					✓	
Observations	38,748	38,748	38,748	38,732	38,748	59,130	59,130	59,130	59,106	59,130
R-squared	0.709	0.728	0.710	0.709	0.709	0.718	0.840	0.720	0.720	0.718
P-value Joint Significance of Pre-treatment Interactions					0.951					0.686

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts and 9,855 villages. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for number of health posts which is not reported for years 1986 and 1996. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects. Additional controls are included as shown in the table.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 7A. Robustness Checks of the Heterogeneous Effects of School Construction on Public Goods

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Dependent Variables:											
	Primary Health Center in the Village				Doctors in the Village				Access to Safe Drinking Water			
Dep. Var. Mean	*0.10*	*0.10*	*0.10*	*0.10*	*0.12*	*0.12*	*0.12*	*0.12*	*0.77*	*0.77*	*0.77*	*0.77*
post	-0.055***	0.000	-0.070***	-0.058***	-0.062***	-0.000	-0.090***	-0.067***	-0.044***	-0.000	-0.043***	-0.044***
	(0.005)	(0.000)	(0.006)	(0.005)	(0.005)	(0.000)	(0.006)	(0.005)	(0.006)	(.)	(0.006)	(0.006)
post * Num INPRES Schools§	0.000	0.000	0.000	0.001	0.000	0.000	-0.000	0.000	-0.000	0.000	-0.001	-0.001
	(0.001)	(0.000)	(0.002)	(0.001)	(0.001)	(0.000)	(0.003)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
post * bad baseline service	0.054***	-0.006**	0.070***	0.058***	0.068***	0.001	0.098***	0.074***	0.160***	-0.002	0.154***	0.160***
	(0.004)	(0.003)	(0.005)	(0.004)	(0.005)	(0.003)	(0.006)	(0.005)	(0.019)	(0.016)	(0.018)	(0.019)
post * bad baseline service * INPRES sch. intensity	0.014***	0.014***	0.015***	0.014***	0.007*	0.007*	0.008*	0.008**	0.057**	0.056**	0.058**	0.057**
	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.023)	(0.024)	(0.023)	(0.023)
Additional Controls:												
Log Population	✓				✓				✓			
Pre-treatment Dep Var* Year FE		✓				✓				✓		
Pre-treatment Covariates* Year FE			✓				✓				✓	
Enrolment rate* Post				✓				✓				✓
Water and Sanitation Program* Post				✓				✓				✓
Observations	59,130	59,130	59,130	59,106	59,130	59,130	59,130	59,106	39,420	39,420	39,420	39,404
R-squared	0.837	0.838	0.838	0.837	0.821	0.821	0.823	0.821	0.924	0.933	0.924	0.924

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts and 9,855 villages. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003. § The number of INPRES schools is defined in deviations from its sample mean. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects. Additional controls are included as shown in the table.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 7B. Robustness Checks of the Heterogeneous Effects of School Construction on Public Goods (Additional Outcomes)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variables:							
	Number of Health Posts				Garbage Disposal			
Dep. Var. Mean	*4.08*	*4.08*	*4.08*	*4.08*	*0.91*	*0.91*	*0.91*	*0.91*
post	-0.112	-0.101	-0.112	-0.117	-0.074***	-0.000	-0.074***	-0.074***
	(0.071)	(0.073)	(0.070)	(0.072)	(0.012)	(0.000)	(0.012)	(0.012)
post * Num INPRES Schools§	0.118	0.251***	0.112	0.120	-0.000	0.000	-0.000	0.000
	(0.078)	(0.084)	(0.077)	(0.079)	(0.000)	(0.000)	(0.000)	(0.000)
post * bad baseline service	2.229***	1.257***	2.206***	2.210***	0.412***	0.011	0.410***	0.410***
	(0.298)	(0.284)	(0.295)	(0.297)	(0.028)	(0.019)	(0.028)	(0.028)
post * bad baseline service *Num INPRES Schools§	-0.433	-0.566	-0.422	-0.438	-0.000	-0.002	-0.000	-0.001
	(0.488)	(0.488)	(0.486)	(0.484)	(0.019)	(0.019)	(0.019)	(0.019)
Additional Controls:								
Log Population	✓				✓			
Pre-treatment Dep Var* Year FE		✓				✓		
Pre-treatment Covariates* Year FE			✓				✓	
Enrolment rate* Post				✓				✓
Water and Sanitation Program* Post				✓				✓
Observations	38,748	38,748	38,748	38,732	59,130	59,130	59,130	59,106
R-squared	0.712	0.729	0.713	0.712	0.786	0.840	0.786	0.786

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts and 9,855 villages. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for number of health posts which is not reported for years 1986 and 1996. § The number of INPRES schools is defined in deviations from its sample mean. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects. Additional controls are included as shown in the table.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 8. Robustness to Controlling for Development Programs and for the Impact of the Economic Crises

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	Dependent Variables:														
	Primary Health Center in the Village					Doctors in the Village					Access to Safe Drinking Water				
Dep. Var. Mean	*0.10*	*0.10*	*0.10*	*0.10*	*0.10*	*0.12*	*0.12*	*0.12*	*0.12*	*0.12*	*0.77*	*0.77*	*0.77*	*0.77*	*0.77*
post	-0.016***	-0.015***	-0.015***	-0.016***	-0.014***	-0.006	-0.005	-0.006	-0.006	-0.005	-0.017**	-0.017**	-0.017**	-0.017**	-0.017**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
post * Num INPRES Schools[§]	0.017***	0.016***	0.015***	0.017***	0.014***	0.010**	0.009*	0.009*	0.010**	0.008*	0.019**	0.019**	0.018**	0.019**	0.018**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Additional Controls:															
Share of wetland * Year FE	✓					✓					✓				
IDT program * Year FE		✓					✓					✓			
Crises Mitigation Programs*Year FE			✓					✓					✓		
Transfers from Upper Govs*Year FE				✓					✓					✓	
All of the above controls * Year FE					✓					✓					✓
Observations	59,130	59,130	59,130	59,094	59,094	59,130	59,130	59,130	59,094	59,094	39,420	39,420	39,420	39,396	39,396
R-squared	0.837	0.838	0.837	0.837	0.838	0.820	0.822	0.820	0.820	0.822	0.922	0.922	0.923	0.922	0.923

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts and 9,855 villages. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects. Additional controls are included as shown in the table.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 9. Robustness to Dropping Villages that Held Elections after 1998

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent Variables:								
	Health Facility in the Village			Doctors in the Village			Access to Safe Drinking Water		
	Baseline Result	Drop if elections in 1999-2000	Drop if elections in 1998-2000	Baseline Result	Drop if elections in 1999-2000	Drop if elections in 1998-2000	Baseline Result	Drop if elections in 1999-2000	Drop if elections in 1998-2000
Dep. Var. Mean	*0.0993*	*0.100*	*0.103*	*0.123*	*0.123*	*0.129*	*0.774*	*0.766*	*0.775*
post 1st Elec after 1992	-0.016***	-0.017***	-0.013**	-0.006	-0.010	-0.003	-0.017**	-0.019***	-0.021***
	(0.004)	(0.005)	(0.006)	(0.005)	(0.006)	(0.008)	(0.007)	(0.007)	(0.007)
post * Num INPRES > 0	0.017***	0.021***	0.018**	0.010*	0.017**	0.008	0.019**	0.019**	0.019**
	(0.004)	(0.005)	(0.007)	(0.005)	(0.007)	(0.010)	(0.009)	(0.009)	(0.009)
Observations	59,130	36,438	19,362	59,130	36,438	19,362	39,420	24,292	12,908
R-squared	0.837	0.828	0.827	0.820	0.818	0.813	0.922	0.922	0.921
Number of Villages	9855	6073	3227	9855	6073	3227	9855	6073	3227

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 10. Robustness Checks of the Effects of School Construction on Public Goods.
(Controlling for Covariates interacted with Post Dummy)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	Primary Health Center in the Villag			Doctors in the Village			Access to Safe Drinking Water			Number of Health Posts			Garbage Disposal		
Dep. Var. Mean	*0.10*	*0.10*	*0.10*	*0.12*	*0.12*	*0.12*	*0.77*	*0.77*	*0.77*	*4.08*	*4.08*	*4.08*	*0.91*	*0.91*	*0.91*
post	-0.001	-0.010***	-0.035	0.006*	-0.014***	-0.095***	0.123***	0.002	0.004	0.961***	-0.005	0.094	0.337***	0.018***	0.157**
	(0.003)	(0.003)	(0.021)	(0.003)	(0.004)	(0.018)	(0.018)	(0.006)	(0.040)	(0.144)	(0.081)	(0.368)	(0.029)	(0.006)	(0.076)
post * Num INPRES Schools[§]	0.014***	0.012***	0.012***	0.007*	0.006*	0.006*	0.013**	0.017**	0.017**	0.249***	0.105	0.113	-0.000	0.012**	0.012**
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.006)	(0.007)	(0.007)	(0.087)	(0.081)	(0.081)	(0.004)	(0.006)	(0.005)
Additional Controls:															
Pre-treatment Dep Var* Post	✓			✓			✓			✓			✓		
Pre-treatment Covariates* Post		✓			✓			✓			✓			✓	
Enrolment rate* Post			✓			✓			✓			✓			✓
Water and Sanitation Program* Post			✓			✓			✓			✓			✓
Observations	59,130	59,130	59,106	59,130	59,130	59,106	39,420	39,420	39,404	38,748	38,748	38,732	59,130	59,130	59,106
R-squared	0.837	0.837	0.837	0.821	0.821	0.820	0.924	0.923	0.923	0.722	0.710	0.709	0.786	0.720	0.720

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts and 9,855 villages. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects. Additional controls are included as shown in the table.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 11.A. Robustness to Actual Election Timing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Dependent Variables:											
	Primary Health Center in the Village				Doctors in the Village				Access to Safe Drinking Water			
Dep. Var. Mean	*0.10*	*0.10*	*0.10*	*0.10*	*0.12*	*0.12*	*0.12*	*0.12*	*0.77*	*0.77*	*0.77*	*0.77*
Dep. Var. Mean at Baseline	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.74*	*0.74*	*0.74*	*0.74*
post 1st Elec after 1992	-0.005*	-0.005*	-0.016***	-0.016***	-0.000	-0.000	-0.007	-0.007	-0.004	-0.005	-0.013	-0.013
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.005)	(0.005)	(0.006)	(0.005)	(0.010)	(0.010)
post * Num INPRES Schools§		0.012***				0.006*				0.012		
		(0.003)				(0.003)				(0.008)		
post * Num INPRES > 0			0.017***				0.010**				0.013	
			(0.004)				(0.005)				(0.011)	
post * INPRES schools =1				0.013***				0.010**				0.006
				(0.004)				(0.005)				(0.010)
post * INPRES schools =2				0.024***				0.011				0.024
				(0.006)				(0.007)				(0.016)
Observations	59,130	59,130	59,130	59,130	59,130	59,130	59,130	59,130	39,420	39,420	39,420	39,420
R-squared	0.836	0.837	0.837	0.837	0.820	0.820	0.820	0.820	0.922	0.922	0.922	0.922

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts and 9,855 villages. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003. § The number of INPRES schools is defined in deviations from its sample mean. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects, year fixed effects and province fixed effects interacted with year fixed effects.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 11.B. Robustness to Actual Election Timing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Dependent Variables:											
	Number of Health Posts				Garbage Disposal				Years of Education of the VH			
Dep. Var. Mean	*4.08*	*4.08*	*4.08*	*4.08*	*0.91*	*0.91*	*0.91*	*0.91*	*9.87*	*9.87*	*9.87*	*9.87*
Dep. Var. Mean at Baseline	*3.83*	*3.83*	*3.83*	*3.83*	*0.82*	*0.82*	*0.82*	*0.82*	*7.71*	*7.71*	*7.71*	*7.71*
post 1st Elec after 1992	-0.064	-0.066	-0.160**	-0.162**	0.000	-0.000	-0.007	-0.007	0.397***	0.394***	0.321***	0.319***
	(0.065)	(0.066)	(0.074)	(0.074)	(0.003)	(0.004)	(0.007)	(0.007)	(0.078)	(0.076)	(0.082)	(0.082)
post * Num INPRES Schools[§]		0.121				0.012**				0.110*		
		(0.082)				(0.006)				(0.058)		
post * Num INPRES > 0			0.140**				0.010				0.109	
			(0.065)				(0.009)				(0.076)	
post * INPRES schools =1				0.091*				0.003				0.051
				(0.046)				(0.009)				(0.075)
post * INPRES schools =2				0.248				0.026**				0.232*
				(0.181)				(0.012)				(0.121)
Observations	38,748	38,748	38,748	38,748	59,130	59,130	59,130	59,130	59,130	59,130	59,130	59,130
R-squared	0.709	0.709	0.709	0.709	0.718	0.718	0.718	0.718	0.583	0.583	0.583	0.583

Notes: Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for number of health posts which is not reported for years 1986 and 1996. § The number of INPRES schools is defined in deviations from its sample mean. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 12. Additional Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variables:							
	Electricity / Kerosene for cooking	Share of Irrigated Agricultural Land	Motor Transportation	Public Toilet	Number of Kindergarten	Number of Primary Schools	Number of High School	Asphalt Road
Dep. Var. Mean	*0.135*	*0.458*	*0.687*	*0.0664*	*1.225*	*3.127*	*0.409*	*0.543*
Dep. Var. Mean at Baseline	*0.0475*	*0.458*	*0.562*	*0.0531*	*0.887*	*2.895*	*0.289*	*0.266*
post 1st Elec after 1992	0.000	0.021	0.018*	-0.007**	-0.070***	-0.048***	-0.021***	0.009
	(0.013)	(0.016)	(0.010)	(0.003)	(0.015)	(0.013)	(0.007)	(0.010)
post * INPRES schools =1	-0.001	-0.021	-0.024**	0.005**	0.064***	0.051***	0.023**	-0.001
	(0.016)	(0.014)	(0.011)	(0.002)	(0.018)	(0.014)	(0.010)	(0.010)
post * INPRES schools =2	0.008	-0.011	-0.025	0.008*	0.109***	0.150***	0.065***	-0.015
	(0.021)	(0.009)	(0.015)	(0.005)	(0.027)	(0.023)	(0.012)	(0.014)
Observations	59,130	58,458	59,130	59,130	59,130	59,130	59,130	59,125
R-squared	0.673	0.238	0.831	0.915	0.891	0.957	0.915	0.755

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts and 9,855 villages. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 13. Summary Statistics on Candidates of Village Elections

	(1) Observations	(2) Mean	(3) Std. Dev.	(4) Years Included
	A. Data on Baseline Data			
Years of Education of the Village Head	59,130	9.87	3.13	1986-2003
Years of Education 25th percentile	59,131	6	-	1986-2003
Years of Education 50th percentile	59,132	9	-	1986-2003
Years of Education 75th percentile	59,133	12	-	1986-2003
Age of the Village Head	59,105	42.95	9.23	1986-2003
Age 25th percentile	59,130	36	-	1986-2003
Age 50th percentile	59,130	42	-	1986-2003
Age 75th percentile	59,130	49	-	1986-2003
	B. Data on Candidate Compostion of Village Head Elections			
Number of Contestants per village	38	4.16	1.33	2009-2015
Years of Education of the Village Head	38	12	1.84	2009-2015
Years of Education 25th percentile	38	12	-	2009-2015
Years of Education 50th percentile	38	12	-	2009-2015
Years of Education 75th percentile	38	12	-	2009-2015
Age of the Village Head	38	42.82	7.60	2009-2015
Age 25th percentile	38	39	-	2009-2015
Age 50th percentile	38	43	-	2009-2015
Age 75th percentile	38	47	-	2009-2015
Years of Education of All Candidates	155	12.43	2.15	2009-2015
Age of of All Candidates	156	42.78	8.74	2009-2015
Age 25th percentile	156	37	-	2009-2015
Age 50th percentile	156	43	-	2009-2015
Age 75th percentile	156	48	-	2009-2015
Years of Education of Youngest Candidate	38	12.74	2.13	2009-2015
Age of Youngest Candidate	38	33.82	5.82	2009-2015
Age 25th percentile	38	30	-	2009-2015
Age 50th percentile	38	32	-	2009-2015
Age 75th percentile	38	39	-	2009-2015

Notes : Panel A reproduces summary statistics of the main dataset used in the paper. Panel B provides summary statistics for data collected through online searches on characteristics of candidates of village elections for 38 villages.

Appendix Table 14. The Effects of School Construction on Village Head Age

	(1)	(2)	(3)	(4)
	Dependent Variable: Age of the Village Head			
Dep. Var. Mean	*42.95*	*42.95*	*42.95*	*42.95*
Dep. Var. Mean at Baseline (year 1986)	*48.46*	*48.46*	*48.46*	*48.46*
post 1st Election after 1992	-3.161***	-3.167***	-3.363***	-3.362***
	(0.296)	(0.296)	(0.306)	(0.306)
post 1st Election after 1992*Num INPRES schools§		0.133		
		(0.170)		
post 1st Election after 1992*INPRES schools >0			0.290	
			(0.235)	
post 1st Election after 1992*INPRES schools =1				0.318
				(0.255)
post 1st Election after 1992*INPRES schools =2				0.228
				(0.355)
Observations	59,105	59,105	59,105	59,105
R-squared	0.458	0.458	0.458	0.458
P-value Joint Signif. Pre-treatment Interactions				

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts. The unit of observation is the village-year level. The sample includes 9,855 villages. The dependent variable is the numer age of the village head in office in the corresponding village. § The number of INPRES schools is defined in deviations from its sample mean. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003. All regressions control for village fixed effects and year fixed effects. *** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 15. The Effects of School Construction on Village Head Gender

	(1)	(2)	(3)	(4)
	Dependent Variable: Dummy for Male Village Head			
Dep. Var. Mean	*0.97*	*0.97*	*0.97*	*0.97*
Dep. Var. Mean at Baseline (year 1986)	*0.99*	*0.99*	*0.99*	*0.99*
post 1st Election after 1992	0.003	0.003	0.004	0.004
	(0.004)	(0.004)	(0.004)	(0.004)
post 1st Election after 1992*Num INPRES schools§		-0.003		
		(0.003)		
post 1st Election after 1992*INPRES schools >0			-0.001	
			(0.004)	
post 1st Election after 1992*INPRES schools =1				0.001
				(0.004)
post 1st Election after 1992*INPRES schools =2				-0.006
				(0.006)
Observations	59,122	59,122	59,122	59,122
R-squared	0.447	0.447	0.447	0.447
P-value Joint Signif. Pre-treatment Interactions				

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts. The unit of observation is the village-year level. The sample includes 9,855 villages. The dependent variable is a dummy that takes value one if the village head is a male. § The number of INPRES schools is defined in deviations from its sample mean. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003. All regressions control for village fixed effects and year fixed effects. *** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 16. Robustness Checks to Controlling for the Average Education of the Electorate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Dependent Variable:									
	Primary Health Center		Doctors in the Village		Access to Safe Drinking Water		Number of Health Posts		Garbage Bin Disposal	
	Baseline Result (common sample)	Controlling for Education of the Electorate	Baseline Result (common sample)	Controlling for Education of the Electorate	Baseline Result (common sample)	Controlling for Education of the Electorate	Baseline Result (common sample)	Controlling for Education of the Electorate	Baseline Result (common sample)	Controlling for Education of the Electorate
Dep. Var. Mean	*0.10*	*0.10*	*0.12*	*0.12*	*0.79*	*0.79*	*3.95*	*3.95*	*0.91*	*0.91*
post 1st Elec after 1992	-0.014***	-0.014***	-0.005	-0.005	-0.020***	-0.020***	-0.104	-0.104	-0.002	-0.002
	(0.004)	(0.004)	(0.005)	(0.005)	(0.007)	(0.007)	(0.091)	(0.091)	(0.006)	(0.006)
post * INPRES schools =1	0.012**	0.012**	0.009*	0.009*	0.015*	0.014*	0.067	0.067	0.001	0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.008)	(0.008)	(0.058)	(0.058)	(0.009)	(0.009)
post * INPRES schools =2	0.023***	0.023***	0.010	0.010	0.034**	0.034**	0.259	0.259	0.023*	0.023*
	(0.007)	(0.007)	(0.008)	(0.007)	(0.017)	(0.017)	(0.196)	(0.197)	(0.012)	(0.012)
Average Education of the Electorate		0.002		0.022***		-0.014		0.001		-0.005
		(0.005)		(0.008)		(0.019)		(0.207)		(0.022)
Observations	49,662	49,662	49,662	49,662	33,108	33,108	32,604	32,604	49,662	49,662
R-squared	0.838	0.838	0.819	0.820	0.923	0.923	0.691	0.691	0.710	0.710

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts and 9,855 villages. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 17.A. The Effect of Village Head Education on the Village Budget (Revenues)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent Variables:						
	Log Total Income	Log Surplus from last year	Log Income from Village Sources	Log Transfer from Central Government	Log Transfer from Provincial Government	Log Transfer from District Government	Log Other Income
Dep. Var. Mean (not logged)	*39669*	*252*	*25444*	*6480*	*729*	*749*	*6014*
	Panel A. OLS						
years of educ VH	0.020***	0.062***	0.029***	0.015	-0.083***	-0.013	0.066**
	(0.004)	(0.018)	(0.006)	(0.011)	(0.026)	(0.029)	(0.027)
	Panel B. 2SLS						
years of educ VH	0.009	-0.199	-0.047	-0.063	-0.276	-0.050	0.048
	(0.035)	(0.178)	(0.074)	(0.126)	(0.284)	(0.249)	(0.224)
Observations	9,855	9,855	9,855	9,855	9,855	9,855	9855
R-squared (OLS)	0.151	0.089	0.067	0.010	0.241	0.096	0.030
Cragg-Donald F-Stat (2SLS)	13.17	13.17	13.17	13.17	13.17	13.17	13.17

Notes: Robust Standard errors, clustered at the district level, in parenthesis. Each column corresponds to a cross-sectional regression for the year 1996, when village government budget information is available. The unit of observation is the village. The dependent variable is defined by the column headings. All regressions include province fixed effects, a quartic on log population and a quartic on the percentage of rural households as controls. Panel A shows the OLS results while Panel B shows the 2SLS results. In Panel B, a dummy for having had elections between 1992 and 1996 is used as an instrument for the years of education of the village head. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 17.B. The Effect of Village Head Education on the Village Budget (Expenditures)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variables:							
	Log Total Expenditures	Log Employee Expenses	Log Expenses in Goods	Log Expenses in Maintenance	Log Infraestructure Expenses	Log Production Facilities Expenses	Log Transportation Facilities Expenses	Log Social Facilities Expenses
Dep. Var. Mean (not logged)	*39594*	*10146*	*877*	*622*	*2799*	*2491*	*6982*	*6302*
	Panel A. OLS							
years of educ VH	0.020***	0.034***	0.010	0.054***	0.067**	0.075***	0.064***	0.049
	(0.004)	(0.005)	(0.007)	(0.014)	(0.031)	(0.025)	(0.024)	(0.031)
	Panel B. 2SLS							
years of educ VH	0.010	0.043	0.127*	0.030	0.644**	0.455*	-0.111	0.521**
	(0.034)	(0.052)	(0.065)	(0.114)	(0.323)	(0.260)	(0.243)	(0.235)
Observations	9,855	9,855	9,855	9,855	9,855	9,855	9,855	9,855
R-squared (OLS)	0.151	0.096	0.034	0.018	0.027	0.046	0.026	0.025
Cragg-Donald F-Stat (2SLS)	13.17	13.17	13.17	13.17	13.17	13.17	13.17	13.17

Notes: Robust Standard errors, clustered at the district level, in parenthesis. Each column corresponds to a cross-sectional regression for the year 1996, when village government budget information is available. The unit of observation is the village. The dependent variable is defined by the column headings. All regressions include province fixed effects, a quartic on log population and a quartic on the percentage of rural households as controls. Panel A shows the OLS results while Panel B shows the 2SLS results. In Panel B, a dummy for having had elections between 1992 and 1996 is used as an instrument for the years of education of the village head. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 18.A. Effects of Village Head Education on Public Good Provision (OLS and 2SLS Results)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Dependent Variables:											
	Primary Health Center in the Village				Doctors in the Village				Access to Safe Drinking Water			
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Dep. Var. Mean	*0.10*	*0.10*	*0.10*	*0.10*	*0.12*	*0.12*	*0.12*	*0.12*	*0.77*	*0.77*	*0.77*	*0.77*
Dep. Var. Mean at Baseline	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.74*	*0.74*	*0.74*	*0.74*
years of education VH	-0.000	0.109*	-0.005***	0.078	0.000	0.053	-0.007***	0.023	-0.000	0.178	-0.006***	0.081
	(0.000)	(0.056)	(0.001)	(0.050)	(0.000)	(0.038)	(0.001)	(0.035)	(0.001)	(0.216)	(0.001)	(0.175)
yrs. educ * bad baseline service			0.005***	0.036***			0.008***	0.042***			0.021***	0.216*
			(0.001)	(0.006)			(0.001)	(0.004)			(0.002)	(0.120)
Observations	59,130	59,130	59,130	59,130	59,130	59,130	59,130	59,130	39,420	39,420	39,420	39,420
R-squared	0.836		0.837		0.820		0.820		0.922		0.924	
Cragg-Donald F-Stat		4.519		1.865		4.519		2.053		0.774		0.360

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts and 9,855 villages. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003. The number of observations varies because of missing values in the dependent variables. In columns 2, 6, and 10 the instrumental variables corresponds to the interaction of the post 1st election after 1992 dummy with the number of INPRES schools in the village. In columns 4, 8, and 12, the set of instrumental variables also includes the post dummy interacted with bad service at baseline and the triple interaction with the number of INPRES schools. All regressions control for village fixed effects and year fixed effects. The 2SLS estimates also include the post 1st election after 1992 dummy as a covariate.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 18.B. The Effect of Village Head Education on Public Good Provision
(OLS and 2SLS Results)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variables:							
	Number of Health Posts				Garbage Disposal			
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Dep. Var. Mean	*4.08*	*4.08*	*4.08*	*4.08*	*0.91*	*0.908*	*0.91*	*0.91*
Dep. Var. Mean at Baseline	*3.83*	*3.83*	*3.83*	*3.83*	*0.82*	*0.82*	*0.82*	*0.82*
years of education VH	0.013*	0.717	0.010	0.664	0.002**	0.113**	-0.009***	-0.137*
	(0.007)	(0.473)	(0.007)	(0.474)	(0.001)	(0.055)	(0.001)	(0.077)
yrs. educ * bad baseline service			0.212**	1.717***			0.058***	0.237***
			(0.092)	(0.564)			(0.005)	(0.018)
Observations	38,748	38,748	38,748	38,748	59,130	59,130	59,130	59,130
R-squared	0.709		0.709		0.718		0.751	
Cragg-Donald F-Stat		8.434		2.906		4.519		1.659

Notes: Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts and 9,855 villages. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for number of health posts which is not reported for years 1986 and 1996. The number of observations varies because of missing values in the dependent variables. In columns 2, and 6 the instrumental variables corresponds to the interaction of the post 1st election after 1992 dummy with the number of INPRES schools in the village. In columns 4 and 8 the set of instrumental variables also includes the post dummy interacted with bad service at baseline and the triple interaction with the number of INPRES schools. All regressions control for village fixed effects and year fixed effects. The 2SLS estimates also include the post 1st election after 1992 dummy as a covariate.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 19.A. Robustness Checks to Controlling for Age of the Village Head (2SLS Results)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent Variables:								
	Primary Health Center in the Village			Doctors in the Village			Access to Safe Drinking Water		
	Baseline Result	Age Exogenous	Age & Interaction Endogenous	Baseline Result	Age Exogenous	Age & Interaction Endogenous	Baseline Result	Age Exogenous	Age & Interaction Endogenous
Dep. Var. Mean	*0.10*	*0.10*	*0.10*	*0.12*	*0.12*	*0.12*	*0.77*	*0.77*	*0.77*
years of education VH	0.079	0.061*	-0.020**	0.022	0.013	-0.027	0.082	0.064	-0.103**
	(0.050)	(0.033)	(0.010)	(0.035)	(0.027)	(0.024)	(0.178)	(0.140)	(0.043)
yrs. educ * bad baseline service	0.036***	0.038***	0.029***	0.042***	0.045***	0.035	0.217*	0.204**	0.274**
	(0.006)	(0.006)	(0.010)	(0.004)	(0.004)	(0.026)	(0.123)	(0.095)	(0.110)
age of the VH		0.014***	0.008		0.008*	0.016		0.018	-0.019
		(0.005)	(0.015)		(0.004)	(0.050)		(0.025)	(0.014)
age of the VH * bad baseline service			-0.007			-0.014			0.054
			(0.016)			(0.053)			(0.041)
Observations	59,105	59,105	59,105	59,105	59,105	59,105	39,396	39,396	39,396
Number of villages	9,855	9,855	9,855	9,855	9,855	9,855	9,855	9,855	9,855
Cragg-Donald F-Stat	1.866	3.521	0.210	2.038	3.303	0.188	0.354	0.570	1.141

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003. The number of observations varies because of missing values in the dependent variables. In columns 1, 2, 4, 5, 7, and 8 the instrumental variables corresponds to the interaction of the post 1st election after 1992 dummy with the number of INPRES schools, the post dummy interacted with bad service at baseline and the triple interaction of the three variables. Columns 3, 6, and 9 also include as instrument the average age and education of village heads in neighboring villages. All regressions control for village fixed effects, year fixed effects and the post 1st election after 1992 dummy.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 19.B. Robustness Checks to Controlling for Age of the Village Head (2SLS Results)

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variables:					
	Number of Health Posts			Garbage Disposal		
	Baseline Result	Age Exogenous	Age & Interaction Endogenous	Baseline Result	Age Exogenous	Age & Interaction Endogenous
Dep. Var. Mean	*4.08*	*4.08*	*4.08*	*0.91*	*0.91*	*0.91*
years of education VH	0.662	0.701	0.252	-0.134*	-0.118*	-0.041**
	(0.475)	(0.618)	(0.228)	(0.077)	(0.063)	(0.020)
yrs. educ * bad baseline service	1.717***	1.727***	3.788	0.236***	0.230***	0.130
	(0.564)	(0.568)	(10.108)	(0.018)	(0.016)	(0.097)
age of the VH		0.094	-0.043		-0.011	0.017
		(0.084)	(0.233)		(0.009)	(0.014)
age of the VH * bad baseline service			-1.762			-0.112
			(8.046)			(0.111)
Observations	38,733	38,733	38,733	59,105	59,105	59,105
Number of villages	9,703	9,703	9,703	9,855	9,855	9,855
Cragg-Donald F-Stat	2.891	2.686	0.0937	1.664	3.435	0.545

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for number of health posts which is not reported for years 1986 and 1996. The number of observations varies because of missing values in the dependent variables. In columns 1, 2, 4, and 5, the instrumental variables corresponds to the post 1st election after 1992 dummy interacted with the number of INPRES schools, the post dummy interacted with bad service at baseline and the triple interaction of the three variables. Columns 3 and 6 also include as instrument the average age and education of village heads in neighboring villages. All regressions control for village fixed effects, year fixed effects, and the post 1st election after 1992 dummy.*** p<0.01, ** p<0.05, *p<0.1

Appendix Table 20. First Stage for the Specification with Heterogenous Effects by Quality of Public Goods at Baseline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Public Good considered by the Interaction terms:									
	Primary Health Center		Doctors in the Village		Access to Safe Drinking Water		Number of Health Posts		Garbage Bin Disposal	
	Dependent Variables:									
	educ VH	educ VH * bad baseline service	educ VH	educ VH * bad baseline service	educ VH	educ VH * bad baseline service	educ VH	educ VH * bad baseline service	educ VH	educ VH * bad baseline service
Dep. Var. Mean	*0.10*	*0.10*	*0.12*	*0.12*	*0.77*	*0.77*	*4.08*	*4.08*	*0.91*	*0.91*
post 1st Elec after 1992	0.416***	-1.102***	0.395***	-1.136***	0.392***	-0.285***	0.838***	-0.000	0.361***	-0.249***
	(0.151)	(0.076)	(0.145)	(0.077)	(0.146)	(0.053)	(0.101)	(0.005)	(0.087)	(0.048)
post * Num INPRES schools[§]	0.243	-0.005	0.243	-0.001	0.105	-0.000	0.154**	0.000	0.077	-0.001
	(0.182)	(0.007)	(0.161)	(0.007)	(0.150)	(0.002)	(0.063)	(0.000)	(0.056)	(0.001)
post * bad baseline service	0.011	1.644***	0.036	1.650***	-0.521**	0.958***	0.074	1.230***	0.352***	1.929***
	(0.125)	(0.070)	(0.118)	(0.071)	(0.235)	(0.239)	(0.323)	(0.324)	(0.129)	(0.139)
post * bad baseline service * Num INPRES[§]	-0.142	0.106*	-0.141	0.103*	0.036	0.149	0.052	0.205	0.113	0.201*
	(0.187)	(0.058)	(0.170)	(0.060)	(0.307)	(0.261)	(0.466)	(0.456)	(0.107)	(0.105)
Observations	59,130	59,130	59,130	59,130	39,420	39,420	38,748	38,748	59,130	59,130
R-squared	0.583	0.756	0.583	0.753	0.681	0.943	0.654	0.970	0.584	0.942

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts and 9,855 villages. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003. The number of observations varies because of missing values in the dependent variables. All regressions control for village fixed effects and year fixed effects.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 21. Effects of Village Head Education on Public Good Provision (2SLS, Alternative Instrumental Variables)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Dependent Variables:									
	Primary Health Center		Doctors in the Village		Safe Drinking Water		Number of Health Posts		Garbage Disposal	
Dep. Var. Mean	*0.10*	*0.10*	*0.12*	*0.12*	*0.77*	*0.77*	*4.08*	*4.08*	*0.91*	*0.91*
years of education VH	0.013*	-0.016**	0.012	-0.023***	0.002	0.031	0.058	0.021	0.028**	-0.068***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.017)	(0.053)	(0.107)	(0.105)	(0.011)	(0.021)
yrs. educ * bad baseline service		0.033***		0.042***		0.191***		1.757***		0.223***
		(0.003)		(0.004)		(0.072)		(0.455)		(0.017)
Observations	59,130	59,130	59,130	59,130	39,420	39,420	38,748	38,748	59,130	59,130
Number of villages	9,855	9,855	9,855	9,855	9,855	9,855	9,703	9,703	9,855	9,855
Cragg-Donald F-Stat	14.73	7.494	14.73	7.624	2.967	0.819	47.98	24.12	14.73	7.699

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003 and number of health posts which is not reported in years 1986 and 1996. The number of observations varies because of missing values in the dependent variables. In the odd columns the instrumental variables corresponds to the post 1st election after 1992 dummy and its interaction of with the number of INPRES schools in the village. In the even columns the set of instrumental variables also includes the post dummy interacted with bad service at baseline and the triple interaction with the number of INPRES schools. All regressions control for village fixed effects and year fixed effects.*** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 22. Effect of Village Head Education on Public Good Provision
(OLS and 2SLS Results, Alternative Demand Predictors)

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variables:					
	Primary Health Center		Doctors in the Village		Number of Health Posts	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Dep. Var. Mean	*0.10*	*0.10*	*0.12*	*0.12*	*4.08*	*4.08*
years of education VH	-0.001***	0.049*	-0.001*	0.002	0.004	0.389
	(0.000)	(0.026)	(0.001)	(0.021)	(0.008)	(0.403)
yrs. educ * high mortality at baseline	0.002***	0.024***	0.003***	0.018***	0.020	0.151
	(0.001)	(0.005)	(0.001)	(0.005)	(0.013)	(0.095)
Observations	59,130	59,130	59,130	59,130	38,748	38,748
Cragg-Donald F-Stat		3.229		3.229		4.724

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for number of health posts which is not reported in years 1986 and 1996. The number of observations varies because of missing values in the dependent variables. In columns 2, 4, and 6 the instrumental variables corresponds to interaction of the post 1st election after 1992 dummy with the number of INPRES schools, the post dummy interacted with high mortality at baseline at baseline and the triple interaction of the three variables. All regressions control for village fixed effects and year fixed effects. Columns 2, 4, and 6 also add the post 1st election after 1992 dummy as a covariate. *** p<0.01, ** p<0.05, *p<0.1.

Appendix Table 23. Effect of Village Head Education on Public Good Provision (2SLS, Robustness Checks)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	Dependent Variables:														
	Primary Health Center			Doctors in the Village			Access to Safe Drinking Water			Number of Health Posts			Garbage Disposal		
Dep. Var. Mean	0.10	*0.10*	*0.10*	*0.12*	*0.12*	*0.12*	*0.77*	*0.77*	*0.77*	*4.08*	*4.08*	*4.08*	*0.91*	*0.91*	*0.91*
years of education VH	0.078	0.083	0.075	0.022	0.030	0.016	0.081	0.070	0.082	0.667	0.753	0.638	-0.137*	-0.151*	-0.138*
	(0.049)	(0.053)	(0.051)	(0.034)	(0.039)	(0.038)	(0.173)	(0.159)	(0.179)	(0.472)	(0.537)	(0.475)	(0.077)	(0.085)	(0.078)
yrs. educ * bad baseline service	0.036***	0.037***	0.044***	0.042***	0.045***	0.059***	0.215*	0.214*	0.219*	1.720***	1.718***	1.708***	0.237***	0.239***	0.237***
	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)	(0.119)	(0.115)	(0.125)	(0.565)	(0.579)	(0.556)	(0.018)	(0.018)	(0.018)
Additional Controls:															
Log Population	✓			✓			✓			✓			✓		
Enrolment rate* Year FE		✓			✓			✓			✓			✓	
Water and Sanitation Program* Year FE		✓			✓			✓			✓			✓	
Pre-treatment Covariates* Year FE			✓			✓			✓			✓			✓
Observations	59,130	59,106	59,130	59,130	59,106	59,130	39,420	39,404	39,420	38,748	38,732	38,748	59,130	59,106	59,130
Number of villages	9,855	9,851	9,855	9,855	9,851	9,855	9,855	9,851	9,855	9,703	9,699	9,703	9,855	9,851	9,855
Cragg-Donald F-Stat	1.868	1.699	1.875	2.055	1.868	2.050	0.363	0.397	0.354	2.913	2.487	2.836	1.657	1.455	1.664

Notes : Robust Standard errors, clustered at the district level, in parenthesis. The sample includes 82 districts. The unit of observation is the village-year level. The dependent variable is defined by the column headings. The years included in all regressions are 1986, 1990, 1993, 1996, 2000 and 2003, except for access to safe water which is not reported for years 2000 and 2003 and number of health posts which is not reported in years 1986 and 1996. The number of observations varies because of missing values in the dependent variables. The instrumental variables corresponds to the interaction of the post 1st election after 1992 dummy with the number of INPRES schools in the village, the post dummy interacted with bad service at baseline and the triple interaction of the three variables. All regressions control for village fixed effects, year fixed effects and the post 1st election after 1992 dummy. Additional controls are included as shown in the table. *** p<0.01, ** p<0.05, *p<0.1.